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Ffor hym was lefere to have at hese bedis hed
Twenty bokis I-clad in blak or red
Of Aristotle & his philoso-phie
Than robis ryche or fedele or gay sautrie
S. I.
U.S. National Museum
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"*To the solid ground*
Of Nature trusts the mind which builds for aye." WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, MAY 3, 1877

THE UNIVERSITIES BILL

VERY little light has been thrown on the future of Oxford and Cambridge by the discussions in the House of Commons last week and this week. Harassed as they are by the difficulties of the Eastern question, our legislators could not, perhaps, be expected to devote serious thought to the fortunes of higher education in England, but as Mr. Knatchbull-Hugessen said, the debate was "certainly more suited to the debating societies of Oxford and Cambridge than to the arena of the House of Commons." Lord F. Hervey, who opened it, had little weightier to remark than that Mr. Grant Duff was "no doubt a very learned and superior person," and Mr. Grant Duff's chief contribution was the venerable witticism that Lord F. Hervey ought to be carried round the country by himself and other advanced reformers, as "the shocking example" of the results of the present system. Mr. Trevelyan delighted the House and the country by the amusing patriotic statement that "It would not be possible to find in any European University forty mathematicians equal to the Wranglers in the Cambridge Mathematical Tripos, or twenty classical scholars to compare with those who stood first in the Classical Tripos at Cambridge or in the School of *Literæ Humaniores* at Oxford;" and with an equally cheerful indifference to the facts Mr. Lowe replied that the teaching of the Universities was "simply disgraceful." When we add that there was much dispute whether the glories of Lord Macaulay, who was not "a resident fellow of Trinity College, Cambridge," should be credited to that University, and that Sir William Harcourt was delighted to hear that an overworked judge like Sir Alexander Cockburn could find time, in spite of his work, to undertake the arduous office of chairman, which would require the "constant and daily attention of one having entire and absolute leisure," we sum up most of what was interesting and novel in the discussion before going into Committee.

On two questions, however, which are of speculative importance, much incidental light was thrown. The Bill gives large enabling powers to the Commissioners, and although, as "An Oxford Man" pointed out in NATURE (vol. xv., p. 391), it is very doubtful whether large reforms can be effected even on the initiative of a strong-willed and clear-headed Commission; it is perfectly certain that little or nothing can be done if the Commissioners are feeble and without origination. In their preparation of schemes for each of the forty colleges they are to be associated with three residents from the college itself, and it is only from the colleges that they can get money to effect any reforms. It is important therefore to understand what are the views of the Government with regard to the reforms which are practical or possible, because it is to advance these views that the Commissions have been selected. Afterwards, everything depends on the Commissioners themselves, on the spirit in which they have undertaken their task, and on the diligence, ability, and discretion with which they are likely to execute it.

The Opposition offered no formal objection to any of the names proposed by the Government, but they suggested the addition of three names to the Oxford and of two names to the Cambridge list. For Oxford they proposed Prof. Bartholomew Price, Prof. Huxley, and Prof. Max Müller, and for Cambridge Dr. Bateson, the master of St. John's College, and Dr. Hooker, the president of the Royal Society. The addition of these names would have greatly strengthened the Commissions, and those who are anxious to see the Universities question treated in a generous spirit and with a wide knowledge of the subject might have been reasonably hopeful of good results. To our mind it is a fatal objection to the Commissioners as they stand that they include no members who are not alumni of the Universities on which they are to sit. Prof. Huxley and Dr. Hooker would have been of the utmost service, because they would have approached University questions from the point of view of men whose lives have been spent outside the Universities. It is certainly important for the Commissioners to have a practical acquaintance with working details, but that would surely have been sufficiently guaranteed by the presence of nine University men on each Commission even though one outsider had been added to each. Prof. Huxley has sat on the Scotch Universities Commission, and has had the largest experience of teaching at unrestricted institutions like the School of Mines and South Kensington. Dr.

Hooker and he would have strengthened the hands of those who wish to see science represented in our higher education, and the addition of their names would certainly not have upset the balance of the Commission, as Prof. Price, Dr. Bateson, and Prof. Max Müller, would have adequately maintained the interests of the older studies. Surely, when we are setting about the reform of our universities, we want all available information about the systems of foreign countries, and Prof. Max Müller could have told the Commissioners many things they have not learnt from their own experience of Oxford and Cambridge. The refusal of the Government to add any names to the original list was an unfortunate sign of the spirit in which they have framed it, and of the attitude in which the Commissioners will face the problems before them. Ministers were successful, but their majorities were so small as to show that the sense of the few members who will not consent as partisans to vote on such questions was decidedly against them. They carried their point by 11, 34, 24, 26, 32 votes on the successive divisions. After their success nobody will care much what becomes of a bill which is meant to change as little as possible, when every crevice that could let in light from the outside world is carefully stopped against it.

The speech of the Secretary of War, who is Member for the University of Oxford, and the general tone of the debate, clearly confirm these anticipations. There will be a slight restriction of the "prize fellowships," the new Government name for the "idle fellowships" of Lord Salisbury. There will not be a great extension of the professoriate, provision even having been made for the amalgamation of several professorships into one. Some money—5 per cent. from some colleges, 10 per cent. from others, nothing perhaps from a third class—will be taken from the colleges for university purposes. There is a provision "for the extension, not for the suppression," of scholarships. Only the superfluities of the colleges are to go to the University, and Mr. Hardy has never had but one opinion on "what some people called the endowment of research." He did not state that opinion so frankly as Mr. Trevelyan, but there was little doubt from the tone of his remarks that it was substantially the same :—

> "It was a mistake, therefore, to assume that we could create in men such qualities by merely endowing old men, and in his opinion it would be better to throw the funds of the Universities into the sea rather than to bestow them in the manner which had been proposed. The people whose prayers the House should listen to were the practical teachers of the University, who were bound to celibacy, and who asked them to make their career a better one, to give them a reasonable income, and to allow them to marry without being compelled to resign their positions. These gentlemen would have six months in the year, which they would be able to devote to the pursuit of science and literature. What they had to do was to find men for the places, and not places for the men. He begged them to consider well before they created a sort of hierarchy of sinecures and semi-sinecures which unless human nature was radically altered by this Bill would only lead to academical jobbery and intellectual stagnation."

No doubt the wholesale conversion of the fellowships of residents and, for that matter, of non-residents into professorships, created in a doctrinaire spirit, and apart from the gradual development of literature and science, would be recklessness and folly. Nobody in his senses wants such a thing. The real note of despair in the whole debate is that Oxford and Cambridge wish to be let alone, and Oxford and Cambridge men in the House are determined that they shall be let alone to consider every question as it comes up from the mere local point of view of Oxford and Cambridge. The jealous exclusion of outsiders is the surest proof of the intention of the framers of the bill and the clearest prophecy of its issues.

The Committee made no real alterations in the bill. There was a desperate attempt to maim it by striking out even the possibility of endowments for research. It was resisted and defeated by an overwhelming majority. Mr. Hardy said "the noble lord and the hon. gentleman seemed to be under the apprehension that if research were brought into the University education would be driven out. On the contrary, he held that no teaching could be successful that was not founded on the most minute research. There were, no doubt, many subjects of research which by their nature were not lucrative to those who prosecuted them but the prosecution of which was of great importance to education throughout the country, and especially to the University in which they were carried on. There was, however, no intention to carry research to the extravagant lengths which some speakers and writers feared would be the case, and which would utterly pervert the purposes of the University. So far from diminishing the educational power of the University, that which was proposed would give to education a more solid basis than it now possessed." Mr. Trevelyan accepted Mr. Hardy's statement as "in all respects satisfactory," and added a remark none the less valuable that it is almost a truism, "They could not have a University where education was proceeding without research proceeding at the same time." The Commissions will thus be left at liberty to use the funds they can detach from the Colleges for the endowment of research. But "Researchers," as Prof. Sylvester calls them, will not for many years to come, grow very fat on the good things of Oxford and Cambridge.

DEEP WELL-BORINGS IN LONDON

THE constantly increasing wants of our English metropolis were very amply provided for during all the earlier stages of its history by the stores of water contained in the extensive beds of gravel lying within the Thames Valley. These stores of water could be reached by means of shallow wells, and all the ancient and famous pumps of our city drew their supplies from this source.

But, as the population of the district increased, the value of this source of water-supply became greatly impaired from two causes ; firstly, the excessive drain upon it, caused by the rapid multiplication of wells ; and secondly, the pollution of its waters by the refuse-matter of a great city.

Hence it became necessary to seek for new sources of water-supply, and the success which had already attended the construction of Artesian wells in the Tertiary districts of Northern France, led to attempts being made to obtain supplies in a similar manner by putting down borings through the impervious London Clay into the water-bearing beds of the Lower London Tertiaries.

For a time the quantity of water thus obtained, as at Merton, Garrett, and many other points, seem to have induced the belief that an inexhaustible source of the all-essential element had been discovered; but the rapid multiplication of these Artesian wells soon revealed the fact that the new and valuable stores had their limit, and that this limit was being very rapidly approached in consequence of the excessive demands which were now being made upon the new source of supply. The deepening of the wells, by which means water was drawn from the Chalk, as well as from the Tertiary strata, promised, however, to do something towards staving off the evil day when London would no longer be able to depend on drafts being honoured by her great subterranean bank.

Such was the state of the question when Mr. Prestwich, now the Professor of Geology in the University of Oxford, undertook its complete investigation as an important geological problem. No one more competent for the task could possibly have been found, for during many years Mr. Prestwich's studies had been devoted to the Tertiary deposits of the London and Hampshire basins; and his great work—"A Geological Inquiry respecting the Water-bearing Strata of the country around London, with Reference especially to the Water-supply of the Metropolis," which was published in 1851—is a masterpiece of minute observation and close and accurate reasoning.

More than this, the geologist points to the work with pardonable pride, as affording convincing proof that his science has now acquired a character for exactness, analogous to that which is justly regarded as the crowning attribute of astronomy. After a most elaborate study of the nature and relations of the various strata which crop out all round the London Basin and of the disturbances to which they have been subjected since their deposition, Mr. Prestwich ventured on a bold *prediction*, namely, that the Chalk beneath London would be found to have a thickness of 650 feet, the Upper Greensand of 40 feet, and the Gault of 150 feet. (*Op. cit.* p. 142.)

At the time when this announcement was made no well in London had been sunk to a greater depth than 300 feet in the Chalk, but now we can appeal to no less than four deep borings in the metropolis, which afford the most convincing proof of the reliability of the data, and the accuracy of the reasoning by which Mr. Prestwich arrived at his interesting results. For the sake of distinctness, we place the estimated and determined results side by side in a tabular form:—

Mr. Prestwich's Estimate.		Boring at Kentish Town.	Boring at Crossness.	Boring at Loughton.	Boring at Meux's Brewery.
Chalk	650	645	646	650	653
Upper Greensand	40	$13\frac{1}{2}$	12	40	28
Gault	150	$130\frac{1}{2}$	148	(?)	159

When it is remembered that the Chalk graduates downwards insensibly into the Upper Greensand, and that it is almost impossible to decide on their line of separation in the cores brought up by boring operations, it will be admitted on all hands that the agreement between the estimated and proved results is marvellously close.

One of the most important conclusions of Mr. Prestwich's work was that the strata below the Gault, the so-called "Lower Greensand," would in the future afford a most valuable underground source of water-supply to our overgrown city.

But in 1855 Mr. Godwin-Austen brought before the Geological Society of London his masterly essay "On the Possible Extension of the Coal-Measures beneath the South-Eastern Part of England," in which he announced the conclusion—based on a most elaborate study of the geological structure of the South of England and the adjoining portions of the Continent of Europe—that an old ridge of Palæozoic rocks underlies the line of the Thames Valley, and is only concealed from us by the Upper Cretaceous strata.

Mr. Godwin-Austen's announcement was as strikingly verified as was that of Mr. Prestwich; for, in the same year that it was made, a boring at Kentish Town which passed through the Gault, reached a curious series of red rocks which are now believed by geologists to be either a portion of the old Palæozoic ridge itself, or a set of littoral deposits formed upon its flanks. And in 1857 the deep boring at Harwich afforded still more unmistakable evidence of the existence of this old Palæozoic ridge in the fact that black slaty rocks were found immediately below the Gault clay.

Although the old ridge of Palæozoic rocks must thus limit the area of the available water-bearing "Lower Greensand" beneath the metropolitan district, yet Prof. Prestwich has constantly argued that very large and valuable supplies of water will yet in all probability be obtained from the latter source.

Hence it is that the endeavour to tap this great subterranean reservoir, which is now being carried out in such an enterprising spirit by the Messrs. Meux and Co., in the Tottenham Court Road, is attracting so much attention from geologists and engineers. The nodular beds at the base of the Gault were reached at a depth of 999 feet from the surface, and some sixty feet of rock below has since been penetrated. The splendid cores brought up by the diamond-borer are at once submitted to Mr. Robert Etheridge, the palæontologist of the Geological Survey, who is carefully studying every trace of fossils which they exhibit. At present there are very strong grounds for believing that the "Lower Greensand" has been reached, and we soon hope to be able to announce that the new source of water supply, so long ago pointed out by Prof. Prestwich, has at last been made available for the ever-increasing necessities of this great city.

J. W. JUDD

LATHAM'S ENGLISH DICTIONARY

A Dictionary of the English Language. Abridged by the Editor from that of Dr. Samuel Johnson, as Edited by Robert Gordon Latham, M.A., M.D., &c. (London: Longmans and Co., 1876.)

WE consider ourselves justified in reviewing an English dictionary in these pages for two reasons; first, because the method of its construction ought to be rigidly scientific, and second, because a large proportion of the words in any modern English dictionary must necessarily be scientific terms.

It is admitted by all competent to pronounce an opinion that there is ample room for a new dictionary of the

English language; that when Dr. Latham undertook the task for which his attainments so well fit him, he had an excellent opportunity for doing a splendid service to our tongue and making for himself a lasting name. The only dictionaries that make any pretence to exhaustivenesss, Webster's, Worcester's, and the Imperial, with all their merits, come far short of what an ideal national dictionary should be, and they cannot for one moment be compared with Littré's *magnum opus*. Webster's etymology is extremely unsatisfactory and misleading in its method, the vocabulary is a conglomeration on no principle, and the definitions are too frequently unmethodical. We consider Worcester in some respects more satisfactory, more scientific in its method than Webster. The Imperial is rather a small encyclopædia than a dictionary, minute description frequently giving place to definition, and the vocabulary being much fuller than that of any existing dictionary. This feature, however, seems rather to be the result of a desire to crowd in as many words as possible than of any well-considered scientific plan. The etymology of the Imperial might almost have been written a century ago. Thus Dr. Latham had a splendid field before him, and Littré has shown what one man is capable of doing in the way of dictionary-making. We need not for the hundredth time contrast his work with the endless pottering of the French Academy. Perhaps it scarcely needs to be proved that in the construction of a dictionary, as in most other great undertakings, failure will surely be the result unless one competent man has the supreme command.

The work before us is an abridgment of Dr. Latham's larger work in four quarto volumes. The abridgment has been made mainly by the omission of the illustrative quotations which form so large a feature in the larger work, and of certain disquisitions on extremely minute points which occur during the progress of the work. Many will be of opinion that the omission of the latter is distinctly beneficial; they are too frequently little else than laborious trifling. The omission of the quotations is, no doubt, a disadvantage; they bear the same relation to and throw the same light on the definition that specimens do in the case of geology and experiments in other sciences. A very few have been retained, and it would have been an advantage had there been many more, as there might easily have been had the various meanings under each word been run on instead of being paragraphed.

Dr. Latham calls his dictionary a new edition of Johnson; if it were only this it would be at once a confession that the work was an anachronism. To bring the heroic old compiler's work up to date would require quite as much labour as Johnson bestowed on the original; and as Dr. Latham's work has so much that is new in all departments, we must regard its title as mainly an act of courtesy to the memory of "the great exicographer." As the abridgment contains all the vocabulary of the larger work, the two in this respect may be regarded as identical, and from its size and price, the larger work is evidently meant to be a practically complete English dictionary.

Dr. Latham's vocabulary is of course much more extensive than that of Johnson. He has read largely in modern works in all departments of literature and science, and thus been able to register many words that did not exist in Johnson's time, as well as many new meanings that have been given to old words. The consideration of vocabulary is probably the most serious that comes before any one who sets himself to the laborious task of compiling a dictionary. His duty is certainly to set down all words used by reputable writers. But is this all? How far back is an English dictionary-maker to go? to Spencer or to Chaucer? Mr. Freeman might possibly say to "Beowulf." Who are to be considered "reputable" writers? Should only "reputable" writers be taken into account? And should no word that has not been printed in a regular way be admitted? How far should slang terms and provincialisms, including Scotticisms (*pace* Prof. Blackie) be admitted? Again, what is to be considered literature? Must all science be excluded, and the vocabulary be confined to such words as occur in poetry, *belles lettres*, history, philosophy? These and many other questions must be settled at the very outset by the compiler of a dictionary making any pretence to completeness, and we are glad to see that, to a considerable extent, Dr. Latham has settled them on the liberal side. His aim has apparently been to make a work that would be useful to people of wide culture and general reading, and he has interpreted the English language to be the language used by the people of England in expressing their thoughts on the varied subjects that engage their attention.

We are at a loss to discover the principle, however, on which Dr. Latham has compiled his vocabulary. He has certainly inserted a large selection of scientific terms, but the selection appears to us to have been made in a capricious and arbitrary manner. He has, for example, given many of the technical names of the divisions and subdivisions of the animal and vegetable kingdoms, but it is not easy to see by what clue he has been guided. Why should Raptores and Natatores find a place while Scansores, Insessores, and all the other avian orders are omitted? Is it that the two former have been detected by Dr. Latham in some "literary" writer, while he has failed to come across the latter? Even Amphibia and Amphibian find no place, nor the adjective Avian. We find Infusoria and Cetacea, and Monotremata, but no Rodentia nor Carnivora, nor a host of other names even more likely than those capriciously registered to be inquired for by readers of works of popular zoology. It is a very nice question whether this class of words should be admitted at all into an English dictionary, but if it be decided affirmatively the only satisfactory scientific method is to admit all. A generic name (*e.g.*, Dionæa) in this respect is quite as important as that of the largest subdivision in zoology or botany.

The defects of the dictionary are equally apparent in other scientific departments. We find Oolitic and Triassic and Drift, the last in some detail, but not Laurentian nor Cambrian, nor such a common word as Pothole. Biogenesis, Abiogenesis, Heterogenesis, and Bacteria are conspicuous by their absence; as are also Eozoon, Atoll, Globigerina, Hipparion, and Amphioxus: Lepidosiren is given in some detail. To Basin no geological meaning is assigned. Palæozoic (with a bare reference of Cænozoic and Mezoic) is found, but not Azoic; Permian, but not Devonian, Silurian, or Purbeck; Labyrinthodont but not Pycnodont. We have Protoplasma but neither Protoplasm nor Protoplasmic. Photosphere we find, but not Chromosphere, nor Corona in its solar application, and neither Heliostat nor Siderostat. The dictionary contains various terms in electricity and magnetism, but not Magneto-Electric, Electro-Biology, Quantivalence, Anode, nor Cathode. Darwinian and Darwinism, long since used as current common terms, find no place here; and no one would guess from the definitions of Evolution and Development the immense significance which these terms have assumed in recent times.

We could give many instances of similar caprice in the admission of scientific terms, but our space does not admit of it. But it is not alone in this class of terms that the vocabulary appears to us to be defective; many words are wanting which, we venture to think, any man of common sense would look for in a modern English dictionary of the pretensions of that edited by Dr. Latham. Under Mule a reference is made to the spinning-jenny, but under neither Spinning nor Jenny is the use of the term explained. Readers of Arctic narratives will look in vain for an explanation of Ice-foot and Ice-master, and the reader will not be surprised at the omission of Snider, Whitworth, and Mitrailleuse. Can any sound reason be given for omitting such a word as Croquet? And where are we to look for an explanation of such national terms as Over and Bye, if not in the most recent of English dictionaries, which registers the "cricketal" signification of Stump? The work is evidently not meant for circulation in America, if we may judge from the absence of all Americanisms, even those which have become current coin in the English tongue, such as Bunkum, Caucus, Mocassin. Might not such words as Ecchymosis and Deoppilation have been spared (who is likely to look for them?) in favour of some or all of the terms referred to. Many words found in Tennyson, Morris, and Swinburne are marked as "obsolete," showing the danger of using the epithet at all.

The etymology seems to us unsatisfactory. To words whose origin is simple and obvious two or three lines are sometimes devoted; while of others whose etymology is certain enough, but which it would have taken some time and trouble to trace, no satisfactory information is given. What satisfaction is it to be told simply that Abandon comes from French *abandonner*, especially when the history of the word can be so beautifully traced? There is a like want of proportion in the definitions, which are in most cases extremely meagre, but in some cases capriciously and unnecessarily diffuse. In the arrangement of the various definitions under each word, moreover, we fail to discover, as a rule, any logical or historical method. In this as in some other respects Dr. Latham has stuck too closely to the old lines of dictionary construction, and missed the opportunity of compiling a work which might have cast all other similar works into the shade. We cannot say that it has dethroned either Webster or Worcester, unsatisfactory in many respects as these are; and there are two or three smaller and cheaper dictionaries, which we venture to think would be more useful to the general reader. The field is still unoccupied, for Dr. Latham's work can never, in our opinion, serve as the standard dictionary of our language. The work is handsome and well printed, and the "Historical Sketch of the English Language" is thoroughly satisfactory.

GUILLEMIN'S "WORLD OF COMETS"

The World of Comets. By Amédée Guillemin. Translated and Edited by James Glaisher, F.R.S. (London: Sampson Low and Co., 1877.)

MR. GLAISHER mentions that he was anxious that M. Guillemin's interesting work upon comets should appear in our language, from the fact of there not being so far any volume that occupied the ground covered by it, while, it may be added, that the recent important advances in this branch of the science renders a pretty complete summary of progress in late years a most desirable help and guide to the student, scattered as the reports of such progress almost necessarily are in the publications of scientific societies and in periodical scientific works at home and abroad.

The greater portion of the volume before us relates to those particular departments of the subject which may be expected to interest the general reader. The historical portion, especially in the earlier ages, when comets were regarded as omens, good or bad, to the time when Newton developed the laws by which their motions are governed, naturally commences the work; then follow chapters upon their orbits, the periodical comets from the short revolution of Encke's comet, to the revolutions of several thousands of years which have been assigned with a greater or less degree of probability to other of these bodies; more particular descriptions of several great comets in recent times, as the comets of 1744, 1811, 1843, 1858, 1861, and the great comet of Coggia in 1874, which made its appearance just prior to the publication of M. Guillemin's treatise. It is, however, in what we must term cometary physics that the volume is most complete, and in which its interest and probable usefulness will mainly consist. The theories of Olbers, Bessel, Faye, Roche, Tyndall, Tait, and others are noticed in a popular and readable style, and are fairly considered collectively, though differences of opinion must still prevail with regard to any inferences to be drawn from them. The researches of Dr. Huggins, Prof. Secchi, MM. Wolf and Rayet, in the spectral analysis of the light of comets, and particularly of Coggia's Comet of 1874, are described, and to these results, as collected by M. Guillemin, Mr. Glaisher has added an important article by Mr. Lockyer, which appeared while the great comet of 1874 was still visible, and in which are detailed the results of spectroscopic examination of the light of the comet with the aid of Mr. Newall's great refractor. The editor has also made some very desirable additions to M. Guillemin's chapter on "The Common Origin of Shooting Stars and Comets."

The work concludes with a list of elliptic comets and their elements and with a general catalogue of cometary orbits to 1876.

We have said that probably the chief interest and value of M. Guillemin's "World of Comets" will be found to consist in the extensive portion of his volume devoted to cometary physics, to the theories which have been advanced to explain their varied aspect, and the formation of the enormous trains by which some comets are accompanied. The comets of short period—a most interesting class—might well have been treated in somewhat greater detail, and in this division of the work we note several oversights. Thus it is stated that the researches of Dr. Axel Möller upon the motion of Faye's Comet, show that that body supports the theory of a resisting medium, first supposed to be indicated by Encke's investigations relating to the comet which bears his name; but as long since as the year 1865 Dr. Axel Möller had relinquished this idea, and from a rigorous discussion of the observations at the first three appearances, alluded to by M. Guillemin, had succeeded in representing the observations by the simple application of the planetary perturbations, without any hypothesis whatever, and his later researches have also negatived the existence of any trace of the effect of a resisting medium upon the motion of this comet. There is some ambiguity in the definition of the element π, or the longitude of the perihelion in the orbit of a comet; from the explanation given by M. Guillemin it might rather be inferred that the longitude is reduced to the ecliptic, which is not the case. The comet discovered by De Vico at Rome, February 20, 1846, is duplicated, appearing first on p. 140 with a revolution of fifty-five years, and again on p. 143 with a period of seventy-three years; the former period resulted from one of the earlier calculations. Pigott's comet of 1783 is named amongst the contents of a chapter p. 133, but there is no further reference to it. In the catalogue of orbits, there are several cases since the year 1866 where the inclination has been reckoned over 90°, as is frequently the case amongst the German computers, and with the unnecessary addition of the letter R in the column headed "direction of motion." To render these orbits consistent with the method hitherto in general use, and indeed adopted exclusively in the preceding part of the catalogue, the inclination given requires to be subtracted from 180°, and for the longitude of perihelion given in the fourth column, $2\,\text{☊} - \pi$, should be substituted.

These, however, are small defects which may easily be avoided in a future edition. As a whole, M. Guillemin's "World of Comets" must prove a welcome aid to the student on entering upon this branch of astronomy.

J. R. HIND

OUR BOOK SHELF

Fownes's Inorganic Chemistry. Edited by Henry Watts, B.A., F.R.S. Twelfth Edition. (London: Churchills.)

IN the present edition of this well-known manual the publishers have, wisely as we think, determined to divide it into two parts. In its old form the work had grown to be as unhandsome and cumbersome a volume as could be well imagined; like an overgrown yeast-cell it was obviously getting too big to hold together much longer, and many a student on his way to and from the lecture-room must have wondered, as he struggled to get the thick squat book into a comfortable carrying position, why the process of gemmation was so long delayed.

The present volume, which treats of physical and inorganic chemistry, contains a considerable amount of new matter, and may be regarded as an accurate representation of the present state of knowledge on these subjects. Among the more important additions we may mention an account of Mendeleeff's Laws of Periodicity, and a very good digest of what is known concerning the new metal gallium and its compounds; this element is associated with indium, with the probable atomic weight 68, as already indicated by M. Mendeleeff. The position of the cerite metals is also determined in accordance with the specific-heat estimations recently made by Hillebrand. On the other hand, it may be doubted if iodine tetrachloride has any real existence, and Michaelis has proved that the reaction $3PbSO_4 + 2POCl_3 = 3SO_2Cl_2 + Pb_3P_2O_8$ is not realised in practice. On the whole, however, the work fully maintains its reputation as a faithful exponent of the state of contemporary chemical knowledge. T.

The Microscopist: a Manual of Microscopy and Compendium of the Microscopic Sciences. Third edition. By J. H. Wythe, A.M., M.D. (London: Churchill, 1877.)

IT is now some twenty-five years since the first edition of this work appeared, and as the author himself remarks in his Preface, it is no small compliment to a work of this kind that for so many years it should hold a place among works of reference, although surrounded by larger and more pretentious volumes. For this third edition the book has been entirely rewritten, the advancement of microscopical science having naturally rendered considerable enlargements necessary. Still the work retains its principal qualities as before, viz., the precise and clear language, the absence of all unnecessary verbiage, and last but not least, the excellent arrangement of the contents. Thus after a brief reference to the history and importance of microscopy, we have able descriptions of the microscope itself and its accessories, followed by general remarks on its use and the more modern methods of microscopic investigation. Then, after a short chapter on the mounting and preserving of objects, we come to well-written and richly illustrated treatises on the application of the instrument in the different sciences, each science being spoken of in turn and in a separate chapter. For the beginner this arrangement is of special value, as it enables him quickly to form a general idea of the whole domain of microscopy. Mineralogy and Geology are followed by a chapter on Microscopic Chemistry; then the author treats of Microscopic Biology, devoting a chapter to Vegetable Histology and Botany, one to Zoology, the next to Animal Histology, and the last to Practical Medicine and Pathology.

The illustrations are original to a great extent; many also are taken from the works of Carpenter, Frey, Stricker, Billroth, and Rindfleisch. The larger plates, of which there are twenty-seven, are particularly well drawn, and add greatly to the general excellence of the work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Hog-Wallows and Prairie Mounds

JUDGING from the descriptions of these deposits, they must be nearly, if not quite, identical with those which I described in a paper on "The Ancient Glaciers North and East of Llangollen," read at the British Association, 1865. These are a series of heaps of glacial drift covering more or less completely the habitat of Cheshire Cheese, *i.e.* the Vale Royal itself, and the slopes which extend from it to those Welsh Mountains that are so prominently seen from Chester. These mounds vary in size and shape according to their position. They are very well-defined and numerous in the valley of the Alyn, between Wrexham and Mold, where they have the form of oblong hog-back mounds usually lying parallel to each other with their longer axes (if I may use the term) nearly at right angles to the general slope of the surface. They may be counted by hundreds, and in some parts are so near together as to form a series of connected undulations. They are largest and most abundant opposite the mouths of the lateral valleys opening into the main valley of the

Alyn. Their origin is well indicated in these positions, by the manner in which they lie opposite the mouths of the valleys at right angles to the course of the present streams.

The most remarkable of all these is a long ridge running parallel to the Great Western Railway near Gresford. It is marked and shaded on the ordnance geological map. Bailey Hill, Mold, is another. This is attributed to the Danes—described in the guide books as a Danish fortification. I have proved the glacial origin of these mounds by finding in them striated subangular boulders, that have travelled considerable distances; such, for example, as large blocks of the Llanarmon limestone, and rounded lumps of curly cannel, that must have crossed the ridge of the Hope Mountain, the height of which varies from 300 to 800 feet above the Leeswood and Tryddn valleys from which the coal must have been carried. On one occasion, during the construction of the Wrexham Mold and Connah's Quay Railway, I saw a large fire blazing in a navvy's shed, and upon examination found that the fuel was curly cannel they had found in making a cutting. They described this find as two pieces, each one "bigger than a man's head." I brought away an unburnt fragment of about 2 lbs. weight. It was a subangular corner, smoothed and faintly striated. The nearest cannel seam to this place—which is over the millstone grit—is about four miles, with the Hope Mountain intervening.

A curious example of the unexpected bearings of scientific investigations upon commercial interest was presented by these cannel boulders. Two or three years before I commenced the study of the ancient glaciation of this district, Mr. W. C. Hussey Jones had proved the value of this curly cannel as a source of paraffin, and what are called paraffin oils, &c. Great excitement resulted, and a great rush was made to "*the Flintshire oildorado.*" This curly cannel was sold at prices varying from twenty-five shillings to thirty shillings per ton at the pit's mouth, while the price of ordinary main coal was only six shillings. The owners of this cannel, or holders of leases or "tak notes," giving a licence to work it, made large sums of money (as much as 80,000*l.* was paid for the transfer of one lease), and consequently great search was made for new seams. Among the searchers were the farmers, landowners, and outside speculators, who commenced boring and sinking and forming companies for cannel mining in the region covered by these "hog wallows;" the evidence upon which their expectations were based being the discovery of pieces of cannel on or near the surface, turned up by the plough or otherwise. Many thousands of pounds were thus wasted. One very worthy man, that I knew very well—a hard-working Welsh farmer—spent the savings of a whole life-time in searching for cannel on his farm, where he had frequently turned up fragments in ploughing. His death speedily followed his ruin. There were many other similar cases. Had I commenced my investigations three years sooner I might have explained the strange and apparently incomprehensible anomaly of Leeswood cannel being found on the south side of the Chester and Mold Railway, and in the neighbourhood of Caergwrle, in spite of an intervening ridge of mountain.

One very curious and instructive feature of these mounds is their change of shape as we proceed from the hill slopes towards the great plain known as the Vale Royal, which was formerly a great estuary or fjord of the Dee. Instead of the long and rather steep hogback ridges we now find a general outspreading deposit dotted here and there rather sparsely with obtuse conical mounds, so obtuse and so much disturbed by agricultural operations that they can only be detected by careful observation.

My explanation of these differences is that the glacier which planed the millstone grit of the Hope Mountain by sweeping over and around it, originally spread out upon the waters of the estuary now forming the Vale Royal, and thus formed the outspread deposit; that it afterwards receded, and the icebergs that broke off and floated away from it were stranded here and there, thawed, deposited their contents, and thereby formed the mounds; while the oblong ridges mark the final step-by-step recession and oscillations of the dying glacier, which formed them partly as terminal moraines, and partly by ploughing up and thrusting before it, in the course of its advancing oscillations, the previously deposited glacial drift. I throw out these speculations suggestively, to be taken for what they are worth; they fit the facts well enough so far as I have been able to study them, but the main object of this letter is to direct attention to this and other corresponding deposits near at home that appear to me to be worthy of further investigation, especially by residents in the neighbourhood and the members of local field-clubs, &c. The Liverpool Naturalists' Field Club paid a visit to the district while I lived there, and I showed the geological members some of these deposits. W. MATTIEU WILLIAMS

Belmont, Twickenham, April 24

IT is apparent from Prof. Le Conte's description of the prairie mounds (NATURE, vol. xv. p. 530) that the drift mounds figured and mentioned by me (vol. xv. p. 379) have quite different origins. The prairie mound would seem to be somewhat similar and have the same origin as a tussocky bog or mountain. The formation of a tussocky bog has been described in "Valleys and their Relation to Fissures, &c.," p. 14. A tussocky mountain is similarly formed very hot weather cracks the peaty upper soil forming deep fissures; while subsequent weathering changes the portions between the fissure into small hills. I lately saw on the coast of Wicklow a considerable area of Æolian drift of this hummocky nature; the hillocks being about four feet high. They were so regular as to have the appearance of being moulded from one model. These could not possibly have their origin in fissures; but they seemed to have a connection with bunches of bent, round which the wind collected heaps of sand. But again why should the bunches of this grass grow at regular intervals? In the same neighbourhood some of this Æolian drift is piled in long parallel ridges, about five or six feet high, and having quite an artificial look. These evidently are wind formed; but how it is hard to conjecture, as they run oblique to the prevailing and most effective winds. G. H. KINAHAN

Ovoca, April 24

Greenwich as a Meteorological Observatory

IN Mr. Buchan's objections to the hypothesis that the temperature of Greenwich is raised by the proximity of London one most important consideration has been omitted. Granted that the mean temperature of the summer months, June to September, is 0°·9 higher at Greenwich than at the eight other stations referred to, it does not follow that this alone is the cause of the higher average temperature at the former place. Greenwich occupies a position farther from the Atlantic and nearer the Continent than the majority of the selected stations, and we might therefore expect to find it subject not only to a higher temperature in summer, but also to a lower temperature in winter. If this be so, the excess which Mr. Buchan admits may be accounted for by the raising of the mean winter temperature from artificial causes; and this view of the case seem to be confirmed by observation. The station at Leyton, Essex, supplies the requisite data; for, although near London and rapidly increasing in population, it is, or rather was, in a country district when the observations were made. It is situated on the verge of Epping Forest, is separated from London by the Hackney Marshes, is rather more than 6¼ miles in a direct line from St. Paul's Cathedral, from which Greenwich is 4¾ miles distant, and is 7 miles nearly north of the last-mentioned place. The meteorological observations were undertaken with the express intention of comparing them with those at the Royal Observatory, with which object the instruments were mounted on a stand precisely similar to the Greenwich stand, and the exposure was unexceptionable. The comparison relates to the daily maximum and minimum temperatures for the three years ending November, 1863. The average was at Greenwich 50°·4, Leyton 49°·9. Allowing for elevation, the results are:—

	Annual.	Maxima.	Minima.	June to September.	December to March.	Maxima June to September.	Minima June to September.	Maxima December to March.	Minima December to March.
Greenwich ...	51°·0	58°·9	43°·0	61°·0	41°·9	71°·0	51°·1	47°·7	36°·1
Leyton ...	50·2	58·3	42·1	61·3	40·8	72·0	50·7	46·9	34·8
Greenwich warmer than Leyton.	+0·8	+0·6	+0·9	−0·3	+1·1	−1·0	+0·4	+0·8	+1·3

These results prove that Greenwich is warmer than Leyton, which is farther removed from the influence of London, and that during the winter months the temperature is higher both by day and night, but chiefly by night, when the excess is 1°·3; also that in summer, while the nights at Greenwich are warmer than at Leyton, the days are cooler. The inference is that the artificial heat of London sensibly affects Greenwich, mostly by raising the temperature of the air in winter and at night, when it might be expected to do so with the greatest effect; and that the temperature at Greenwich by day in summer is depressed by the smoky atmosphere hindering the transmission of the sun's rays when they are most potent. H. S. EATON

Croydon, April 24

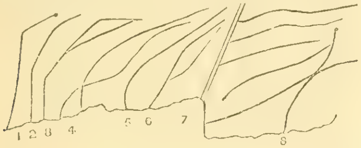
Ancient Characters at Cissbury

TWO years ago some marks were discovered at the entrances of galleries in one of the pits at Cissbury, which appeared to have been scratched on the chalk with a flint instrument. They were suspiciously like figures or masons' marks, but yet had all the tints of age, and resembled more or less early letters.[1] The little woodcuts , represent the forms with sufficient exactness. Other marks found shortly afterwards in a second pit were thought to be merely a trellis pattern. In attempting to cut them out of the rock, the chalk was broken into fragments, but fortunately rubbings had been previously taken.

The doubt as to the genuineness of the above marks was removed by the further discovery, in September, 1875, of a third set (again at the entrance of a gallery) in another pit at Cissbury Camp. They were arranged in two lines, the lower one presenting all the appearance of an inscription. A number of detached blocks, with distinctive marks upon them, were found in this pit amongst the rubble with which it was filled. One of these blocks, which was discovered about five feet below the surface, had four definite marks scratched upon it at even distances.

Up to the time that this third pit was opened no distinctive marks had been met with, except those above-mentioned, though there were many thousand accidental pick-marks on the walls and loose chalk.

I have now the satisfaction of mentioning that upon examining with more care the marks found in the second pit, the diagonal scratches of the trellis prove to be eight branching characters of a peculiar form. The vertical scores which cross them turn out to be later additions, cut with a finer tool than the serrated flint that has left its mark on the more superficial and broader lines of the characters beneath. In the following woodcut the vertical lines are not shown, and the rune-like forms are placed slightly further apart than is actually the case; the characters themselves, however, are facsimiles[2]:—



Tracings and photographs have been submitted to Professors Sayce and Rhys, and also to Dr. Haigh and other palæographists, who all consider the marks to be characters, though unable at present to give an opinion as to their date. But several on the detached blocks found near the surface, it is thought, may possibly be Anglo-Saxon.

Some of your correspondents may perhaps be able to say whether similar forms have been met with elsewhere. Can it be that the branching characters are examples of the symbols alluded to in the traditions of the Bards?

I may mention that Dr. Haigh thinks that the Celts had writing distinct from and earlier than the Oghams; and he has noticed on the stones of a sepulchral chamber at Keryaval, in Brittany, signs very like letters. J. PARK HARRISON

The Rocks of Charnwood Forest

IT has been a matter of regret to geologists that Mr. Plant has not published in some accessible form his stores of knowledge on Charnwood. We cannot tell how far our facts may be new to him, but we believe that we have been able to make considerable corrections in and additions to all contained in Jukes, Ansted, Coleman, or the Survey Memoir.

We are glad to find Mr. Plant supporting us on the intrusive character of the syenites. But the question can hardly be regarded as previously settled. Mr. Coleman leaned to the idea of their priority to the stratified rocks, Prof. Jukes to their being contemporaneous, and Prof. Ansted to their being metamorphic. When in NATURE (vol. xv. p. 97) Prof. Green suggested the first of these views no one adduced any proof to the contrary. Of all that Mr. Plant says we were well aware, but could not regard the evidence as conclusive. Our opinion is founded on the examination of actual contacts between syenite and sedimentary rock, a thing which so far as we know has not previously been described.

We are well acquainted with the very curious "altar stones" which are doubtless of volcanic origin, but these and the rocks of Bardon no more prove the Markfield syenite to be intrusive, than the ashes and breccias of the Borrowdale series prove the intrusive character of the Wastwater granite. Further, we cannot admit any connection between the Bardon "greenstones" and the Markfield syenite. T. G. BONNEY

St. John's College, Cambridge, April 28 E. HILL

Yellow Crocuses

SEVERAL years ago I observed that snowdrops which I had introduced into my garden were destroyed by poultry getting in among them at the hungry season when these are in blossom. I recollect placing a bantam cock in the garden, and observed that he pecked hastily at a few of the blossoms, and then left off. I then tore up pieces of writing-paper and spread them over the newly turned-up soil. These were hastily visited and as hastily dropped by a few of the poultry. Next I procured some Indian corn, and scattered it among the poultry for the first time. A few hens tried to swallow a grain here and there, but left the most of them. It required two or three days' experience to get them to feed on the Indian corn, and a very short time taught them to exclude snowdrop blossoms from their bill of fare. May not the case of the crocuses mentioned by Mr. Renshaw be explained as similar to that of schoolboys, who eagerly try a bright unknown berry and soon leave off when it is unpalatable? At least so I explained the fact of my snowdrops being more fiercely attacked on their first appearance in the garden than ever they have been since.

Our glen in a few weeks will be made beautiful by the blossoms of the bird-cherry, which grows plentifully on the margin of the streams and the waysides, attaining much larger dimensions than those given by Mr. Bentham in his "Handbook of British Flora," many of the trees being twenty feet high. The caterpillar of the pale spotted ermine moth feeds so eagerly on its leaves that I have, in some summers, seen the trees reduced to ugly skeletons by the middle or end of July. In autumn the beautiful red berries of the Guelder-rose adorn our thickets, but if "fruit has become beautiful so as to point it out to birds for the dissemination of the seed," we do not seem to have the birds which care for these berries, as only three weeks ago I pulled some fine clusters from a bush growing in a sheltered nook.

Tynron, Dumfriesshire, April 21 JAMES SHAW

INCLOSED is a letter that I had from my friend, Dr. Grierson, Dumfriesshire, a month ago, complaining of a pair of ducks that had gobbled up almost every one of his yellow crocuses, and only the yellow ones. I am further informed by Mr. John Young, Hunterian Museum, Glasgow, that the habit of the sparrows taking the yellow crocuses without touching the blue or striped has been long known to him. DAVID ROBERTSON

The Ship-Worm

Teredo navalis certainly is able to endure a long continuance of fresh water. At the town of Brisbane (Queensland), piles, &c., are sheathed with "Muntz metal" to prevent its attacks. The river is subject to long-continued freshes. I remember one which lasted at least ten days, and during that time ocean-going steamers could not ascend to the town, the flood was so powerful. Brisbane is situated far below the *extreme* salt-water flood, but whenever there is a fresh in the river, of even small amount, the water at that town is (according to my recollection) rather more fresh than salt at the end of each ebb tide.

I never saw *Teredo* there, but I took its existence for granted, from the fact that piles, &c., were protected with metal, and the caution given me by the builder of my boat against keeping her in the water when not in use. Brisbane is about twenty-five miles from the full influence of the Pacific, and, to the best of my recollection, the salt water is carried (on the flood) at least thirty miles up the river above the town, when there is no fresh coming down. So far does the salt water indeed extend, that at a time of severe drought (1865–66, I think) it was proposed to bring fresh water for the supply of the town from the principal affluent, the Bremmer, which joins the Brisbane about forty-two miles above the town, as it could not be obtained nearer on account of the high range of the salt flood. It was to have been brought in huge floating tanks towed by a steamer.

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[1] *Journ. Anthropol. Inst.*, No. 18, p. 265.

[2] The last character, on the right of the inscription, has been corrected by lengthening the upper stroke. In the rubbing it was accidentally detached from the cross-lines.

ARTHUR NICOLS

PROF. TYNDALL ON THE SPREAD OF DISEASE

PROF. TYNDALL occupied the chair on Saturday night at the concluding lecture of Dr. Corfield's course on the laws of health. The subject of the lecture was "Infectious Diseases." In proposing a vote of thanks, Prof. Tyndall paid a high compliment to the lecturer for the thoroughly sound instruction which he had so clearly conveyed. He had made it plain that contagion consisted, not of gas or vapour, but of definite particles sometimes floating in gas, in the air we breathed, or in the water we drank; and that, like organic seeds in the soil, they multiplied themselves indefinitely in suitable media, the great probability being that these disease-producing particles were living things. A close study of the subject, extending now over several years, enabled him to agree entirely with the lecturer in the parallelism which he had declared to exist between the phenomena of contagious disease and the phenomena of ordinary putrefaction. The case of flies, for example, to which the lecturer ascribed the power of communicating disease from one person to another, was exactly paralleled by phenomena in putrefaction. Chop up a beefsteak, steep it in water, raise the temperature a little above the temperature of the blood, pour off the water, and filter it; you get a perfectly clear liquid; but that liquid placed in a bottle and exposed to the air soon begins to get turbid, and that turbid liquid, under the microscope, is found to be swarming with living organisms. By suitably heating this perfectly clear beef tea, it can be sterilised, everything being killed which is capable of generating those little organisms which produce the turbidity; and by keeping it from coming in contact with the floating particles of the air, it might be preserved transparent for years. He had now some sterilised beef-tea of this sort, which had been preserved for eighteen months in a state of perfect transparency. But if a fly dipped its foot into an adjacent vessel containing some of the turbid fluid, and then into the transparent fluid, that contact would be sufficient to infect the sterilised infusion. In forty-eight hours the clear liquid would be swarming with these living organisms. The quantity of the turbid liquid which attaches itself to the finest needle-point suffices to infect any amount of the infusion just as the vaccine lymph taken up on the point of a surgeon's lancet spreads disease through the whole body. Here, also, as in the case of contagious disease, there was a period of incubation. In proof of what the lecturer had stated that the contagion of these communicable diseases was not gaseous or liquid, but solid particles, he would describe an experiment he had made only a few weeks since. Eighteen months ago he had a chamber prepared from which all floating particles of dust were removed, and in it he placed a number of vessels containing animal and vegetable refuse which soon fell into putrefaction, and also two or three vessels containing perfectly clear beef-tea and mutton broth, as transparent as water, in which the infective particles had been killed by heat. Although all these vessels had stood for eighteen months side by side there had been no communication of contagion from one to the other. The beef tea and mutton-broth remained as transparent as when put in, though the other vessels emitted a most noisome stench. But if a bubble were produced in one of the putrefying masses by blowing into it, and if on rising to the surface and bursting the spray of the bubble was allowed to fall into the transparent beef-tea or mutton-broth, in forty-eight hours it became as bad as its neighbours. It was not therefore sewer gas which did the mischief, but the particles which were carried and scattered by the sewer gas. Referring to another point on which the lecturer had insisted—viz., that there was no power of spontaneous generation of the germs or contagion of diseases, Prof. Tyndall said that, though at present great names were opposed to that view, he would venture to predict that ten years hence there would be very few great names opposed to the lecturer on that matter. With regard to the power of specific contagia to be generated in decomposing animal matter, he would say that for the last twenty-one years he had been in the habit of visiting the upper Alpine valleys, where, amongst the Swiss chålets, there was the most abominable decomposition going on from day to day, and exceedingly bad smells, but there these contagious diseases were entirely unknown. If, however, a person suffering from typhoid fever were transported there, the disease would spread like wildfire from this infected focus, and probably take possession of the entire population. It might be taken, therefore, that any of these special diseases required its special germ or seed for its production, just as you required a grape seed to produce a vine. He entirely agreed with all that the lecturer had stated as to these diseases "breeding true." He never found the virus of small-pox producing typhoid, or *vice versâ*. The subject was one of the most important which could engage the attention of the scientific physician—indeed, Prof. Tyndall doubted whether, in the whole range of medical art and science there was a subject of equal importance. But in dealing practically with this question of infectious disease, the scientific physician must not stand alone—he ought to be aided by the sympathy of an enlightened public. Here, in England, we did not like to be pressed into good behaviour by external influence; and if anything was to come in the way of really great sanitary improvement, it would be from the people themselves. Hence, in a people who were jealous of government interference, it was of primary importance that they should be properly instructed; and he did not exaggerate in the slightest degree in declaring that sound and healthy instruction had been imparted to them in the lecture which they had just heard.

SUSPECTED RELATIONS BETWEEN THE SUN AND THE EARTH

I.

WHEN the telescope first enabled us to scrutinise the solar surface, the spots thereby revealed formed a stumbling-block to some of the early observers, who were unwilling to attribute the smallest taint of imperfection to our luminary. And although the spots came speedily to be recognised as true solar appendages, yet until comparatively recent times they were looked upon as mere scientific curiosities, having no perceptible reference to ourselves, or indeed to anything else.

In the eyes of the last century astronomers the sun shone upon the earth and kept us in leading-strings, and this was an end of the whole matter. But we have now advanced one step beyond the position of those men, inasmuch as we have accumulated evidence tending to show that the physical state of the solar surface affects us in a variety of ways. With regard to some of these we are nearly certain, while with regard to others we are less so; in all we are profoundly interested, but we are not yet fully awake to a true measure of our responsibility or to the necessity of keeping a continuous watch upon the sun. It may perhaps be desirable here to review the somewhat heterogeneous mass of evidence from various quarters which leads us to believe in the existence of these peculiar relations.

SOLAR SPOTS, MAGNETIC DECLINATION, AND AURORAL DISPLAYS.

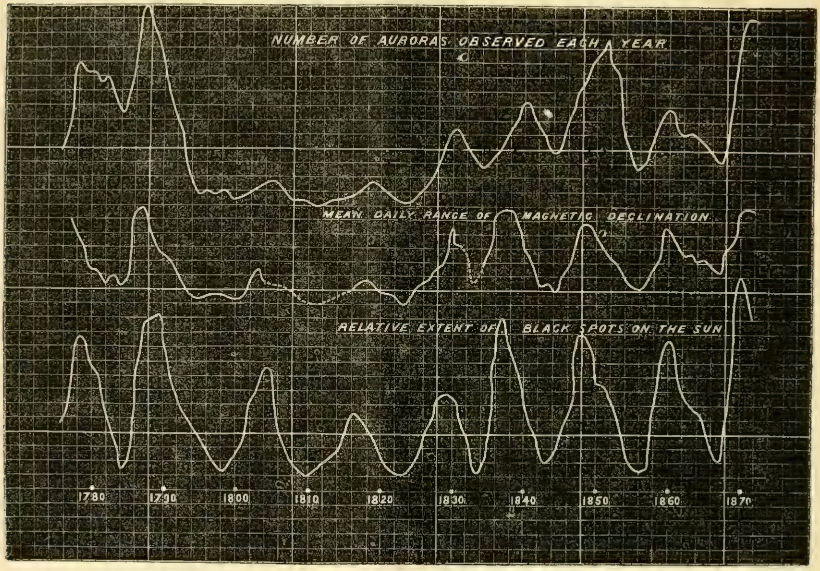


DIAGRAM A

About fifty years ago a careful observer, Hofrath Schwabe, of Dessau, with true Teutonic persistence set himself to observe the sun's surface day after day, and as the result of forty years' observations, he was rewarded by the discovery of a cycle in the frequency of spots. This will be seen from the following table :—

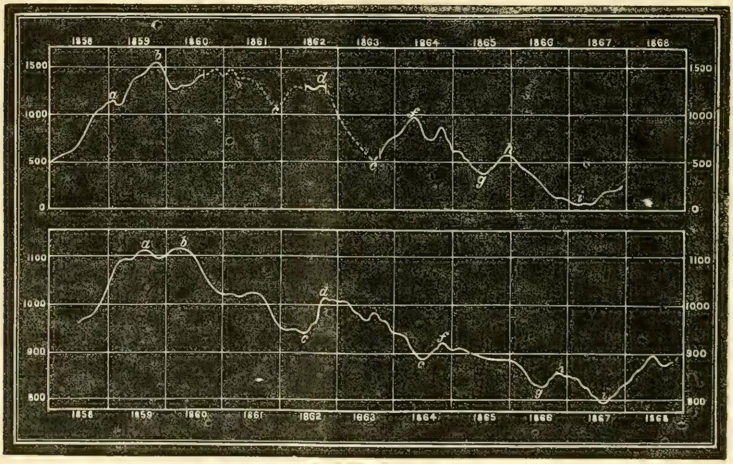


DIAGRAM B.—The upper curve denotes sun-spot fluctuations, the lower curve magnetic fluctuations.

TABLE I.

Number of New Groups of Sun-spots observed each Year. The Observations from 1826 *to* 1863 *were made by Hofrath Schwabe, the others were made at the Kew Observatory.*

Year.	No. of new groups.	Year.	No. of new groups.	Year.	No. of new groups.
1826	118	1842	68	1858	188
1827	161	1843	34	1859	205
1828	225	1844	52	1860	210
1829	199	1845	114	1861	204
1830	190	1846	157	1862	160
1831	149	1847	257	1863	124
1832	84	1848	330	1864	113
1833	33	1849	238	1865	93
1834	51	1850	186	1866	45
1835	173	1851	151	1867	17
1836	272	1852	125	1868	115
1837	333	1153	91	1869	224
1838	282	1854	67	1870	403
1839	162	1855	38	1871	271
1840	152	1856	34	1872	186
1841	102	1857	98		

From which it appears that 1828, 1837, 1848, 1860, and 1870 were years of maximum, while 1833, 1843, 1856, and 1867 were years of minimum sun-spot frequency.

While Schwabe was observing the sun with praiseworthy regularity, Sir E. Sabine was likewise observing the magnetism of the earth. A freely suspended magnetic needle is usually thought to be very constant as to the direction in which it points, and this is no doubt quite true as far as large fluctuations are concerned. Nevertheless, between certain small limits it is always in motion—it has, for instance, a well understood oscillation depending upon the hour of the day, besides which it is also liable to irregular fluctuations that occur abruptly. Now Sabine perceived that these abrupt and spasmodical affections of the needle were most frequent in years when sun-spots were most frequent; and, furthermore, inasmuch as these fluctuations of the magnet are almost invariably accompanied with displays of the aurora borealis, he came to the conclusion that auroral displays occur most frequently in years of maximum sun spots. Our readers will no doubt remember the brilliant auroræ of 1870, which was likewise (see Table 1) a year of maximum sun-spot frequency.

What we have said refers to the spasmodical affections of the needle, but its diurnal oscillations are not less dependent on the state of the sun's surface.

Here also we have a maximum amount of fluctuation in years of maximum sun-spot frequency.

This near relation between sun-spots on the one hand, and magnetic oscillations and auroral displays on the other, is exhibited in Diagram A, which has been compiled by Prof. Loomis, the well-known American meteorologist.

Close and striking as is the relation between these three associated phenomena exhibited in the above diagram, the intimacy of this connection may be rendered even more obvious if we confine ourselves to such observations of the solar surface and of magnetic fluctuations as have been made with the greatest possible accuracy.

For this purpose Schwabe's eye-observations are not precise enough, and we must, as far as sun-spots are concerned, make use of some very accurate measurements of the solar spotted area made at Redhill by the late R. C. Carrington, along with the results deduced from the solar photographs taken at the Kew Observatory, under the superintendence of Mr. Warren De La Rue.

Again, as far as magnetic observations are concerned, let us employ the results derived from the self-recording magnetographs at the Kew Observatory.

Furthermore, in order to equalise oscillations of short period, let us plot a solar curve, each point of which represents the mean of nine months' sun-spot observations, and alongside of it a magnetic curve, each point of which similarly represents the mean of nine months' magnetic observations.

A comparison of this kind has been made by the writer of these remarks, the results of which were recently communicated by him to the Royal Society. These results are embodied in Diagram B, in which accurate sun-spot observations are compared with Kew declination ranges, that is to say, with the diurnal oscillations of a magnetic needle, freely suspended at the Kew Observatory.

A comparison of the two curves given above will show us that almost every prominent fluctuation of the sun-spot curve is represented in the magnetic curve, similar letters being employed to denote what appears to be corresponding fluctuations.

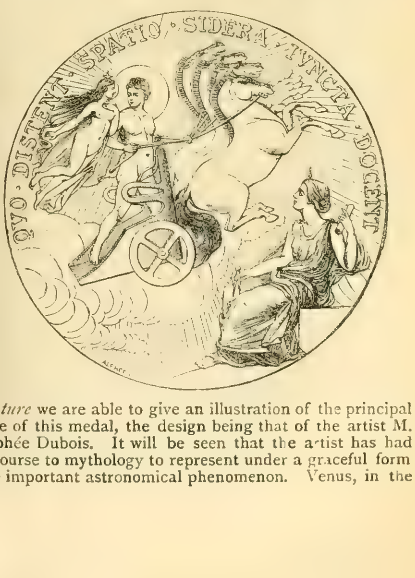
There is, however, a greater want of similarity for that part of the solar curve which is dotted, but this represents the results of eye-observations taken by Hofrath Schwabe, while the more accurate Kew photoheliograph was unfortunately out of action.

It will be perceived that the magnetic fluctuations invariably follow after or lag behind the corresponding solar fluctuations in point of time, the mean amount of this lagging being probably six months. We may therefore conclude from these comparisons that there is a very close and intimate relation between the physical condition of the sun's surface and the diurnal oscillations of the magnetic needle freely suspended at the Kew Observatory, and also that the former is probably the cause of which the latter is the effect, or at least that the magnetic change lags behind the corresponding solar phenomenon in point of time. BALFOUR STEWART

(*To be continued.*)

THE FRENCH TRANSIT MEDAL

WE recently announced that the Paris Academy of Sciences had presented an appropriate medal to those Frenchmen who were engaged in observing the recent transit of Venus, as well as to all the members of the Academy. By the kindness of the editor of *La Nature* we are able to give an illustration of the principal face of this medal, the design being that of the artist M. Alphée Dubois. It will be seen that the artist has had recourse to mythology to represent under a graceful form the important astronomical phenomenon. Venus, in the



simple costume of the goddesses, passes before the car of Apollo, the god of the sun, while Science observes the phenomenon on the earth and records the results. The legend is the composition of a member of the Academy of Inscriptions. On the obverse of the medal is the following inscription :—

INSTITUT DE FRANCE
ACADÉMIE DES SCIENCES
PASSAGE DE VÉNUS SUR LE SOLEIL
8–9 DECEMBER, 1874.

THE EFFECT OF INAUDIBLE VIBRATIONS UPON SENSITIVE FLAMES

DURING a recent visit to Birmingham my friend and host, Mr. Lawson Tait, showed me some interesting experiments with one of Mr. Galton's whistles, capable of yielding vibrations beyond the limit of hearing. This led to the suggestion of trying a sensitive flame with these whistles, and in fulfilment of my promise to select and send to Mr. Tait a burner sensitive to very high notes, I was yesterday led to make the following experiment, the result of which is, I believe, new, and I think sufficiently interesting to put on record. A sensitive flame was obtained just two feet high when undisturbed, but shrinking down to seven inches under the influence of the feeblest hiss or the clink of two coins. Adjusting the Galton whistle, which Mr. Tait lent me, so as to yield its lowest note, little effect was produced on the flame ; a shrill dog-whistle produced a slight forking of the flame, but that was all. Raising the pitch of the Galton whistle, the flame became more and more agitated, until, when I had nearly reached the upper limit of audibility of my left ear, and had gone quite beyond the limit of my right ear, the flame was still more violently affected. Raising the pitch still higher, until I quite ceased to hear any sound, and until several friends could likewise detect no sound, even when close to the whistle, I was astonished to observe the profound effect produced upon the flame. At every inaudible puff of the whistle the flame fell fully sixteen inches, and burst forth into its characteristic roar, at the same time losing its luminosity, and when viewed in a moving mirror, presenting a multitude of ragged images, with torn sides and flickering tongues—indicating a state of rapid, complex, and vigorous vibration.

Nor was this effect sensibly diminished by a distance of some twenty feet from the flame. Placing the flame at one end of the large lecture theatre of this college, and blowing the whistle at the furthest point away, a distance at least of fifty feet and more than thirty feet above the flame, still the effect produced was very pronounced. There can hardly be a more striking experiment. A single silent and gentle puff of air sent from the lips through the whistle, nothing whatever to be heard, and yet fifty feet away an effect produced that might readily be seen by thousands of people.

The extreme smallness of the amount of motion actually concerned in producing this great change in the aspect of the flame is evident. For the inaudible vibrations, having at their origin but a small amplitude, gave rise to a spherical air-wave,[1] which at a radius of fifty feet—and with the vast enfeeblement due to this distance—knocked down a two-foot flame, though the surface acted upon had an area of less than a square inch—for it is only the root of the flame that picks up the wave motion. Of course everything depends upon the delicately-poised state into which the flame has previously to be brought. It then, like a resonant jar, enters into a state of vibration which appears to be synchronous with the note producing the effect. By this means it may be possible, with the aid of a mirror moving at a known speed, to determine the vibration number of these high notes, and thus with greater exactitude fix the upper limit of hearing.

The flame giving the effect here described was produced by coal gas contained in a holder under a pressure of ten inches of water, and issuing from a steatite jet having a circular orifice 0·04 inch in diameter.[1]

W. F. BARRETT

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[1] I have no doubt a similar result would attend an experiment made in the open air, if the air were still enough to allow it to be made.

[1] The conditions necessary for obtaining the utmost sensitiveness of the flame are described in an article I published on the subject in the *Popular Science Review* for April, 1867.

SOUND-VIBRATIONS OF SOAP-FILM MEMBRANES

THE vibration-forms of membranes agitated by their fundamental and upper tones, have usually been studied by means of thin bladder or india-rubber stretched on a ring or frame (see Helmholtz "Sensations of Tone," chaps. iii. and v. ; Pisko, "Die Neueren Apparate der Akustik," p. 75). While I was lately trying with Mr. R. Knight the capabilities of various membranes of taking impressions from vocal sounds for phonautographic purposes, the idea occurred of using soap-film. This was at once carried into effect by dipping the end of a lamp-chimney into some soap-solution, strengthened in the usual way with glycerine and a little gelatine. On singing near the open end of the chimney, the series of forms belonging to the various notes became plainly visible, those produced by the upper tones being as it were engine-turned in their complex symmetry, in a way to which the sand-lines on so coarse a material as caoutchouc can bear no comparison. To exhibit these forms at a popular lecture here last night, the light of an oxyhydrogen magic lantern was simply reflected off the vibrating film upon the screen in a disc of some three feet in diameter, so as to show its patterns on a large scale when set in movement by talking, singing, and playing a cornet in its neighbourhood. The effects were of singular clearness and beauty. To lecturers who may use this new and easy means of making the more complex sound-vibrations appreciable by the eye, I would mention that by slightly thinning the soap-solution, and adding a few drops of ammonia, they may obtain a film more free from interference-colours, so as to display the vibration-figures on an almost clear ground. But if this is done, the thicker mixture should be used afterwards, for the gorgeous scenic effect of the masses of prismatic colour whirled hither and thither by the musical vibrations.

EDWARD B. TYLOR

Wellington, Somerset, April 20

THE OTHEOSCOPE[2]

I COMMUNICATED to the Royal Society in November last, an account of some radiometers which I had made with the object of putting to experimental proof the "molecular pressure" theory of the repulsion resulting from radiation. Continuing these researches I have constructed other instruments, in which a movable fly is caused to rotate by the molecular pressure generated on fixed parts of the apparatus.

In the radiometer, the surface which produces the molecular disturbance is mounted on a fly, and is driven backwards by the excess of pressure between it and the sides of the containing vessel. Regarding the radiometer as a heat-engine, it is seen to be imperfect in many respects. The black or driving surface, corresponding to the heater of the engine, being also part of the moving fly, is restricted as to weight, material, and area of surface. It must be of the lightest possible construction, or friction will greatly interfere with its movement; it must not expose much surface, or it will be too heavy; and it must be a very bad conductor of heat, so as to retain the excess of pressure on one side. Again, the part corresponding to the cooler of the engine (the side of the glass bulb) admits of but little modification. It must almost necessarily be of glass, by no means the best material for the purpose; it is obliged to be of one particular shape; and it cannot be brought very near the driving surface.

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[2] On Repulsion Resulting from Radiation. Preliminary note on the Otheoscope, by William Crookes, F.R.S., &c Read before the Royal Society, April 26, 1877.

A perfect instrument would be one in which the *heater* was stationary; it might then be of the most suitable material, of sufficient area of surface, and of the most efficient shape, irrespective of weight. The *cooler* should be the part which moves; it should be as close as possible to the heater, and of the best size, shape, and weight, for utilising the force impinging on it. By having the driving surface of large size and making it of a good conductor of heat, such as silver, gold, or copper, a very faint amount of incident radiation suffices to produce motion. The black surface acts as if a molecular[1] wind were blowing from it, principally in a direction normal to the surface. This wind blows away whatever easily movable body happens to be in front of it, irrespective of colour, shape, or material; and in its capability of deflection from one surface to another, its arrest by solid bodies, and its tangential action, it behaves in most respects like an actual wind.

Whilst the radiometer admits of but few modifications, such an instrument as the one here sketched out is capable of an almost endless variety of forms; and as it is essentially different in its construction and mode of action to the radiometer, I propose to identify it by a distinctive name, and call it the Otheoscope (ωθέω, I propel).

The glass bulb is an essential portion of the machinery of the radiometer, without which the fly would not move; but in the otheoscope the glass vessel simply acts as a preserver of the requisite amount of rarefaction. Carry a radiometer to a point in space where the atmospheric pressure is equal to, say, one millimetre of mercury, and remove the glass bulb; the fly will not move, however strong the incident radiation. But place the otheoscope in the same conditions, and it will move as well without the case as with it. In the preliminary note already referred to,[2] I described a piece of apparatus by which I was able to measure the thickness of the layer of molecular pressure generated when radiation impinged on a blackened surface at any degree of exhaustion. At the ordinary density of the atmosphere the existence of this molecular disturbance was detected several millimetres off, and its intensity increased largely as the generating surface and movable plate were brought closer together. It would be possible, therefore, to construct an otheoscope in which no rarefaction or containing vessel was necessary, but in which motion would take place in air at the normal density.[3] Such a heat-engine would probably work very well in sunlight.

Aided by the mechanical dexterity of my assistant, Mr. C. H. Gimingham, I have constructed several varieties of otheoscope. These I propose to exhibit at the *soirée* of the Royal Society on Wednesday next, as illustrations of the very beautiful manner in which, at this stage of my investigations, theory and experiment proceed hand in hand, alternately assisting each other, and enlarging our knowledge of those laws of molecular movement which constitute a key to the relations of force and matter.

The following is a list of the otheoscopes I have already made, together with some new experimental radiometers, which will be exhibited for the first time on Wednesday:—

1. *Otheoscope.*—A four-armed fly carrying four vanes of thin clear mica is mounted like a radiometer in an exhausted glass bulb. At one side of the bulb a plate of mica blacked on one side is fastened in a vertical plane in such a position that each clear vane in rotating shall pass the plate leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced; but if the light shines on the black plate the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near.

2. *Otheoscope.*—A four-armed fly carries roasted mica vanes and is mounted in an exhausted glass bulb like a radiometer. Fixed to the side of the bulb are three plates of clear mica equidistant from each other in a vertical plane, but oblique to the axis. A candle brought near the fixed plates generates molecular pressure, which, falling obliquely on the fly, causes it to rotate.

3. *Otheoscope.*—A large horizontal disc, revolving by the molecular disturbance on the surface of inclined metallic vanes, which are blacked on both sides in order to absorb the maximum amount of radiation.

4. *Otheoscope.*—Inclined aluminium vanes driven by the molecular disturbance from the fixed black mica disc below, blowing (so to speak) through them.

5. *Otheoscope.*—A large horizontal coloured disc of roasted mica, driven by inclined aluminium vanes placed underneath it.

6. *Otheoscope.*—A bright aluminium disc cut in segments, and each segment turned at an angle, driven by a similar one below of lampblacked silver.

7. *Radiometer.*—A vertical radiometer, made with eight discs of mica blacked on one side, and the whole suspended on a horizontal axis which works in two glass cups. The motion of the radiometer is assisted on each side by driving vanes of aluminium blacked on one side.

8. *Radiometer.*—A vertical turbine radiometer, the oval vanes of roasted mica blacked on one side.

9. *Radiometer.*—A spiral radiometer of roasted mica blacked on the upper side.

10. *Radiometer* of large size, showing great sensitiveness.

11. *Radiometer.*—A two-disc radiometer, the fly carrying roasted mica discs blacked on one side; in front of each blacked surface is fixed a large disc of thin clear mica. The molecular disturbance set up on the black surface, and streaming from it, is reflected in the opposite direction by the clear plate of mica, causing the fly to move abnormally, *i.e.*, the black surface towards the light.

12. *Radiometer.*—A two-disc radiometer, the fly carrying roasted mica discs blacked on one side, similar to No. 11, but with a large clear disc on each side. The molecular disturbance, prevented from being reflected backwards by the second clear disc, is thus caused to expend itself in a vertical plane, the result being a total loss of sensitiveness.

13. *Radiometer.*—A two-disc, cup-shaped, aluminium radiometer, facing opposite ways; both sides bright. Exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3·37 seconds. A screen placed in front of the concave side so as to let the light shine only on the convex surface repels the latter, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the convex side is screened off, so as to let the light shine only on the concave, continuous rotation is produced at the rate of one revolution in 6·95 seconds, the concave side being apparently attracted. These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive

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[1] *Molecular*, not *molar*. There is no wind in the sense of an actual transference of air from one place to another. This molecular movement may be compared to the movement of the gases when water is decomposed by an electric current. In the water connecting the two poles there is no apparent movement, although eight times as much matter is passing one way as the other.

[2] *Proc.* Royal Soc., November 16, 1876, p. 310.

[3] Since writing this I have constructed such an instrument. The movement takes place in the way I had anticipated.—W. C., April 26, 1877.

action of radiation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

14. *Radiometer.*—A two-disc, cup-shaped, aluminium radiometer, lamp-blacked on the concave surfaces. In this instrument the usual action of light is reversed, rotation taking place, the bright convex side being repelled, and the black concave attracted. When the light shines only on the bright convex side, no movement is produced, but when it shines on the black concave side, this is attracted, producing rotation.

15.—*Radiometer.*—A cup-shaped radiometer similar to the above, but having the convex surfaces black and the concave bright. Light shining on this instrument causes it to rotate rapidly, the convex black being repelled. No movement is produced on letting the light shine on the bright concave surface, but good rotation is produced when only the black convex surface is illuminated.

16. *Radiometer.*—A multiple-disc, cup-shaped, turbine radiometer, bright on both sides, working by the action of warm water below and the cooling effect of the air above.

17. *Radiometer.*—A four-armed metallic radiometer with deep cups, bright on both sides.

18. *Radiometer.*—A four-armed radiometer, the vanes consisting of mica cups, bright on both sides.

19. *Radiometer.*—A four-armed radiometer having clear mica vanes. The direction of motion being determined by the angle formed by the mica vanes with the inner surface of the glass bulb.

DROUGHTS AND FAMINES IN SOUTHERN INDIA[1]

THE paper on this subject, noted below, a copy of which we have just received, will no doubt awaken much interest, not only on account of its scientific bearings but also from its bearings on so very practical a subject as the famines of India. It is most gratifying to see that the subject has been taken up by one who gives evidence on every page of rare capacity as a scientific statistician. There is throughout an absence of straining the facts before him beyond what they may legitimately bear, and a skill in combining them so as to eliminate, as far as possible, what is merely accidental from the results ultimately arrived at in their relation to the sun-spot period.

The data discussed in Dr. Hunter's paper are the amounts of the annual rainfall at Madras from 1813 to 1876, and the relative number of sun-spots from 1810 to 1876. The results of the inquiry are given in the following six propositions:—

1. That no uniform numerical relation can be detected between the relative number of the sun-spots and the actual amount of rainfall.

2. That although no uniform numerical relation can be detected between the relative number of sun-spots and the actual amount of rainfall, yet that the minimum period in the cycle of sun-spots is a period of regularly recurring and strongly marked drought in Southern India.

3. That, apart from any solar theory, an examination of the rain registers shows that a period of deficient rainfall recurs in cycles of eleven years at Madras; that this period consists of the eleventh and second series of years in the cycle; which two series also contain six out of the seven years of minimum sun-spots falling in this century up to 1878.

4. That after the period of minimum rainfall in the eleventh and second series of years in the cycle, the rainfall rises to a maximum in the fifth year; after which it again declines to its minimum period in the eleventh and second years.

5. That, apart from any solar theory, the statistical evidence shows that the cycle of rainfall at Madras has a marked coincidence with a corresponding cycle of sun-spots; that in this cycle of eleven years both the sun-spots and the rainfall reach their minimum in the group consisting of the eleventh, first, and second years; that both the rainfall and the sun-spots then increase till they both reach their maximum in the fifth year, after which they decline together till both again enter their minimum period in the eleventh, first, and second series of years.

6. That while the statistical evidence discloses a cycle of drought in Southern India, coincident in a marked manner with a corresponding cycle of sun-spots, it also tends to show that the average rainfall of the years of minimum rainfall in the said cycle approaches perilously near to the point of deficiency which causes famine. That the average is, however, above that point; and that, while we have reason to apprehend recurring droughts and frequent famines in these cyclic years of minimum rainfall, the evidence is insufficient to warrant the prediction of a regularly recurring famine.

It will be observed that these results are strongly confirmatory of the general conclusions arrived at by Meldrum and others, who have examined the question from data collected from a large area, and embracing an extended series of years, the only noteworthy point of difference being the larger rainfall of the first year of the cycle, as compared with the eleventh and second years which immediately precede and follow it. It is perhaps only to be looked for that such an anomaly should be met with in dealing with the rainfall of only one place, embracing a period of sixty-four years, seeing that the accidental occurrence of one or two cyclones, accompanied with unusually heavy local rainfall, would be sufficient to produce the anomaly in question. The anomaly would in all likelihood have disappeared if the area of observation had been wider or the time of observation longer. It is scarcely necessary to do more than point out the absolute necessity of establishing physical observatories in order to obtain the data for the investigation of the connection between the state of the sun's surface and the state of terrestrial convection currents, it being only through their cosmical relations that we may reasonably hope to solve many of the more difficult problems of meteorology, some of which lead to intensely practical issues.

OUR ASTRONOMICAL COLUMN

MR. GILL'S EXPEDITION TO ASCENSION.—In an address to the Royal Astronomical Society on April 8, 1857, "On the means which will be available for correcting the measure of the sun's distance during the next twenty-five years," the Astronomer-Royal directed attention to a method of making observations for parallax, not applicable to the planet Venus, but applicable to Mars, namely, by "observing the displacement of Mars in right ascension when he is far east of the meridian, and far west of the meridian, as seen at a single observatory," and he particularised the advantage of this method, and expressed his opinion that it is "the best of all." The observations are not attended with the very great expense which is involved in the efficient observation of a transit of Venus, indeed if made at an established observatory need entail little or no cost; they may be conducted by a single observer or series of observers, in the latter case with a due regard to personal equation, and each observatory co-operating in the work, will furnish a result quite independent of the rest, so that the observer has the satisfaction of knowing that by the method recommended his own observations alone will give a value for the most important unit of measure in astronomy. The Astronomer-Royal confined his remarks to the observation of differences of right ascension, recommending as of the first consequence a firmly-mounted equatorial, and as advantageous though not absolutely necessary the chronographic method of transits first introduced by the American astronomers. The oppositions of Mars in 1860 and 1862 were referred to with regard to their relative advantages for such observations.

Mr. Gill has taken a further and an important step in the direction of utilising observations of Mars for the determination of the solar parallax. Encouraged by Lord Lindsay's liberal offer of the loan of the heliometer employed in the expedition to the Mauritius for the observation of the transit of Venus, Mr. Gill proposes to leave England this month for the island of Ascension, and to apply the heliometric method of measurement of distances instead of observing differences of right ascension, as suggested in the Astronomer-Royal's address, and as was stated in NATURE last week, the council of the Royal Astronomical Society have guaranteed 500*l.* for the expenses of Mr. Gill's expedition. Ascension has been fixed upon, not without a careful consideration of probable meteorological conditions about the time of the opposition of Mars in September, in which it is understood the records of the Meteorological Office have been of the greatest service, and in fact, have induced Mr. Gill to fix upon Ascension for the site of his temporary observatory in preference to St. Helena, the astronomical condition being about the same for the two islands, *i.e.*, their latitudes not differing much from the declination of the planet when nearest to the earth, so that it is observable at a considerable hour-angle on both sides of the meridian.

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[1] "The Cycle of Drought and Famine in Southern India," by W. W. Hunter, LL.D., Director-General of Statistics to the Government of India.

The *modus operandi* proposed by Mr. Gill, is as follows :—two stars, *a* and *b*, one preceding and the other following the planet, are selected for each night of observation, and their angle of position and distance relative to the planet are computed roughly for 4h. E. and 4h. W. hour-angle, and the right ascension and declination of the middle point between star and planet ; so that the proper stars of comparison are readily found. The heliometer axis is directed to this middle point, the position-circle set to the position-angle, and the segments set to the approximate distance. The observer finds in the field of view a star and the planet ; by turning the handle by which the segments are moved in *distance*, the images of star and planet are made to move relatively to each other in the direction of a line joining the objects, while if the position-handle is turned, the images move in the direction of a line perpendicular thereto. Suppose that the star *a* is viewed through segment A, and the planet through segment B. According to Mr. Gill's usual practice the observation would proceed thus :—

I. Measure of position-angle.
II. Measure of distance, both limbs.
III. Reverse segments, and view star by segment B and planet by segment A.
IV. Measure position-angle.

This constitutes one measure.

V. Repeat this process with star *b*.
VI. Reverse position-circle and repeat the comparison with star *b*.
VII. Compare again with star *a*.

This constitutes a complete symmetrical set, which Mr. Gill has found can be secured on an average in 1h. 30m., sometimes in 1h. 10m., or if there be interference from cloud it may occupy 2h.

In the measure of a position-angle, by a movement of the handle for distance, the star may be made to move, relatively to the planet along the line of separation of the lenses, so that the star successively occupies positions 1, 2, 3 . . . 3, 2, 1, &c. This motion may be very slow and the position-circle being set so that the motion of the planet completes the bisection, the observer has only to go on moving the star slowly till the limb is seen to symmetrically bisect the star (the time of which is noted) precisely as Jupiter's limb bisects one of his satellites.

The measure of distance is conducted with equal care, but is not so readily explained without a diagram. Mr. Gill finds his method possesses very great delicacy. It sometimes happens that it is not possible to find a star sufficiently bright to compare with Mars in his full light. In such cases the brilliancy of the planet can be easily kept down by a wire-gauze screen, which, by an arrangement at the eye-end, can be laid over either segment of the object-glass and at any angle thereto.

In a letter to M. Leverrier, published in the *Bulletin International* of April 27, Mr. Gill states that the observations of Juno, which he made with Lord Lindsay at the Mauritius with the same heliometer, showed that the determination of the diurnal parallax by measuring with this instrument the distance of the planet from a star preceding and a star following is susceptible of an extreme precision, and he found the probable error in the determination of the planet's position for each complete observation of the morning or evening did not exceed ± 0″·075. Lord Lindsay has stated that the value of the solar parallax, resulting from these observations of Juno (a single discordant one only being rejected) is 8″·82, which approaches near to Prof. Newcomb's value, 8″·85, adopted provisionally by the German astronomers, and to M. Leverrier's theoretical determination, 8″·86. This sufficiently indicates the utility of the method, and Mr. Gill intends to avail himself of the close oppositions of the minor planets Ariadne and Melpomene during his visit to Ascension to obtain values of the parallax by observation on the same principle.

COMET 1877 II. (WINNECKE, APRIL 5).—This comet may be expected to prove a fine telescopic object during the absence of moonlight in the circumpolar sky, with its stellar-looking nucleus and double or broad fan-shaped tail. The annexed positions for midnight at Berlin are from elements by Herr Plath, of Hamburg, and have been received from Prof. Winnecke :—

		R.A				Declination.			Log. Distance from Earth.,
		h.	m.	s.		°	′		
May 4	...	23	26	5	...	+65	59·2	...	9·99601
5	...	23	36	41	...	68	7·2	...	9·99640
6	...	23	49	22	...	70	11·6	...	9·99741
7	...	0	4	45	...	72	11·3	...	9·99902
8	...	0	23	31	...	74	4·5	...	0·00123
9	...	0	46	49	...	75	48·8	...	0·00400
10	...	1	15	34	...	77	21·5	...	0·00732
11	...	1	50	47	...	78	38·3	...	0·01116
12	...	2	32	59	...	79	35·1	...	0·01549
13	...	3	19	55	...	80	7·8	...	0·02028
14	...	4	8	57	...	+80	13·6	...	0·02548

The following orbit has been calculated by Mr. Hind from observations at Strasburg, on April 5 and 25, and at Berlin and Leipsic on April 14 :—

Perihelion Passage, 1877, April 17·64687, G.M.T.

	°	′	″	
Longitude of Perihelion	253	30	9	Mean Equinox, 1877·0
,, Ascending Node ...	316	33	53	
Inclination	58	54	22	
Distance in Perihelion	0·950250			

Heliocentric motion—retrograde.

These elements represent the observations during the interval very closely.

NOTES

M. FLAMMARION has been authorised by M. Leverrier to use one of the largest refractors of the Paris Observatory for the investigation of the motion of double-stars round a common centre of attraction. This liberality on the part of the chief of the Paris Observatory is highly creditable. M. Leverrier, indeed, is desirous of placing the immense means of investigation possessed by the observatory at the service of a number of independent workers not belonging to the staff of the establishment, but who have given solid proofs of their zeal and capacity for research in some particular science. His ambition is to create at the observatory a national astronomical institution where qualified scientific men may find ample means for following their own special studies.

SIR DAVID MONRO, late Speaker of the House of Representatives in New Zealand and an active promoter of science in that colony, died at Nelson, New Zealand, on February 15. He graduated in Medicine in the University of Edinburgh in 1834, where his great grandfather, grandfather, and father successively held the Chair of Anatomy. He devoted the leisure of an active political life to the pursuit of botany, and by his discoveries, which were published by Dr. Hooker in his "New Zealand

Flora," he added largely to our knowledge of the vegetation of New Zealand, on which he also wrote an instructive essay that is published in the first volume of the *Transactions* of the New Zealand Institute.

We learn with the greatest pleasure that the Health Committee of the Police Board of Glasgow has agreed to carry out at eight stations in that city the system of continuous automatic observation of the constituents of the air, special attention being given to its impurities arising from manufactures and other causes which has been devised and worked out since March, 1876, by Mr. E. M. Dixon, in connection with Dr. Russell, Medical Officer of Health. The Committee has already expended fully 200*l.* in fitting up a laboratory and the observing stations with the instruments required, and are prepared to expend a sum of 300*l.* per annum in carrying out this very important practical investigation. The results, including meteorological observations made in connection with the scheme, will be published monthly, the first number appearing in June next.

Gay-Lussac, the great French physicist and chemist, was born in 1778, and his centenary will be celebrated by a festival and the erection of a statue either in Limoges or Paris.

A series of lectures upon zoological subjects will be given in the Zoological Gardens, Regent's Park, on Thursdays at 5 P.M., after Whitsuntide. The first lecture will be delivered in the Lion House, and others in the lecture-room near the Reptile House. May 24: "The Lion House and its Inhabitants," P. L. Sclater, F.R.S; May 31: "Sea-urchins and Star-fishes," Prof. Huxley, F.R.S.; June 7: "Sloths and Ant-eaters," Prof. Flower, F.R.S.; June 14: "Whales and Porpoises," Prof. Flower, F.R.S.; June 21: "Man-like Apes," Prof. Garrod, F.R.S.; June 28: "Variation in Domestic Animals," W. B. Tegetmeier, F.Z.S.; July 5: "Hornbills and their Habits," Dr. Murie, F.Z.S.; July 12: "Birds of Prey," R. B. Sharpe, F.Z.S.; July 19: "Frogs and Toads," Prof. Mivart, F.R.S.; July 26: "The Ornithorhynchus," Prof. Garrod, F.R.S. These lectures will be free to Fellows of the Society and their friends, and to other visitors to the Gardens.

The annual *conversazione* of the Royal Society was held at Burlington House on Wednesday week, and was numerously attended. There was a large collection of instruments brought together, among the principal of which were the following:—An Automatic Spectroscope, which can be used with 2, 4, or 6 prisms, solar eye-piece arrangement, and new split slit, whereby any lines in the spectrum can be measured; a Heliostat, with large crown-glass prisms, to be employed with the spectroscope; Governor for 18-inch reflector, which will keep time with a variation of rate of five seconds per minute, at pleasure; all designed and exhibited by Lieut.-Colonel Campbell, of Blythswood, and constructed by A. Hilger.—Prof. W. G. Adams exhibited an Apparatus for producing interference of light by means of thick plates, and Apparatus for the reflection and refraction of radiant heat and light, fitted to Clifton's optical bench, and constructed by Messrs. Elliott Brothers. The half-prism direct-vision spectroscope made for Greenwich, about which there has recently been a correspondence in Nature, was also shown. Then there was a Hydroclinometer, an instrument for taking ranges, without any calculation, from coast batteries over 100 feet in height; a small hydroclinometer, a modified form of the above, for giving the inclination of slopes, &c., without any adjustment, and for larger guns; an electric position- and range-finder for coast batteries; a Field-Artillery range-finder; an Infantry range-finder; a patent self-adjusting optical square, which by a simple adjustment can immediately be corrected to the true right angle, without the aid of any other instrument; an electric chronograph, for the measurement of minute portions of time, velocities of shot, &c., by the *free* fall of a weight; these were exhibited by Capt. Watkin, R.A.—Lieut. G. S. Clarke and Prof. Herbert M'Leod showed an instrument called the Cycloscope, an apparatus for determining the speed of machinery by means of a tuning-fork or reed of known period; also for ascertaining the pitch of a tuning-fork by means of a cylinder rotating at a known speed. There were also Telephone and (patent) Thermo-electric Pile (in action), with specimens of Gray's telephone, exhibited by Messrs. C. and L. Wray; improved Holtz electrical machine with four plates and self-charging arrangement, in glass case, ready for use in any condition of the atmosphere, and Manometric apparatus, for showing effects of sound on a sensitive flame, exhibited by Mr. Ladd; teeth, bones, and ancient works of art lately found in caves in Derbyshire, exhibited by Mr. Boyd Dawkins, F.R.S.; specimens of cast and wrought iron treated by Prof. Barff's process for the prevention of corrosion, which consists in acting on iron at suitable temperatures with dry steam, exhibited by Prof. Barff; specimens of the core of well, from Meux's Brewery; the large induction-coil, with secondary wire of 280 miles, constructed for Mr. W. Spottiswoode by Mr. Apps (in operation), was shown in the meeting room, and Mr. Crookes's Otheoscope, of which we give an account this week.

On Monday Prof. Boyd Dawkins commenced a series of eight Field Lectures on Geology at Owens College. Six of the lectures will be in connection with excursions to various places from Manchester.

Mr. William Gossage, F.C.S., the inventor of several important processes in practical chemistry, died at Earlsleigh, Bowdon, Cheshire, on April 9, in his seventy-eighth year.

The Council of the Royal Geographical Society have awarded the Royal medal to Capt. Sir George S. Nares, R.N., for having commanded the Arctic Expedition of 1875-6, and to Pundit Nain Singh, for having added a greater amount to our positive knowledge of the map of Asia than any individual of our time. In his first great journey he for the first time determined the position of Lhassa, the capital of Tibet, besides surveying the course of the great river Tsanpo, or Bramaputra, from near its source to near its entrance into the Himalayan region; in his last he traversed and surveyed the high Plateau of Tibet from its extreme north-west to Lhassa, a line of 1,200 or 1,400 miles of entirely new country. No reward was ever better earned than that bestowed by the Society on Nain Singh, who, indeed, deserves to be ranked among the first of explorers. While pursuing his arduous and dangerous work he was paid at the rate of 7*l.* per month, and now retires, satisfied we believe, on a pension of 50*l.* a year. Through his labours we have now for the first time a scientific basis on which to construct a map of Tibet. A gold watch, with an appropriate inscription, was at the same time awarded to Capt. Albert Markham, R.N., for having commanded the northern division of sledges in the Arctic Expedition of 1875-6, and for having planted the Union Jack in 83 deg. 20 min. 26 sec. N., a higher latitude than had ever been reached by any previous expedition.

It is but a poor set-off to the horrors of war that it is a means of speading a real knowledge of geography; but that it does do so was shown in this country during the last Oriental war—the Crimean. As might have been expected, numerous war-maps have already appeared. The most satisfactory of these maps is a large one published by Mr. Stanford on the scale of fifty miles to an inch, including Turkey in Europe and her tributary states, together with such parts of neighbouring countries in Europe and Asia as are more immediately connected with the settlement of the Eastern Question. Any one wishing to follow the movements of the two armies could not obtain a better guide. All the physical and political features, including the railways up to date, are shown with great clearness. Mr. Stanford publishes two other war-maps, one on a smaller scale and at a cheaper price than the above, and Jankowsky's Russo-Turkish war-map, a picture or bird's-eye map of Turkey and the Black Sea. A very fine and moderate-priced map comes to us from Perthes, of Gotha. It is prepared by Dr. Petermann, and is evidently a compilation from several of the maps in Stieler's Atlas. It embraces all the country in Europe and Asia likely to be included in the theatre of war, so long at least as it is confined to the two combatants now in the field. This map, sold at a very moderate price, may be had in London from Mr. Stanford.

THE war just begun will in no way interfere, we believe, with the forthcoming Paris Exhibition. The works are progressing with such activity that everything but the Trocadero palace will be ready at an earlier date than was anticipated. The Trocadero building has been delayed by legislative difficulties, which, however, have been overcome, and that building will not be behind its time.

M. HENRY GIFFARD is constructing, near the Champ de Mars at Paris, a workshop for the preparation of sulphate of iron. The apparatus was tried for the first time last Friday, when the balloon *Eole* was inflated in an hour and a half, and was sent up with an aëronaut. The capacity of the balloon being 220 cubic metres, the rate of production is very satisfactory. It is expected that the sale of sulphate will cover almost all the expenses, so that numerous scientific ascents may be made in the ensuing summer. The monster captive balloon of 20,000 cubic meters will be inflated by the same process.

THE annual meeting of the Royal Institution was held on Tuesday. The Annual Report of the Committee of Visitors for the year 1876, testifying to the continued prosperity and efficient management of the Institution was read and adopted. The real and funded property now amounts to above 84,000*l.* entirely derived from the contributions and donations of the members. Seventy-two new members paid their admission fees in 1876.

THE forty-eighth anniversary of the Zoological Society was held on Monday. The number of fellows, fellows-elect, and annual subscribers at the close of the year 1876 amounted to 3,311, showing an addition to the strength of the society of seventy members during the year 1876. The number of honorary members at the same date was fourteen, of foreign members twenty-five, and of corresponding members 199. The total income of the society in 1876 was 34,955*l.*, exceeding that of the year 1875 by 6,216*l.* The total expenditure of the society in 1876 was 31,635*l.* The total assets of the society on December 31, 1876, were calculated to be 15,516*l.*, while the liabilities were reckoned at 4,430*l.* The total number of visitors to the society's gardens in 1876 had been 915,764, the corresponding number in 1873 (hitherto the most successful year in this respect) having been 713,048. The number of visitors in 1876 had therefore exceeded that of any other previous year since the opening of the gardens, by more than 200,000. The report stated that the total number of animals in the collection on December 31, 1876, had been 2,265.

IN the May part of Petermann's *Mittheilungen* Herr K. Zöppritz has a critical paper on Watson and Chippendall's Survey of the White Nile and Junker's Survey of the Sobat. Herr Zöppritz expresses some dissatisfaction with the observations of the former as being vague and careless and difficult to reconcile with data already obtained. A valuable paper by Dr. Dorst describes and discusses the movements of the ice between Greenland and Spitzbergen as observed by him in the steamer *Bienenkorb* in 1869. It is an important contribution to our knowledge of the currents of this region.

THE examination for the Sheepshanks Astronomical Exhibition, which is of the annual value of 50*l.* and tenable for three years, will be held on May 21 in Trinity College Lecture-room No. 4. All undergraduates of the University are eligible, but in the event of a candidate who is not a member of Trinity College being elected, he must become a member of Trinity. Candidates are required to send their names, and, if not members of Trinity, certificates of moral character and good conduct to one of the tutors of Trinity on or before May 19.

A DETAILED account of Father Cecchi's remarkable seismograph to which we recently referred will be found in the January number (1877) of *Elettricista*. The Cecchi seismograph has been adopted with good success at several of the larger Italian observatories and meteorological stations. In order to enable also smaller establishments to obtain a similar apparatus at much less cost, Prof. Cecchi has lately constructed a simpler one on the same principle, which meets all the requirements for seismical observations and gives nearly as many and as exact data as the larger instrument. A full description of this is now being published in the *Elettricista*, and the adoption of it for meteorological stations may be strongly recommended. The whole cost will not exceed 4*l.* or 5*l.*

M. SICARD, member of the Italian Anthropological Society, on making excavations on his property near Kischeneff, in Bessarabia, at a place called Moguil Liondia, discovered a very large tumulus of earth, with human skeletons, remains of iron objects, and an amulet of carved bone. One of the corpses appeared to have been interred with a horse, much in the same way as the tribes of the Tehuelches and Pehuelches still bury their fellow-men. Unfortunately the skulls were dispersed, but M. Sicard is going to continue his excavations, and will give a detailed account of his highly interesting discovery in the *Rivista di Antropologia e Etnologia*, published by Prof. Mantegazza.

AT the last meeting of the Ethnographical Section of the Russian Geographical Society, M. Poliakof, referring to the results of his last journey on the Obi, pointed out the remarkable similarity between the present state of civilisation of the Ostiaks and that of the prehistoric inhabitants of the reindeer period of France and Middle Europe. After a description of the features which the present flora and fauna of the banks of the Obi have in common with those of Europe at that period, M. Poliakof described the primitive mode of life of the Ostiaks. Their utensils and implements almost exactly resemble those of the stone period and the islands of the Pacific, being made exclusively of stone, of teeth and claws of bears, and of bone, and their clothes being either furs or woven from nettle filaments. M. Poliakof described at length their mode of life, their wretched homes, their customs, their family relations, and their religion, the latter being a mixture of the rudest fetishism with the strangest superstitions. This people are rapidly fading away before the advance of European civilisation.

DURING the diluvial epoch, the Danube entering into the Vienna Basin, formed an inland sea, and covered the Tertiary formations with deep layers of so-called loess, a mixture of loam, lime, sand, and foliaceous mica. The Imperial Academy of Sciences at Vienna has lately set in operation an extensive series of excavations with the view of uncovering the secrets hidden beneath this thick coating of alluvium, and has already been rewarded by interesting discoveries. The excavations in the neighbourhood of Zeiselberg have disclosed a widespread deposit of bones mingled with numerous evidences of the presence of mankind. These consist in quantities of charcoal, bones which have been worked, artificially prepared flints, &c. The bones among which these prehistoric remains were found, are those of the bear, horse, mammoth, ox, reindeer, rhinoceros, and wolf, all belonging to the diluvial fauna, and all apparently inhabiting the Vienna Basin at that distant epoch in the company of man, for a chance gathering of these remains through the agency of water is precluded by the local topography of the place.

News from M. Prshevalsky, forwarded on March 23 by telegraph from Vernyi, appears in the official paper, the *Russian Invalid*. On February 11 he had reached Lake Lob-Nor, *viâ* the Valley of the Lower Tarim. The population of the Valley is very sparse. Its height above the sea is somewhat more than 2,000 feet. Its flora and fauna very poor. The topography is quite different from that represented on the maps. He was, at the time of telegraphing, in the mountains Altyn-Tagh, some distance south of Lake Lob-Nor. The valleys of the exterior spurs of these mountains reach about 12,000 feet above the sea. Here, as well as in the lower regions there are wild camels. In the neighbourhood of Lob-Nor he found the ruins of two old towns. He was to spend February and March at Lob-Nor, April on the Lower Tarim, and May and June on the Tian-Shan, returning to Kuldja at the beginning of July.

We have received as a contribution to the Gauss Fund, 1*l.* from Mr. G. Griffith.

The additions to the Zoological Society's Gardens during the past week include an Egyptian Gazelle (*Gazella dorcas*) from Egypt, presented by Her Majesty the Queen; an Indian Leopard (*Felis pardus*) from India, presented by Dr. Sidney Smith; a Crested Porcupine (*Hystrix cristata*) from Ceylon, presented by Capt. Smerdon, s.s. *Orion;* a White Pelican (*Pelecanus onocrotalus*) from Egypt, presented by Mr. A. C. Henderson; a King Parrakeet (*Aprosmictus scapulatus*) from New South Wales, presented by Miss Jones; a Suricate (*Suricata zenik*) from South Africa, presented by Mr. J. Forbes Dixon; an Indian Cobra (*Naia tripudians*) from Ceylon, presented by the Hon. W. D. Wright; a Beisa Antelope (*Oryx beisa*), two African Sheep (*Ovis aries*), eight Vulturine Guinea Fowls (*Numida vulturina*), from East Africa, a Toque Monkey (*Macacus pileatus*) from Ceylon, deposited; a Pigmy Marmoset (*Hapale pygmæus*), two Bay-headed Parrots (*Caica leucogastra*), a Rough Terrapin (*Clemmys punctularia*) from the Upper Amazons, purchased.

SCIENTIFIC SERIALS

The American Journal of Science and Arts, April, 1877.—On the sensation of colour, by C. S. Peirce.—Note on the binocular phenomenon observed by Prof. Nipher, by J. Le Conte.—Revision of the genus Belemnocrinus, by C. Wachsmuth and F. Springer.—Thorpe's and Bunsen's methods for the estimation of nitrogen in nitrates, by S. W. Johnston.—Westfield during the Champlain period, by J. S. Diller.—New embryonic forms of trilobites, by S. W. Ford.—The winds of the globe, or the laws of atmospheric circulation over the surface of the earth, by J. H. Coffin.—On some nitro-derivatives of diphenylamide, by P. T. Austen.—On mineral analysis; on some fluorides; and on molecular volumes, by F. W. Clarke.—On the identity of the so-called Peganite of Arkansas with the Variscite of Breithaupt and Callamite of Damour, by A. H. Chester.—On a fibrous variety of sepiolite from Utah, by the same.—On Dr. Peale's notes on the age of the Rocky Mountains in Colorado, by J. J. Stevenson.

Poggendorff's Annalen der Physik und Chemie, No. 2, 1877.—The spectra of chemical compounds, by M. Moser.—Researches on the volume-composition of solid bodies, by M. Schröder.—Current regulator for gas, by M. Teclu.—Contribution to Boltzmann's theory of elastic reaction, by M. Kohlrausch.—Further communications on the connection between the viscosity and the galvanic conductivity of various liquids, by M. Grotrian.—On the theory of resonators, by M. Grinwis—On photography of the less refrangible parts of the solar spectrum, by M. Vogel.—Researches on the motions of radiating and irradiated bodies, by M. Zöllner.—New proof of the falsity of the emission theory of light, by E. Feussner.—On double excitation of the ebonite electrophorus, by M. Schlösser.

Beiblätter zu den Annalen.—Quantitative comparison between friction and galvanic electricity in respect of tension, by M. Nystrom.—On the deduction of a new electrodynamic fundamental law, by M. Clausius.—Thermoelectric researches, by M. Tidblom.

The *Naturforscher* (March).—From this part we note the following papers:—On the functions of the larger brain (cerebrum), by Herr Goltz.—On the expansion of growing vegetable cells through the tension existing between the contents of the cells and the membrane forming them, by Hugo de Vries.—On the spectrum of the new star in Cygnus, by R. Copeland.—On the atomicity of phosphorus, by Ira Remsen.—On the high tides in the River Elbe, by K. R. Bornemann.—On the reproduction of eels, by M. C. Dareste.—On the formation of hail, by H. Fritz.—On the inhalation of air by the roots of plants, by MM. P. P. Déhérain and J. Vesque.—On electrolysis accompanied by the development of hydrogen at both electrodes, by Emil Elsaesser.—On the daily and yearly course of magnetic declination, by J. Hann.—On the behaviour of leaves in an atmosphere free from carbonic acid, by B. Corenwinder.—On the companions of the pole star, by A. de Boe and others.—On the spreading of drops of liquids, by Fillipo Centolesi.—On the preparation of photographic plates in daylight, by Oswald Lohse.—On the origin of the flying power of bees, by Herr Dönhoff.—On the chlorophyll of Coniferæ germinating in the dark, by R. Sachsse.—On the behaviour of chlorophyll in the vine, by G. Briosi.—On the glycogen contained in muscles, by Th. Chandelon.

Journal de Physique, March.—On the dynamical theory of gases, by M. Violle.—On the effects of a jet of air sent into water, by M. de Romilly.—On the suspension and ebullition of water on a large-meshed tissue, by the same.—On the phenomenon of the black drop and its influence on observation of the transit of Venus, by M. André.—The persistence of impressions on the retina, various experiments with the projection-phenakisticope, by M. Gariel.—On Optography, by M. Kühne.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. x., fasc. iii.—On two recent works (on flagellation and the anatomical museum at Pavia) presented to the Institution, by M. Verga —On some rare alterations of the first formation of the uterus and its attachments; on a cause not yet confirmed of distonia, by M. Sangalli.—On a new defence of the theory of Melloni on electrostatic induction, by M. Cantoni.—On the divisibility of comets into minute parts, and on a dark spot found in the Milky Way, by P. Secchi.

Revue des Sciences Naturelles, tome v. No. 4.—On the so-called cladodes of Ruscus, by M. Duval-Jouve.—Study of a chromogenic bacterium in the water of steeping of flax (*Bacterium rubescens*, Ray Lankester [?]), by Prof. Giard.—On the development of the Anguillula Aceti, Ehrb., by M. Hallez.—Economical Aquarium, by M. Sabatier.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 22.—"On Stratified Discharges *v.* Stratified and Unstratified Forms of the Jar-Discharge," by William Spottiswoode, M.A., F.R.S. It is well known that if a Leyden jar be discharged through a vacuum tube, the discharge generally takes the form of an unbroken column of light, extending from the point of the positive terminal to the hilt of the negative, *i.e.*, to the extreme negative end of the tube; and that it shows no trace of either negative glow or intervening dark space. On the other hand, I have found, by experiments with a large Leyden battery, that if a tube having one terminal connected with the negatively charged coating of the battery and the other held beyond striking distance from the positively charged coating, the discharge in the tube will show a separation of the positive from the negative part by a dark intervening space. Under suitable circumstances of exhaustion it will also show striæ, in the same manner as when the discharge is effected directly with a Holtz machine, having the conductors either closed or open beyond striking distance (see Roy. Soc. *Proceedings*, vol. xxiii. p. 460). Again, I have found, with the same battery, that if the tube be connected—otherwise as before—and held at a distance less than at first, but a little greater than striking distance, a stratified discharge much more brilliant and more like that produced by a coil will be exhibited. The latter form of discharge appears to the unassisted eye as an unbroken column of light, but with a negative glow and dark space. A revolving mirror, however, resolves the column into a regular array of striæ, having a rapid proper motion towards the positive terminal.

With a view to examining the transition from the stratified to the unstratified form as closely as possible, a Holtz machine was employed, with a battery of one or more jars. The outside of this battery and one terminal of the tube were connected with the earth; and the inside and the other terminal were alternately connected with the positive conductor of the machine, so that the battery was alternately charged and discharged through the tube. The amount of charge was regulated partly by the distance through which the conductors of the machine were separated, and partly by the number of revolutions of the machine during which the charging took place.

The first object proposed was to ascertain whether a jar could be charged with so small a quantity of electricity as of itself to give a stratified discharge in a tube.

A number of tubes tried with various amounts of battery charge, but with the same surface, showed that, as the charge was increased, the head of the positive column advanced towards the negative terminal, the dark space became narrower, and the glow contracted in dimensions; and when the head of the column drew very near to the negative terminal, the glow, instead of covering the whole surface of the terminal, formed a small drop at the point. On still further increasing the charge, the drop withdrew to the hilt of the terminal; and finally, when it had completely retreated into the hilt, the continuous or true jar-discharge took place.

With a view to testing experimentally how far the effects here described were due to quantity, and how far to tension, the size of the jar was altered, all other circumstances remaining the same. It was then found that the maximum charge compatible with stratification was greater with a large than with a small jar.

As a further experiment in this direction, a series of jars were arranged in cascade, and it was found that the greater the number of jars so arranged the smaller the charge necessary to ensure a true jar-discharge. A charge insufficient to destroy stratification with one jar was sufficient to destroy them when more than one was used in cascade. These results point to tension rather than to quantity as the determining cause of the character of the discharge.

The duration of the stratified discharges observed throughout these experiments was exceedingly short, indistinguishable, in fact, from that of the true jar-discharge. When viewed in a revolving mirror they showed no sign whatever of prolonged duration, and we may thence conclude that, so far as our present instrumental arrangements extend, there is no inferior limit to the duration of discharge necessary for the production of striæ.

A comparison of the results here obtained with those detailed in Part ii. of these researches, shows that the phenomena produced by suitable disposition of the Leyden battery coincide with those produced by the induction-coil. With the coil it was found that (1) for a given electromotive force the column of striæ was shorter the larger the battery surface or strength of current used; (2) that the proper motion, when directed as usual towards the positive terminal, was more rapid the greater the electromotive force employed. With the Leyden battery it was found that (1) in order to maintain the same length of column with an increased surface the charge must be increased in a larger proportion than the surface; and (2) it was noticed that the striæ which when the tension was low were distinct and well separated, became more blurred as the tension rose, until they sometimes were blended into an apparently unbroken column of light. The presence, however, of the negative glow still showed that the true jar-discharge had not yet been reached.

Geological Society, April 11.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—John Robert Campbell, James Carter, William Radcliffe Ellis, William Hamilton Merritt, William Morgans, and Edmund Albert Parsick, were elected fellows of the society. The following communications were read:—On sandworn stones from New Zealand, by John D. Enys, F.G.S. The author exhibited specimens of sandworn pebbles from near Wellington, in New Zealand, and described their mode of occurrence.—The Bone-caves of Cresswell Crags, third paper, by the Rev. J. Magens Mello, F.G.S. In this paper the author gave an account of the continued exploration of these caves, and of the completion of the examination of the Robin Hood Cave, noticed in his previous communications. Five deposits could be distinguished in the Robin Hood Cave, namely, when all present:—1. Stalagmite, 2 feet. 2. Breccia, with bones and flint implements, 1 foot 6 inches. 3. Cave-earth, with bones and implements, 1 foot 9 inches. 4. Mottled bed, with bones and implements, 2 feet. 5. Red sand, with bones and quartzite implements, 3 feet. The most important discoveries were made in the cave-earth, and chief among these was a fragment of bone, having on it a well-executed outline of the head and neck of a horse, the first recorded discovery of any such work of art in this country. As the result of the exploration of these caverns, the author said it is evident that during the Pleistocene period, Derbyshire and the adjoining counties were inhabited by a very numerous and diversified fauna, the vast forests and pastures which extended far to the east and south offering a congenial home to the mammoth, the woolly rhinoceros, the hippopotamus, the Irish elk, the reindeer, the bison, and the horse, whilst among them the hyæna, the glutton, the bear, the lion, the wolf, the fox, and the great sabre-toothed *Machairodus*, roamed in search of prey; and that with these and other animals man lived and waged a more or less precarious struggle, amidst the vicissitudes of a varying climate, sheltering himself in the numerous caves of the district, which were already the haunts of the hyæna and its companions.—On the mammal-fauna of the Caves of Creswell Crags, by Prof. W. Boyd Dawkins, F.R.S. In this paper the author gave an account of the remains found in the caves explored by the Rev. J. M. Mello. He stated that the recent explorations had proved that the Robin Hood Cave was inhabited by hyænas, not only during the deposition of the cave-earth and breccia, but also during that of the red-sand clay underlying it, which had also furnished traces of the existence of man. After noticing the conditions of the fossil bones found in the caves, the author proceeded to remark upon the general results of the explorations with regard to their Pleistocene fauna, and concluded that there is no evidence from these or other caves in this country to prove that their faunas are either pre- or inter-glacial, and that we have no proof of the existence of pre- or inter-glacial man in Britain.

Zoological Society, April 17.—Mr. Osbert Salvin, F.R.S., in the chair.—The secretary exhibited and made remarks on some young Anacondas which had been produced dead by the large female Anaconda purchased on February 15.—The secretary exhibited some photographs of the young gorilla, now living in the Berlin Aquarium, and made some remarks on what, it now seemed certain, was an example of this ape, which was formerly living in one of Wombwell's travelling menageries, and was after its death transferred to the late Mr. C. Waterton's collection.—A letter was read from Mr. W. A. Willes, in which he gave an account of the success which had attended the endeavours of the Acclimatisation Society at Christchurch to introduce salmon into New Zealand from the United States.—A communication was read from Mr. W. A. Forbes, F.Z.S., containing a description of the peculiar organ known as the *Bursa Fabricii* in birds, and of its variations and modifications in the different genera of the class which he had had an opportunity of examining.—A communication was read from M. L. Taczanowski, in which he gave a list of the birds collected in North-Western Peru in 1876 by Messrs. Jelski and Holzmann. Amongst several new and interesting forms described was a new genus and species of Fringillidæ proposed to be called *Gnathospiza raimondii*.—A communication was read from the Rev. R. Boog Watson, containing some notes on the Madeiran Mollusc identified by the Rev. R. T. Lowe as *Achatina folliculus*.—A communication was read from Mr. E. P. Ramsay containing the concluding portion of his list of birds met with in North-Eastern Queensland, chiefly at Rockingham Bay.—A communication was read from Dr. Otto Finsch, containing a preliminary account of the birds collected during his recent journey in the North-Eastern part of Turkestan.—A communication was read from Prof. Owen, containing the description of a new species of extinct kangaroo of the genus *Sthenurus*, which he proposed to call *Sth. minor*, together with some remarks on the relation of this genus to *Dorcopsis*.—Mr. Edgar A. Smith read a paper containing descriptions of new species of South-American *Helicidæ* in the British Museum.—The Marquis of Tweeddale, F.R.S., gave descriptions of four new species of birds from the Indian region. These he proposed to name as follows:—*Trichostoma leucoprocta*, *Chrysococcyx limborgi*, and *Pomatorhinus austeni* (from Tenasserim), and *Brachypteryx buxtoni* (from Sumatra).—Mr. Osbert Salvin exhibited and pointed out the character of a new genus and species of bird of the family Ampelidæ, from Costa Rica, and proposed to call it *Phainoptila melanoxantha*.

Meteorological Society, April 18.—Rev. T. A. Preston, M.A., in the chair.—W. Morris Beaufort and Arthur A. Pearson were elected fellows of the Society.—The following papers were read :—On the meteorology of Mozufferpore, Tirhoot, for 1876, by C. N. Pearson, F.M.S. This year partook of the abnormal character of its predecessor, but in a different degree, and with widely different results. The total fall of rain was 57·69 inches, of which no less than 43·34 inches were registered in August, September, and October.—On the Diéthéroscope, by Prof. J. Luvini, of Turin. This is a new instrument contrived by the author for observing the changes of atmospheric refraction optically.—Improved form of thermometer for observing earth temperature, by G. J. Symons, F.M.S. This apparatus consists of an iron pipe driven in the ground to the required depth, and a small but very strong thermometer, the bulb of which is so protected that no change of indication occurs when the thermometer is drawn out of the tube for reading. The pipe is closed at the bottom by welding, and the point hardened so as to penetrate the soil with ease. For depths of 3 feet and under the thermometer is inserted in a light rod, but for all greater depths it is mounted in a short weighted stick attached to a strong chain.—Note on the degree of accordance of Mr. Glaisher's and the Kew thermometer standards, by William Ellis, F.R.A.S. This paper gives an account of the comparison of eight thermometers at the Royal Observatory which had been previously compared with Mr. Glaisher's and the Kew standard thermometers, and the result shows that the two standards are practically identical.

Entomological Society, April 4.—Prof. Westwood, president, in the chair.—Messrs. G. Harding, C. A. Briggs, and J. T. Carrington were elected ordinary members, and Messrs. E. H. Birchall, T. D. Gibson-Carmichael, and V. Cluse were elected subscribers.—The Secretary exhibited a collection of fine species of Lepidoptera from a place about twenty miles from Bangkok in Siam, forwarded to him by Mr. R. Garner, F.L.S., of Stoke-upon-Trent.—Mr. McLachlan exhibited a specimen of *Ophideres materna*, a brightly-coloured exotic species of *Noctuidæ*, given to him by Mr. R. H. Scott, of the Meteorological Office, with a note to the effect that it was taken at sea in lat. 25° 24′ S., long. 62° 10′ E. (the nearest land being the island of Mauritius, about 360 miles distant), by Capt. Raeburn, of the ship *Airlie*. The moth is a common Indian species, but is found also in Africa. A specimen was long ago received from Brazil, and Mr. Grote had recently noticed its occurrence in Florida. He also exhibited a cocoon and pupa of a species of *Cetoniidæ* (probably *Diplognathus silaceus*), from Cameroons, sent to Mr. Rutherford. The cocoon appeared to be formed of dark brown earth, but attached thickly to the exterior were oval, slightly flattened, deep black, hard bodies (each nearly five lines long by two broad), which he thought were probably the excrement of some rodent animal. Mr. Champion exhibited *Stenus kiesenwetteri* (hitherto only found in this country at Wimbledon), *Gymnusa brevicollis*, *Bembidium nigricorne*, and *Plociomerus luridus*, all from Chobham ; also *Philonthus cicatricosus*, from Shoreham.—Mr. Howard Vaughan exhibited (on behalf of Mr. Bidwell) a specimen of *Notodonta trilophus*, taken about the year 1867 by a lamplighter at Ipswich, who had it alive with several specimens of *N. ziczac*. It was only the second (authentic) capture of the insect, the first having been found at Saint Osyth, Essex, by Mr. Douglas.—The President read a letter he had received from Mr. B. G. Cole respecting the subject of Season-Dimorphism in Lepidoptera. He observed that from a number of eggs laid by *Ephyra punctaria*, those that emerged in July were of the spotted variety, while those which remained in the pupa state till the following May, in all respects resembled the mother. Mr. Cole referred to some remarks by Dr. Knaggs, published in the Entomologists' *Monthly Magazine* (vol. iii. p. 238) as bearing on the same subject. He considered it probable that the insects that were produced by a slow process of development would produce the May form (which might be considered the type), whilst those whose development was hastened by the heat and light of summer would produce smaller and less perfect insects. Mr. McLachlan alluded incidentally to the Lepidoptera brought home by the Arctic Expedition from the far north (82° N.), and said that the larvæ of most of those species, must, of necessity, require more than one season to acquire their full growth ; for the short and fitful summer was utterly inadequate for the full development in one season of most of the species, and furthermore, it was probable that the pupa state might habitually last several years.—The President read notes upon a strepsipterous insect, parasitic on an exotic species of Homoptera, (*Epora subtilis*, Wlk.), from Sarawak, accompanied by drawings illustrating the metamorphosis. He also read notes on the genus *Prosopistoma*, especially with regard to the species from Madagascar described by Latreille, of which he exhibited the types.—Mr. Cameron communicated a paper on East Indian *Tenthredinidæ*, and Mr. Butler a paper on the Lepidoptera of the Amazon Valley, collected by Dr. Trail in the years 1873–75.—Mr. Baly communicated descriptions of new species of *Halticidæ* and Mr. C. O. Waterhouse a monograph of the Australian species of the Coleopterous family *Lycidæ*.—Mr. F. Smith read descriptions of new species of the genera *Pseudomyrma* and *Tetraponera*, belonging to the family *Myrmicidæ*.

Institution of Civil Engineers, April 24.—Mr. George Robert Stephenson, president, in the chair.—The first paper read was on a deep boring for coal at Scarle, Lincolnshire, by Prof. Edward Hull, F.R.S. This boring was commenced about four years ago by a local company, to test the presence of coal in the neighbourhood of Lincoln—Mr. J. T. Boot, of Mansfield, being the engineer—and had been carried out by the Diamond Rock Boring Company. The total depth attained was 2,030 feet ; but as this depth was insufficient for the object in view, it was desirable that the bore-hole should be carried further down. The following formations, with their approximate thicknesses, had been passed through :—

	Depths. Feet.		Thickness. Feet.
Alluvial Strata	1 to	10 ...	10
Lower Lias Clay and Limestone	10 ,,	75 ...	65
Rhœtic Beds	75 ,,	141 ...	66
New Red Marl and Sandstone	141 ,,	1,500 ..	1,359
Permian Beds	1,500 ,,	1,900 ...	400
Carboniferous Strata	1,900 ,,	2,030 ...	130

Although the carboniferous strata had been reached, the cores brought up were of so peculiar a character as to leave it uncertain to what portion of the carboniferous formation they belonged ; and, as the question of the eastward extension of the Yorkshire coal-field was one on which a boring at this spot was calculated to throw much light, it was important, both in an economic and in a scientific point of view, that it should be continued until something definite had been determined.—The second paper read was on street tramways, by Mr. Robinson Souttar.

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THURSDAY, MAY 10, 1877

MATHEMATICS IN AMERICA

Elements of the Differential and Integral Calculus, by a new Method, founded on the True System of Sir Isaac Newton, without the Use of Infinitesimals or Limits. By C. P. Buckingham. (Chicago: S. C. Griggs and Co., 1875. 343 pp.)

Elements of the Infinitesimal Calculus, with Numerous Examples and Applications to Analysis and Geometry. By James G. Clark, A.M. (Ray Series. New York: Wilson, Hinkle, and Co., 1875. 441 pp.)

On a New Method of Obtaining the Differentials of Functions with Especial Reference to the Newtonian Conception of Rates or Velocities. By J. Minot Rice and W. Woolsey Johnson. (New York: D. van Nosbrand, 1875. 32 pp.)

AN American writer who had exceptional opportunities of contrasting the methods of mathematical teaching adopted in his own country with those which obtained at Cambridge twenty-five years ago, strongly condemns the Transatlantic system, and leads his readers to infer that the attainments of the ordinary graduate in this particular branch of study were only on a par with those of a fairly trained schoolboy here. It may be supposed, then, that not many of the students ventured upon the difficulties of the calculus. Indeed, he writes that "at Yale where the course used to be thought a very difficult and thorough one, the Differential was among the *optional* studies at the end of the third year." (Bristed: "Five Years at an English University," vol. ii, pp. 94, &c., 1852.)

We are not in a position to say that all this has been changed in the interim, but among many evidences of the increased interest taken in mathematical studies we may surely refer to the three works now before us. All three give evidence of careful study and honestly grapple with the difficulties which beset the learner at the very threshold of his inquiries. De Morgan long ago wrote that "it is matter of common observation that any one who commences the study, even with the best elementary works, finds himself in the dark as to the real meaning of the processes which he learns, until, at a certain stage of his progress, depending upon his capacity, some accidental combination of his own ideas throws light upon the subject." The authors of the third work under review refer to D'Alembert's precept, "Allez en avant, et la foi vous viendra."

Mr. Buckingham takes as his fundamental idea of the conditions under which quantity may exist to be that we must not consider it only as *capable* of being increased or diminished, but also as being actually in a *state of change*. "It must (so to speak) be *vitalised*, so that it shall be endowed with *tendencies* to change its value; and the rate and direction of these tendencies will be found to constitute the groundwork of the whole system. The differential calculus is the SCIENCE OF RATES, and its peculiar subject is QUANTITY IN A STATE OF CHANGE."

Conceding to Leibnitz the honour of being the first to construct a system of rules for the *analytical machinery* of the science, he will not allow that he ever got beyond the ancient conception of the conditions of quantity. "The only original birthplace of the fundamental idea of quantity which forms the true germ of the calculus was in the mind of the immortal Newton."

An introduction of thirty-six pages discusses the method of Descartes, the infinitesimal method (the results of which are true, while the method is false—"true results *not* because its principles are true, nor because its errors are small, but because they are, whether great or small, exactly equal, and *exactly cancel and destroy each other*. . . . the system is but a mere artifice."), the method of limits (here our author discusses Lemma I., Book I. of the "Principia," considers Newton's defence of the Lemma, and the opinions of Comte, Lagrange, and Berkeley, and points out what he believes to be the fundamental errors of this method and of the infinitesimal method). What is called the true method of Newton is then treated of. Referring to Newton's letter to J. Collins (December 10, 1672), he says that the theory on which Newton formed *his* method of fluxions is contained in the second Lemma. The lemma is given in full and discussed. "It is to be remarked that the doctrine of *limits* is nowhere hinted at, but the results are direct, positive, and substantial." We cannot tarry longer over this matter, but in connection with this point refer to De Morgan's "On the Early History of Infinitesimals in England" (*Phil. Mag.* November 1852). Prof. Clifford, too, if our recollection of an oral communication be correct, puts this lemma prominently forward in his (? unpublished) "Foundations of the Differential Calculus and of Dynamics." In the work itself we have the calculi (differential and integral) applied to the subjects which usually find a place in similar treatises. There is an appendix of thirteen pages on geometrical fluxions. Many examples are worked out, but the merit of the work does not lie at all, we think, in this direction, but altogether in the numerous discussions which are to be found in almost every chapter.

Mr. Clark's work has been exceedingly well printed, the type is very clear, and the paper good. This treatise, too, is written with a view to remove "all grounds for that feeling of uncertainty which often possesses the student at the very outset, and from which he rarely finds it possible to extricate himself." Much space is given to an exposition of the Doctrine of Limits—the work being founded mainly on that by Duhamel. A large number of examples have been taken from English treatises (Hall, Walton, and Todhunter). Rather more ground is covered in this treatise than in the former; in neither, however, have we any discussion of maxima and minima of functions of more than two independent variables, nor of methods of changing the variables in multiple integrals. Here a few pages are devoted to definite integrals and to differentiation and integration under the sign $\int$. Seven chapters are devoted to the elementary parts of the theory of differential equations. The work, though it does not reach the level of the like works by Messrs. Todhunter and Williamson, is yet a compact and fair elementary treatise.

The third work on our list is a revised edition of a paper read before the American Academy of Arts and Sciences, January 14, 1873. It is the authors' intention to publish a text-book in accordance with the plan adopted in this pamphlet. The objects are, "first to present a new method of deriving the differentials of functions by means of their algebraic characteristics with the aid of a few elementary properties easily established, and secondly to show that the method of rates or fluxions may be advantageously used for the purposes of instruction, and the use of infinitesimals, limits, and series entirely avoided until the student is well grounded in the elements of the calculus."

The first seven articles under the head "the Newtonian Method of Fluxions," treat of the methods in general use at the present time, and contain extracts from Todhunter, Lacroix, Carnot, and Cournot, especially directing attention to the positive advantages of the Newtonian method, as set forth by the last-named writer. The next six articles are occupied with the "Proposed Method of treating the Differential Calculus."

The remaining half of the pamphlet is given to algebraic and transcendental functions. It would be very interesting to lay before our readers an account of the ingenious methods adopted by our authors, but it would take up too much space. Some idea of the original paper (and there are no great differences, we fancy, between the two publications) can be got from an account of it furnished by Mr. J. W. L. Glaisher, F.R.S., in vol. iv. (pp. 58-64) of the *Messenger of Mathematics* (1875).

Altogether, on a review of the three books before us, we anticipate that mathematical studies are destined to occupy a more prominent position in the American colleges and schools than they have in the past.[1]

R. TUCKER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Visibility of the Ultra-Violet Rays of the Spectrum

It is well known how surprisingly rich in rays of high refrangibility the spectrum of the electric arc formed between carbon points is, above that of all other artificial flames ; and also how far beyond the ordinarily discernible rays of the solar spectrum, formed by a glass prism, light may be traced by eyes carefully shielded, and raised to the highest susceptibility to perceive it. The name of "lavender-grey" rays has been given to them from a colour of that tint which they are considered to possess, but the term "ultra-violet," which is more commonly used, betrays perhaps a lingering doubt as to the sensible existence of another order of coloured rays in the spectrum distinct from the violet and superior to it in refrangibility, which has yet been detected by very close and careful observation. All doubts of this kind, which from want of sufficient acquaintance with that part of the spectrum I have myself been hitherto rather too prone to entertain, have lately been quite dispelled by frequent observations of the spectrum of the electric arc between carbon points thrown by a quartz prism on a white paper screen. The violet end of the spectrum terminates rather abruptly, or at least beams with bright colour that fades off very quickly ; and in the dark space beyond it three more refrangible bright bands are visible with more or less distinctness. The middle one of the three is the brightest, and from its perfect freedom from colour, in which it contrasts most remarkably with the strongly-tinted light-belt near it, and its distant separation from the violet termination of the continuous spectrum, I at first hastily ascribed it to a "ghost," or faint image of the slit, indirectly refracted and reflected through the prism, and thrown with the spectrum on the screen. That it is not so, however, is shown by the action of these three lines on fluorescent substances, of all of which that I have tried they excite the fluorescence most strongly, especially that of fluorescin, eosin, rose of Magdala, and other solutions, all of which alike show these rays to be clearly defined and well-insulated spectral bands. In particular, the solutions of æsculin, paviin and amido-terephthalic acid are only excited by these "ultra-violet" lines, and not by any rays in the spectrum of lower refrangibilities, clearly showing that the vigorous fluorescence that they produce is not the effect of any ordinary light-beam of common refrangibility, irregularly transmitted by the prism, but that they are well-marked rays, probably of carbon, in the spectrum of the voltaic arc. The light of the middle band is bright enough to be easily reflected and examined separately from the rest of the spectrum on a white screen, where it is so nearly grey or colourless that it scarcely admits of being ranged in any colour scale, although the name "lavender-grey" perhaps expresses better than any other term the faintest possible tone of colour which, if it exhibits any at all, this almost purely neutral, or steel-grey band of rays may possibly be suspected to possess. It is a little more strongly absorbed by ordinary plate-glass than the neighbouring violet bands ; but it remains visible in the spectrum of the arc formed by an ordinary flint glass prism, though much spread out and enfeebled by the dispersion, which greatly exceeds that of a quartz prism of the same refracting angle. It is perhaps for this reason that it is not perceptible in the spectrum of the arc as usually projected on a screen with a fluid-prism of bisulphide of carbon, but if the latter is replaced by benzine, which disperses the light less than flint glass, it forms a pretty conspicuous grey band in the spectrum. The other two lines or bands are so much fainter than the principal one, that in general they can only be found with the help of a fluorescent substance, and where so faintly visible it is not possible to speak positively as to their colour. The less refrangible is very near the violet termination of the spectrum, and when well seen it shares its violet tinge ; the more refrangible one is nearly as far beyond the principal grey band as this band is beyond the margin of the violet, and as far as its weak light allows one to distinguish, it is of the same colour as the brightest band. In order to determine their positions, some measurements were made of metallic lines, and of the spectra of sodium, lithium, thallium, and strontium in the arc, with the result that the violet part of the continuous spectrum extends to the closely-neighbouring positions of the hydrogen line Hδ (h), the potassium flame-spectrum line Kβ, and the last violet line in the arc spectrum of a salt of strontium. The first faint outlying ray occupies nearly the position of H_1 in the solar spectrum, and it is therefore in the true violet region of the spectrum, as its colour faintly indicates. The prominent grey line begins with its brightest edge about as much further beyond this, from the end of the violet field ; and becoming weaker from there, it is about twice as broad as the distance between the two Fraunhofer lines H, its mean position in the spectrum being nearly as far from H as H is from h, reckoning the distances as they would be seen with the quartz prism and with solar light. The third faint line occurs about as far again from the violet as this band ; and it lies at least as far beyond H as the distance between G and H in the solar spectrum. Yet it is visible there by glimpses, like the first faint member of the group, which it does not yet by any means surpass in the strength with which it produces fluorescence.

If any fresh proof was needed of the characteristic grey appearance of visible rays in this portion of the spectrum it was soon presented in one of the metallic spectra used to determine their positions. The spectrum of mercury exhibited a bright line (beautifully distinct when a fluid prism of benzine was used with a refracting angle of between 50° and 60°), much brighter than the principal grey carbon band, considerably more refrangible, and of the same tintless, and perfectly neutral grey appearance. Though not so distant from the violet as the most refrangible faint carbon line, it is yet according to the best measurements and identifications that were made, about as far beyond H_1 in a prismatic spectrum as H_1 is from G ; and radiation of this high degree of refrangibility is evidently strongly luminous, when sufficiently intense, with homogeneous grey light characteristic of this region, and contrasting conspicuously in its appearance with the zone of violet colour, which often borders closely upon it in electric spectra.

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[1] We are confirmed in our views on this subject by a perusal of Dr. Sylvester's characteristic address at the Johns Hopkins University on Commemoration Day, February 22, 1877.

The wave-lengths of the bands, and other positions in the spectrum, roughly obtained, by which it may be possible to identify some of them in photographic spectra, although open to some uncertainty from the inconstant length and strength of the arc of flame in the electric lamp, which confused and shifted some of the comparison lines, were as follows :—

Electric Arc with Carbon Poles.	Wave-lengths.
End of the violet field (*h*, Kβ, and last violet line in arc-spectrum of strontium, 4,080 4,100)	About 4,100.
First light-band ; faint violet-grey (H_1, 3,968 ; H_2, 3,933)	About 4,000–3,950.
Second do., strong grey band	About from 3,900 to 3,800.
(Strong grey line of mercury	About 3,700).
Third do., faint, grey	Between 3,600 and 3,500 (?).

Other metallic arc-spectra probably present lines in this portion of the spectrum, of which it would be interesting to examine the apparent brightness and the colours. At present the most conspicuous that I have met with is the grey line of mercury, which is brighter and more refrangible than the grey band of the electric light between carbon points. Its very advanced position in the spectrum, and the absence, or negative appearance of colour in its pretty bright light, both taken together seem to indicate very clearly that the grey or "lavender-grey" division of the spectrum fully equals in extent, when seen prismatically, the violet, the indigo, the blue, or any of its other better known, and much more ordinarily visible companion regions, the seven Newtonian colour-spaces of the spectrum. A. S. HERSCHEL

College of Science, Newcastle-on-Tyne, April 26

Pele's Hair

I HAVE read with great interest Mr. Moseley's description of Pele's Hair in NATURE (vol. xv., p. 547), since it furnishes information which I was most anxious to obtain. It seemed to me extremely probable that the analogy between Pele's Hair and the artificial furnace products would not be confined to the long fibres, and I did my best to ascertain whether irregular glassy spherules occurred along with the natural products. I was unable to obtain specimens for examination, but paid a visit to my friend Mr. J. G. Sawkins, F.G.S., who had explored the crater and collected the hair, in order to ask him whether he had ever noticed the pear-shaped spherules. He told me that he had never seen anything but the glassy fibres. I must say that I felt very much inclined to believe that the specimens usually collected are the material which has been blown some distance by the wind, consisting of the fibres from which most of the spherules have been broken. Mr. Moseley's letter in NATURE, and another which he has kindly addressed to me, make me believe that the analogy between the artificial and natural products is more complete than I was able to ascertain before Mr. Moseley's observations were published. In conclusion I would say that these facts in no way invalidate my arguments in respect to meteorites. They merely show that in certain cases the glassy volcanic spray, like melted furnace-slag, can to some extent collect into more or less imperfect spherules, so far analogous to those in meteorites as to indicate how those remarkable bodies were formed, but these spherules are accompanied by many fibres, which I have never yet seen in meteorites. This difference appears manifestly to depend on the difference in the temperature of the space into which the glassy spray was thrown. If the temperature of the air in the crater of Kilauea were equal to that of the melting point of the lava, we should almost certainly find, as in meteorites, many spherules and no hairs. H. C. SORBY

The Critical Point of Carbonic Anhydride

As the writer is not aware that any attempts have hitherto been made by others to exhibit to a large class the phenomena attending the passage through the critical point of a liquid in the presence of its gas, he is of opinion that the following account of a method which he has found very successful may be of interest :—

Dr. Andrews's apparatus for the study of gases was employed in the experiments, and the image of the tube containing the carbonic anhydride was projected on a screen by means of the oxy-hydrogen lime-light and a solar microscope which magnified it about 120 diameters. Dr. Andrews's apparatus consists of a thermometer tube filled with carbonic anhydride and a second tube filled with dry air, which serves to measure the pressure applied. The lower ends of these tubes dip beneath the surfaces of mercury contained in test-tubes, which are suspended in strong copper cylinders communicating with each other, and filled with water, which presses on the mercury in the test-tubes. The pressure is applied by means of long steel screws which pass through the bottoms of the cylinders. For the filling and mounting of these tubes the University of Cambridge is indebted to the kindness of Dr. Andrews. The lantern was supported on three screws, which allowed it to be raised or lowered so as to bring any required portion of the thermometer tube into the field of view of the microscope. The best height for the lantern was found to be such that the top of the tube was rather less than half an inch above the axis of the microscope. When the oxygen was turned on, the radiation from the lime cylinder raised the temperature of the portion of the tube within the field of view above the critical point in little more than a minute, so that no other source of heat was required ; but when the oxygen was turned off the tube cooled through several degrees.

The best method of performing the experiment is as follows :— The lantern having been properly adjusted, the gas should be lighted, the oxygen turned on, pressure applied until the surface of the mercury comes into the field of view and the microscope focussed so as to give a distinct image of this surface. The pressure should then be relieved and a blast of cold air from a bellows or gas bag directed against the tube. This will cool it considerably below the critical point. The pressure should then be increased, the cold blast being continued until the inverted image of the concave surface of the liquid reaches the middle of the field of view appearing as a broad dark line possessing considerable curvature, and, of course, concave downwards. The focussing screw should now be finally adjusted so as to give the best image of this surface, and the blast then stopped. Immediately after cutting off the blast the operator must obtain command over one of the screws and carefully increase the pressure as the temperature rises so as to keep the image of the liquid surface just above the centre of the picture on the screen. As the temperature and pressure increase the broad image of the surface becomes narrower and less concave until, as the temperature approaches the critical point, the line becomes very thin and faint and loses its curvature altogether ; it then seems to explode into mist and vanish as the critical point is reached. Another half turn of the screw then produces the well-known clouds or flickerings, which are best seen on the screen somewhat below the middle of the field, and in a few more seconds all is steady. More pressure should then be applied until the mercury reaches the axis of the microscope, but no change of state will be manifested by the carbonic anhydride.

It is important that the image of the surface of the liquid should not be below the centre of the field of view on the screen, for if the liquid stand in the tube above the axis of the microscope, since the greatest heat is there concentrated, bubbles of gas are liable to be formed within the liquid and to damage the continuity of the surface. Perhaps the flickerings may be due to unequal temperatures at different parts of the tube, so that some are just above and others just below the critical point. The mode of propagation of a sound wave through a substance just at the critical point may be an interesting subject for inquiry.

After passing the critical point the blast of air should be directed against the tube for about a minute. This will, of course, cause the image of the mercury to descend upon the screen, but no change of state will appear to take place in the carbonic anhydride. The pressure should then be rapidly diminished by turning the screws, when a violent ebullition will be seen, showing that the whole of the contents of the tube had assumed the liquid state during the cooling, the gas having passed at the critical point into the liquid without breach of continuity, so that no indication of a change of state was apparent on the screen. On increasing the pressure and continuing the blast the liquid surface will again appear, and the experiment can be at once repeated. WM. GARNETT

Cavendish Laboratory, Cambridge

Floating Cast Iron

HAVING read the interesting letter on this subject which appeared in NATURE (vol. xv., p. 529), I send the following copy of notes of experiments which I made about three years ago.

Several pieces of pig iron were put into a ladle (holding about one ton of metal); these at *first sank*, and a rush of hot metal took place upwards; after a few seconds the pieces of pig iron appeared *floating, with very little of their bulk* above the surface of the molten metal. A piece of flattish metal of irregular shape floated with a small portion alone of its corners above the surface; it was close to side of ladle. Pieces of flat cast-iron bars, 20″ × 2″ × 1″, were carefully placed on surface (the latter being well skimmed); they floated without going below the surface. One of these pieces, which was put in *end on*, kept in this position for a few seconds, with its upper end above the surface; the other end then came up and floated on its flat side. In some cases a sharp crack was heard when the metals touched, and a white flame on one occasion burned like a gas jet from the side of one of the pieces.

The surface of the molten metal was in constant motion due to the currents within its mass, and showed the variegated texture or "break" peculiar to this condition of the metal. From notes of an experiment which I arranged for, but did not see carried out, I find that a cast-iron ball of about 2¾″ diameter, when lowered by a fine wire upon a well-skimmed surface of molten cast iron, *disappeared completely* at first, and then in a few seconds rose and floated with about half an inch diameter of surface exposed; it was then raised from the metal, when it showed a red glow on the lower part. It was again lowered, but now *did not sink*, but floated with about *twice* the surface exposed, as on the first experiment.

Different views are held as to the behaviour of cast iron when passing from the molten to the hot solid state, and finally to the cold (or ordinary temperature) state.

Some hold that the molten metal, on solidifying, expands like water passing into ice, and that it retains this expansion to such an extent that the cold solid is specifically *lighter* than the molten metal. Others hold that no such expansion takes place, and that finally the cold solid is specifically *heavier* than the molten metal. A third view is that the molten metal on solidifying *expands*, and that it then *contracts* during cooling, until it reaches ordinary temperature, when through the cooling it is specifically *heavier* than in the molten state.

From the fact that in foundry practice the linear contraction is taken at $\frac{1}{96}$th part, there can be little doubt that the finally cooled solid is specifically *heavier* than the molten metal; again, from the sharpness of form of iron castings and other circumstances, expansion appears to take place on solidification.

The above experiments, I think, favour this latter view, as the floating took place more readily with small than with large pieces, partly due to their relative bulks and surfaces.

A probable explanation, in part at least of these phenomena, I think, is that the cold metal, when at first put in, is specifically *heavier* than the molten metal, but owing to the great heat around it (over 2,000° F.) it is rapidly heated, and consequently *expanded*, and when sufficient volume has thus been obtained it floats. It is evident that small pieces, being more readily heated, may remain floating, whilst heavy pieces, whose volumes are larger in proportion to their surfaces, will take longer to heat, so as to induce the required change of volume, and may therefore at first sink, remaining below the surface till sufficiently expanded to rise and float. The experiment with the ball bears out this well, as, being a sphere, its surface was a minimum.

These experiments appear to corroborate very well those of your correspondent.

The following experiments which I lately made with lead may be of interest:—

An ingot of lead of 14 lbs. weight was placed on the surface of about 160 lbs. of molten lead; it at once melted. After allowing the metal to cool a little, an ingot was carefully placed on the surface, when it immediately sank, bubbles rising up to the surface; it was heard to strike the bottom of the ladle. Another ingot was tried; it also sank, and could be felt at the bottom (these ingots were cast from the lead in the pot). A small solid piece was cast of about 1½ lb. weight, which also sank. Pieces of *sheet lead* were rolled up and placed on surface; these *floated*: the contained air and great surface in the latter would account for this.

These latter experiments with lead correspond very well with those of your correspondent with zinc. W. J. MILLAR

Glasgow

Yellow Crocuses

IN my garden the sparrows do not touch the crocuses. In that of a friend, some miles off, they attack the yellow ones exclusively. I address you chiefly to report a fact related to me by the vicar of a neighbouring parish, whose garden is infested with mice. He tells me that for some time he thought he could not grow crocuses at all, as the mice destroyed the corms, discovering and digging down to them, even when there was no trace of the plants on the surface. At last he found that they did not attack the purple crocus, and on his planting the edge of a long border, with alternate clumps of yellow and purple crocuses, the mice almost entirely destroyed all the clumps of yellow, but left the purple untouched. Possibly the purple plant possesses some acrid or bitter taste, rendering it nauseous to animals—the corms to mice, the flowers to sparrows and other birds.

Newton-le-Willows, May 4 THOMAS COMBER

Hog-Wallows and Prairie Mounds

IF Mr. Williams is right, and the "hog-wallows" are simply American cousins of our "eshars" or "kames," is it not reasonable to credit that "atmospheric erosion" to which Prof. Le Conte attributes the formation of the former with a much more important influence upon the shapes of the latter than British geologists generally seem disposed to accord to it? It is very difficult to conceive that mounds of loose sand and gravel, whether in valleys or on plains, should have retained the impress of the glacier or the iceberg throughout the vast time that must have elapsed since these phenomena entirely disappeared. And if it be conceded that these mounds have been modified in any degree by subaërial denudation, it will be found difficult to limit the extent to which they are indebted to it for their present forms, or indeed to deny that it alone may have shaped them.

Newport, Fife, May 7 JAS. DURHAM

A "Golden Bough"

IN the gardens of New College, Oxford, there is a fine avenue of horse-chestnut trees, most of which have had some of their lower limbs lopped off, followed by the usual crop of abundant smaller shoots around the original bough. In one tree, however, with respect to one severed branch, these resultant shoots bear, year after year, not green, but pale yellow leaves, the summer through—

> "Primo avulso non deficit alter
> aureus, et simili frondescit virga metallo."

It would be interesting to know of other instances of such a veritable "golden bough," and whether any explanation can be given of chlorophyll so remarkably failing to develop its blue-green constituent under no obviously peculiar circumstances. It seems a strange anomaly to find an apparent case of host and saprophyte in one. HENRY T. WHARTON

SPONTANEOUS GENERATION

ON Friday evening last the Rev. W. H. Dallinger made an important communication to the members of the Royal Institution on "Recent Researches into the Origin and Development of Minute and Lowly Life-forms; with a Glance at the Bearing of these on the Origin of Bacteria." Biological Science to-day presents us with a magnificent generalisation; and that which lies within it and forms the fibre of its fabric, is the establishment of a continuity—an unbroken chain of unity—running from the base to the apex of the entire organic series. But does this imposing continuity find its terminus on the fringe and border of the organic series, and for ever pause there? or, can we see it pushing its way down and onward into the unorganised and the not-living, until all nature is an unbroken sequence and a continuous whole? That such a sublime continuity may be philosophically hypothecated is to be believed. But that data have been presented to us demonstrating how and by what path the inorganic passes to the vital, the living into the not-living, may be denied. The properties of living matter distinguish it absolutely from all other kinds of things, and the facts to-day in the hands of the biologist furnish us with no link between the living and the not-living. This is an inference which has been fiercely disputed.

But what are the nature of the proofs relied upon to establish the "spontaneous" or not living origin of living things? They were chiefly thermal experiments upon the lowest septic organisms, without an attempt to discover what was their life history, and whether they propagated by germs or not. It was argued that the adult organisms being killed at a given temperature much below the boiling point of water, if an infusion were boiled with every possible precaution, and whilst boiling hermetically sealed, and after a lapse of time on opening the vessel the organisms were found in a living state, they must have arisen *de novo*. That is, the not living would have produced the living; that this method is useful, and that it must be pursued in an exhaustive inquiry into the whole subject, must be freely admitted. But that it is the best, or at least the only, method of inquiry for the biologist we may gravely doubt.

Ten years ago Mr. Dallinger determined to endeavour to work out by actual microscopic observation the life history of some of the lowly life forms.

After four years spent in preparation he commenced his work in conjunction with Dr. Drysdale, the plan needing two observers. A characteristic feature of the work was that each set of observations should be made absolutely continuous, so that nothing should have to be inferred. An arrangement was made by which the little drop of septic fluid containing the objects under examination should be free from evaporation, and very high powers were employed. The largest adult objects included in the examination were the one-thousandth of an inch, the smallest adults were the four-thousandth. Six forms altogether were selected, and, by long, patient, and unbroken watching, their whole history was worked out. While reproduction by fission seemed at first to the observers to be the usual method, prolonged research made known that spores were produced. These were so small that a magnifying power of 5,000 diameters was needed to see them as they began to grow. The glairy fluid from which they developed seemed at first homogeneous, and it was only when growth set in that the spores became visible. All that could be learnt about the origin of the glairy fluid was that a monad, larger than usual, and with a granulated aspect towards the flagellate end, would seize on one in the ordinary condition. The two would swim about together till the larger absorbed the smaller, and the two were fused together. A motionless spheroidal glossy speck was then all that could be seen. This speck was found to be a sac, and after remaining still for from ten to thirty-six hours it burst, and the glairy homogeneous fluid flowed out. The young spores that came into view in this were watched through to the adult condition. Bearing on the subject of spontaneous generation, this fact was learnt, that while a temperature of 140° F. was sufficient to cause the death of adults, the spores were able to grow even after having been heated to 300° F. for ten minutes. Can it be philosophical, Mr. Dallinger asked, with the life history of bacteria still unknown, to assume it as a different method of propagation? Some experiments based on Prof. Tyndall's use of the electric beam to test optically pure air were made. The remains of infusions known to contain certain spores were diffused through glass tubes, in which were placed vessels with fluid. Monads always appeared in the fluids, but when after the air in the tubes had been allowed to purify itself by settlement, fresh fluids were introduced, no monads appeared. That there is no such thing as spontaneous generation of monads seems quite clear, and when bacteria are in like manner studied, there can be hardly a doubt the same law will be found to hold good with them.

GREAT GUNS

IT is natural that at the present time great interest should be taken in all efforts to improve, that is, to render more destructive, our implements of war. Even since the last war on the European field great advances have been made in this direction; and, as our readers know, one of the largest guns ever constructed is at present on its trial in this country. Some months ago experiments were made with what is known as the 81-ton gun, the invention of Mr. Robert Fraser; the gun was sent back to Woolwich for some alterations to be made, and on Friday the experiments were resumed at Shoeburyness on a larger scale. On the previous occasion the gun was loaded with 370 lbs. of powder, and threw a blind Palliser shell against the target. This target is of enormous strength, as strong and firmly founded as the ingenuity of engineers can make it. It is formed of four plates of the best rolled iron, each plate being 8 inches thick, and 5 inches of solid teak filled up each of the three intervals between the four plates. The 32 inches of iron and 15 inches of teak thus placed are solidly screwed together by bolts 3 inches in diameter, the whole forming, as far as scientific engineers and artillerists could construct it, an apparently impenetrable and immovable mass. To secure the target still more, iron plates were placed on the top and at the side, those at the side being strutted against the target with heavy timbers; and the supports at the rear of the target, to hold it up, as it were, against any blow, were of the like solid and substantial character.

The target stood at 120 yards' distance from the gun. On Friday the charge of powder was 425 lbs., and the weight of the blind Palliser shell 1,700 lbs. At the base of the latter was an expanding copper-ridge, known as the "Lyon" gas-check, which in the explosion would expand and fill the rifling, thus enabling the full energy of the exploded powder to be utilised.

After the gun was fired, by electricity, it was examined and found to have worked admirably; it had run back 55 feet on its tramway, which rises slightly, and had run down again to the firing-point. The shot was found not only to have penetrated three plates and the teak intervals to all four, as on the previous occasion, but to have bulged out the fourth plate some 15 inches from its normal position. The last plate was, moreover, broken across, the edges of the broken part gaping wide, and showing the head of the shot, which had thus penetrated further in distance than the 47 inches of iron and teak of the target. The powerful framework behind the target was greatly shaken. The shot itself had "set-up," *i.e.*, closed towards the head with the enormous energy, the rear-part, the gun-metal studs, and the copper gas-check, crumbling into pieces. The initial velocity of the shot, as registered by M. Le Boulenger's invention of wires with electric communication was 1,600 feet a second, and the striking velocity 1,585 feet. The mean pressure on the gun was found to be 20 tons, the interior remaining quite unaltered.

Herr Krupp, the well-known Prussian artillerist, has been devising a weapon even more formidable than that whose power of destruction was shown on Friday to be so immense. The Fraser gun is of wrought iron, but the new Krupp gun is of cast-steel, both being very nearly of the same weight, though the latter has the great advantage over the former of being a breech-loader. The length of the gun, including the breech-piece, is 29 feet 6 inches, the breech-piece itself being 6 feet 4 inches in length. The calibre of the gun is 15¾ inches. The weight of the projectile will be 750 kilogrammes, or 1,650 lbs., and the powder charge will be 396 lbs. The external diameter of the Krupp gun, independently of a narrow strengthening ring at the extreme rear, is 5 feet 10 inches, that of the Fraser gun being 6 feet. The core of the Krupp gun is a steel tube in two lengths, upon which four steel rings overlap, rising in steps from a point between the muzzle and the trunnions, and accumulating in the thickness towards its rear. These more massive rings are irrespective of the narrow strengthening ring over the powder chamber. The external diameter of the gun at the muzzle is 2 feet 3½ inches.

In common with the other large Krupp guns, the rifling of the new weapon is on the polygroove system, the elongated projectile being rotated by means of the gas-check. The velocity anticipated from the projectile fired from the Krupp 80-ton gun is 473 metres per second at the muzzle, or 1,552 feet, producing an energy of 27,543 foot tons, equal to 556 foot tons per inch of shot's circumference.

But this is not all. Should the demand arise, the great Prussian gun-maker has a design already prepared for a gun of 124 tons, to be made on the same plan as the one just described. The larger weapon would have a calibre slightly exceeding 18 inches, and would throw a steel shell weighing 1,000 kilogrammes, or a chilled iron shell of 1,030 kilogrammes. The weight of the projectile would therefore be practically a ton, and the charge of powder will be probably about 500 pounds.

SUSPECTED RELATIONS BETWEEN THE SUN AND THE EARTH[1]

II.

IN my last article I endeavoured to show that as a matter of fact there is an intimate connection between the physical state of the sun's surface and the diurnal range of the magnet freely suspended at the Kew Observatory. It was suggested that this relation might be that of cause and effect, inasmuch as the variations of spotted area exhibited in Diagram B invariably precede the corresponding variations of magnetic range.

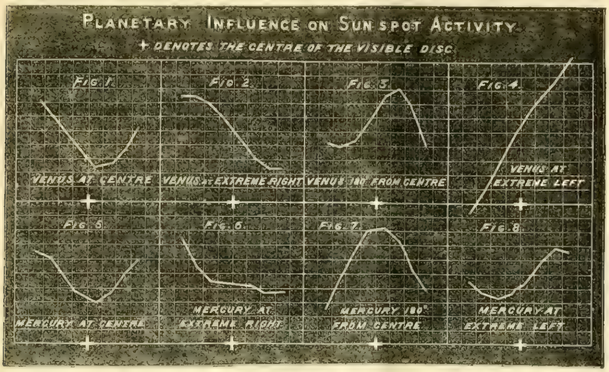

Diagram C.

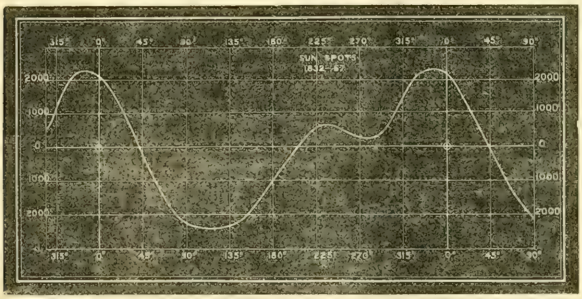

Diagram D

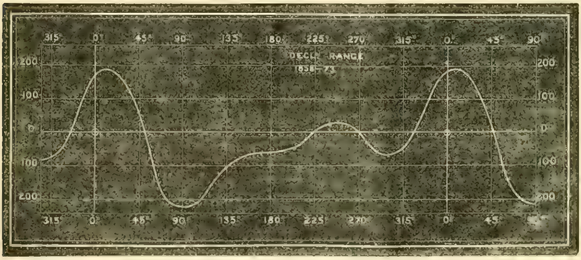

Diagram E.

It cannot, however, with propriety be said that sun spots are the cause of magnetic oscillations, for it has been pointed out by Mr. J. A. Broun that even when there are no spots on the solar surface the magnet has yet a very considerable range in its daily oscillations. Then, on the other hand, the spectroscopic researches of Mr. J. N. Lockyer and others leave us little room for doubting that there may be vast solar activity without sun-spots, while, however, spots will probably make their appearance when the disturbance of the sun's surface is very great.

In fine, sun-spots will probably only afford us a rough means of estimating solar activity just as rainfall might give us a rough means of estimating the meteorological activity of a district of the earth. Is it not possible that sun-spots are in truth a species of celestial rainfall?

Be this as it may, it is evident that, inasmuch as sun-spots exhibit a recurring period, we are entitled to say there is a period of this kind in the meteorology of the sun. The interesting question then arises, What can be the possible cause of such a period? This question has been discussed by Mr. Warren De La Rue and those associated with him in his solar researches. The theory propounded by these observers is that the planets are in some unknown way concerned in the production of spots. In their paper, which will be found in the *Proceedings* of the Royal Society for March, 1872, they make the following remark :—

"It might be said, 'How can a comparatively small body like one of the planets so far away from the sun cause such enormous disturbances of the sun's surface as we know sun-spots to be?' It ought, however, we think, to be borne in mind that in sun-spots we have, *as a matter of fact*, a set of phenomena curiously restricted to certain solar latitudes, within which, however, they vary according to some complicated periodical law, and presenting also periodical variations in their frequency of a strangely complicated nature. Now these phenomena must either be caused by something within the sun's surface, or by something without it. But if we cannot easily imagine bodies so distant as the planets to produce such large effects, we have equal difficulty in imagining anything beneath the sun's surface that could give rise to phenomena of such a complicated periodicity. Nevertheless, as we have remarked, sun-spots do exist, and obey complicated laws, whether they be caused by something within or something without the sun. Under these circumstances, it does not appear to us unphilosophical to see whether as a matter of fact the behaviour of sun-spots has any reference to planetary positions. There likewise appears to be this advantage in establishing a connection of any kind between the behaviour of sun-spots and the

[1] Continued from p. 11.

positions of some one prominent planet, that we at once expect a similar result in the case of another planet of nearly equal prominence, and are thus led to use our idea as a working hypothesis."

Proceeding upon this principle, these observers measured every sun-spot recorded by Mr. Carrington from the beginning of 1854 to the end of 1860, as well as every one photographed at the Kew Observatory from the beginning of 1862 to the beginning of 1867, and the results of all these measurements are recorded in Diagram C.

In this diagram each curved line is supposed to represent the behaviour, as regards size, of the various groups of spots as they pass across the disc of the sun by solar rotation from left to right. If, for instance, a spot were always to retain the same magnitude, its path would be represented by a horizontal line, but if it were to become smaller at the middle of its course than at either extremity, then we should have it represented as in the first figure. Now, from this diagram, we find that whenever either Venus or Mercury is between or nearly between our earth and the centre of the sun, the sun-spots behave as in the first figure; that is to say, as they are carried round by rotation nearer to the planet, they become less, and as they are carried away from the planet they become greater. Secondly, when Venus or Mercury is at the extreme right of the sun the spots diminish in size all the way across. Thirdly, when Venus or Mercury is on the other side of the sun, exactly opposite the earth, the spots have their maximum in the centre; and, finally, if Venus or Mercury be at the extreme left, the spots augment in size all the way across; in fine, they are always least in the immediate neighbourhood of Venus or Mercury, and greatest when that portion of the sun to which they are attached is carried by rotation to the position farthest from the influential planet.

If there be any truth in this evidence it would seem to follow as a corollary that when two influential planets are together on one side of the sun, their peculiar spot-producing action should be conspicuously great, and hence there should be a greater than usual amount of spots when such conjunctions take place.

On the other hand, when one influential planet is on one side of the sun and another on the other side, they might be supposed to counteract each other, and hence the spotted area would be conspicuously small. In a memoir which will be found in the *Transactions* of the Royal Society for 1870 the Kew observers have

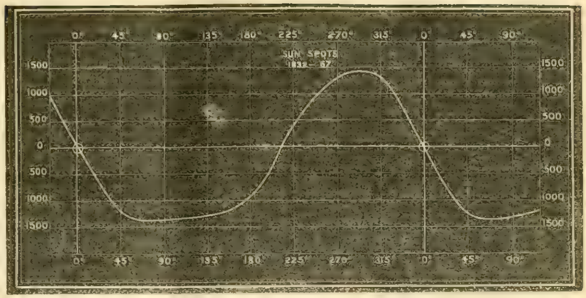

Diagram F.

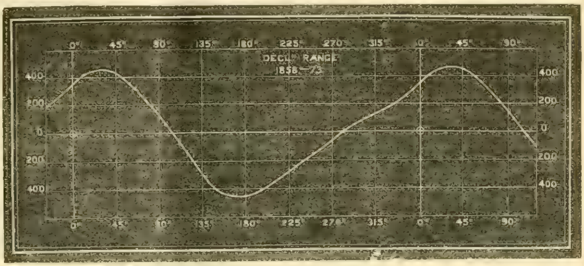

Diagram G

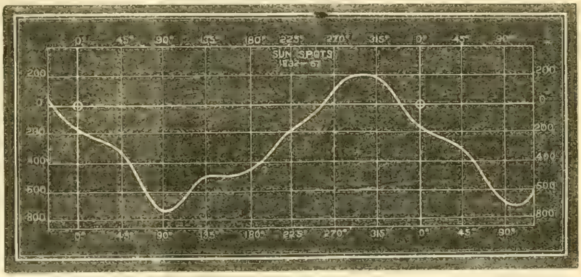

Diagram H.

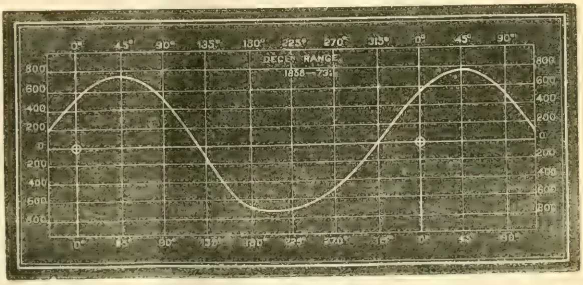

Diagram I.

investigated this point also, and they appear to have found sun-spot inequalities depending on the relative positions of the various influential planets.

For instance, there is a greater than usual amount of sun-spots when Venus and Jupiter are together; there is the same a little before the time when either Venus and Mercury or Mercury and Jupiter are together, and finally, there is the same a little before the time when Mercury is nearest the sun.

These results of strictly solar observation are capable of being verified in quite a different manner. If the planets have an influence on the sun and if the state of the sun's surface affects terrestrial magnetism, it might be expected that we should have magnetic inequalities depending upon the positions of the planets.

By this it is not meant that the planets influence the magnetism of the earth directly, but rather through their effect upon the solar surface.

Again, it was shown in the last article that terrestrial magnetic effects at Kew lag behind corresponding states of the solar surface. This lagging behind ought therefore to be exhibited in any comparison which we make between sun-spot inequalities depending on the planets and magnetic inequalities at Kew depending on the same cause *if the latter inequalities are caused indirectly through the medium of the sun.*

A comparison of this kind has recently been made by the writer, using for this purpose those inequalities of short period that were most likely to be exhibited in the limited series of magnetic observations at his disposal for the purpose.

The results are embodied in the preceding diagrams. Diagram D represents the sun-spot, and diagram E the magnetic inequality due to the relative positions of Mercury and Venus (0° denoting conjunction). Diagram F represents the sun-spot, and diagram G the magnetic inequality due to the varying distance of Mercury from the sun (0° denoting perihelion). Diagram H represents the sun-spot, and diagram I the magnetic inequality due to the relative positions of Mercury and Jupiter (0° denoting conjunction). From all these it will be seen that there is a striking likeness in character between the planetary sun-spot inequalities, and the planetary magnetic inequalities derived from the records of the Kew Observatory—the latter, however, lagging behind the former in point of time, as might have been expected.

It is unquestionably a very strange and striking conclusion that the daily range of the magnet freely suspended in a vault of the Kew Observatory, should be sensibly greater about the times when Venus and Mercury, or Venus and Jupiter come together in position, and also about the times when Mercury is nearest the sun.

Perhaps it is not too much to say that the facts described in the last article go to show that the sun influences the earth, and possibly also the other planets in some unaccountable manner, while the facts of this article go to show that (shall we say in return) the most conspicuous planets of the system, and possibly also the earth, are not without an influence upon the state of the solar surface. I may be permitted, in conclusion, to transcribe a paragraph from a former essay on this subject (Owens College essays). "At first sight we are startled by the supposition that a planet like Venus, which comes nearer to the earth than it ever does to the sun, should in any way be accountable for such enormous manifestations of energy as those which occur over the sun's surface. But the wonder will disappear if we bear in mind that there may be two kinds of causes or antecedents. Thus we may say that the blacksmith is the cause of the blow with which his hammer strikes the anvil, and here the strength of the blow depends upon the strength of the smith. But we may likewise say that the man who pulls the trigger of a gun or cannon is the cause of the motion of the ball, and here there is no relation between the strength of the effect and that of its cause.

"Now, in whatever mysterious way Venus and Mercury affect the sun, we may be sure it is not after the fashion of the blacksmith—they do not deal him a violent blow producing all this enormous effect, but they rather pull the trigger, and immediately a very great change takes place."

BALFOUR STEWART

(*To be continued.*)

THE NEW ZOOLOGICAL GARDENS AT CALCUTTA

THE propriety of establishing Zoological Gardens at Calcutta, has, as those who are acquainted with the proceedings of the Asiatic Society of Bengal are well aware, been before the public and the Indian Government for these last fifty years. It is, however, only within a very recent period that anything has been practically effected, and the first report on the progress made in the development of the new institution during the first year of its existence has only just reached us. Before alluding to its contents, a few words on the origin of the present scheme may be acceptable to those who take an interest in the subject.

Many previous plans for the institution of Zoological Gardens in Calcutta, including that proposed by Sir Joseph Fayrer in 1867, having come to nothing, Mr. L. Schwendler, of the Indian Telegraph Department, brought the subject again to the notice of the Council of the Asiatic Society in March, 1873. Mr. Schwendler proposed that the necessary capital should be raised by subscription, but that the Government of Bengal should grant the site and give a contribution towards the annual expenditure. This scheme, although taken up with interest by the Asiatic and Agricultural Societies and supported by the press, would have ended, like its predecessors, in failure, had not the energy of Mr. Schwendler led him to adopt a different course of action. Having a fine private collection of living animals of his own, Mr. Schwendler was able to prove to the Viceroy of India (then Lord Northbrook), who honoured him with a visit, how easy it was to maintain such an establishment in a climate so well adapted to animal and vegetable life as that of Calcutta. Instead of the large and expensive houses necessary in these inclement climes simple sheds suffice as a protection for the animals against the weather, and the luxuriant vegetation is ever springing up to contribute to their shelter and retirement. In fact, if only space is provided, and sufficient fencing is put up, animals can be kept almost in the same state as in their native wilds, and buildings may be dispensed with. So practical was Mr. Schwendler's illustration of how easily zoological gardens might be established in Calcutta by showing his own grounds fitted up for the purpose, that the Viceroy was convinced at once, and quickly brought the excellent Lieutenant-Governor of Bengal to a similar state of mind. Having taken up the matter, Sir Richard Temple set to work at it with his usual energy, and by a minute of September 24, 1875, granted a large site for the purpose on the road leading from Surat Bridge to the Governor's official residence at Belvedere. Shortly afterwards an honorary managing committee was appointed, with Lord Ullick Browne as president; Mr. Schwendler and Dr. King and Mr. Watson as members, and Mr. C. Buckland, private secretary to the Lieutenant-Governor, as honorary secretary. The objects of the new institution, besides the general instruction and recreation of the community, were specified to be to facilitate scientific observations on the habits of animals, to encourage their acclimatisation, and generally to promote the science of zoology. Upon the starting of the new institution, Mr. Schwendler immediately hastened to present to it his whole collection of living animals, and the Governor-General promised to remove the Barrackpore menagerie to the new site as soon as the necessary preparations were made. All the native princes, nobility, and gentry subscribed liberally towards the proposed gardens—for instance, the Maharajah of Burdwan gave 3,000*l.*; others followed this liberal example, and the greater part of the required capital of 30,000*l.* was quickly raised. Such rapid progress was made that occasion was taken of the Prince of Wales's presence in Calcutta on December 27, 1875, to inaugurate the new institution. His Royal Highness expressed his gratification at the results already achieved, praised Mr. Schwendler for the public spirit he had displayed, and accepted the patronship of the gardens.

The Royal Zoological Gardens of Calcutta thus inaugurated were opened to the public for the first time on May 6 of last year. From that date up to the close of the year more than 50,000 persons had visited them, without including members and donors, and soldiers with their wives and children who have a free entrance. The buildings are, of course, yet far from complete, but amongst those finished are, as we are informed, many deer-paddocks, which are already well tenanted; a large and several smaller aviaries, also well filled; a large bear-house in three compartments, and furnished with a large bath; two monkey-houses, and a very large pit fitted up for the residence of rhinoceroses. Within the gardens is also a large tank or lake, with two islands used for water-fowl, and a restaurant and keepers' dwellings have likewise been erected.

At the present time Mr. C. T. Buckland, C.S., is the president of the Association for the maintenance of the gardens, and Dr. J. Anderson and Mr. H. M. Tobin have the general superintendence and honorary care of them, the paid officials consisting of natives only. A European director was appointed in January, 1876, but the Government of Bengal were shortly afterwards stopped by the Supreme Government from contributing to the expenses of the gardens, and his services had consequently to be dispensed with.

This change of front in the Supreme Government, who had virtually pledged themselves to assist in the scheme, and who have not yet redeemed their promise to transfer the Barrackpore menagerie to the new gardens, is a subject of not unnatural complaint on the part of the committee, who are now striving hard to have matters replaced upon their former footing. As the Indian Government keep up botanical gardens in Calcutta, and pay a scientific officer a liberal salary to superintend them, they would surely be fully justified in treating the Zoological Gardens in the same way, especially until the new institution is fairly set a-going. Living animals, as we all know, are far more attractive to the general public than living plants, and there can be no question, we believe, that in Calcutta, as in London, zoological gardens are more popular than botanical. The public of Calcutta have come bravely down with a sum of 30,000*l.* to set the institution going, and will be greatly disappointed if the Government do not support them. A scientific director for the establishment is an absolute necessity, as it cannot be expected that Dr. Anderson and others who now manage it can continue their gratuitous services. Lord Northbrook is now earning his well-merited repose in this country, but looking to the countenance and favour that he has already shown to the Zoological Gardens at Calcutta, we cannot doubt that he will assist in the appeal that is, we understand, now being made to the authorities at home, to obtain permission from the Government of Bengal to continue the support which it gave at first. We may also fairly call upon Lord Salisbury, who has on many occasions shown his appreciation of scientific work, to devote a few minutes' attention to this subject. It is certain that no better step could be taken for the advancement of Zoological Science in India than the establishment of the Zoological Gardens of Calcutta on a firm footing. Like our Gardens in London they might easily be made a centre whence encouragement is diffused to zoological investigations of every kind. A well-selected director, appointed and paid by the Government, would at once place matters on a satisfactory foundation, and tend to bring together support to the Institution from every quarter, and we cannot doubt that the present obstruction will be removed by the Central Authority as soon as the real facts of the case are brought before them.

OUR ASTRONOMICAL COLUMN

DOUBLE-STAR MEASURES AT CINCINNATI.—In Nos. 2 and 3 of the publications of the Cincinnati Observatory are two series of micrometrical measures of double-stars made with the 11-inch Merz refractor. The first series includes measures by Prof. O. M. Mitchell at the old Observatory, confined, with few exceptions, to the stars of the great Dorpat Catalogue, and made in the years 1846–48, a small number of which only had appeared in the *Sidereal Messenger*.—The second series comprises measures of objects situated for the most part beyond Struve's limit of south declination made in the years 1875–76, and will probably be found the most useful of the two, observations of these southern stars being as yet in small number. Mr. Ormond Stone, the present director at Cincinnati, remarks that "no systematic survey of the southern heavens similar to that made by Struve of the northern heavens has ever been undertaken," and a large proportion of Sir John Herschel's doubles have never been properly measured micrometrically. The Cincinnati object-glass having been refigured by Alvan Clark during the last winter, the director purposes devoting the instrument to supplementing the labours of other astronomers by measuring double-stars between 15° and 35° of south declination; no doubt in the course of this work new binary systems will be detected.

Amongst the stars in the second of the above series, is *h* 2036, the duplicity of which was first remarked by Sir John Herschel with the 20-feet reflector in sweep 307 (1830, Oct. 15), when the position was registered 53°·0, and the estimated distance was 2″. The last Cincinnati measures give for 1876·78, position 26°·4, distance 1″·64, and Capt. Jacob's intervening measures at Poona and Madras, confirm the retrograde motion in the angle; indeed, he first pointed out the probable binary character of the star, and also suggested another noticeable feature, viz., the apparent variability of *both* components (Mem. R.A.S., vol. xxviii. p. 41). A comparison of the whole of the estimates of magnitude to 1876, is certainly confirmatory of Capt. Jacob's suspicion. The stars have not been noted as differing more than half a magnitude, and generally have been considered of equal brightness, yet the estimations vary from 6·7 (Jacob 1857) to 9·0 (Ormond Stone, 1875). The object is well within reach in this country, and deserves watching. It may be remarked that the N.P.D. given in Sir John Herschel's fifth series of measures with the 20-feet reflector, is 1° too small. By an observation in the Washington zones, the position for the beginning of the present year is in R.A. 1h. 13m. 54·7s., N.P.D. 106° 26′ 15″.

CHANGE OF COLOUR IN α URSÆ MAJORIS.—Herr Weber continues his observations upon the colour of this star, a periodical change in which from intense fiery-red to yellow was first suspected by Dr. Klein of Cologne about fifteen years since. According to Herr Weber the change is from fiery-red to white or slightly yellowish white. The following are recent observed epochs of red light:—1876: September 5, October 10, November 14, December 21. 1877: January 16, March 23, whence an average period of thirty-three days is indicated. The star was white or nearly white, 1876: October 28, December 30. 1877: February 8 and March 13. The star is said to remain red or reddish for a shorter time than it is seen white or yellowish white. See *Astron. Nach.*, Nos. 2,111 and 2,127.

α CENTAURI.—The measures of this star which have lately appeared in NATURE show that we are yet without any satisfactory orbit, and it is much to be desired that it may be closely watched during the next few years. Mr. Gill it is understood intends to establish a good epoch in the autumn with Lord Lindsay's heliometer at Ascension. It does not appear to be too late to secure measures which will possess the greatest interest in the actual near approach of the two fine stars forming this splendid binary.

THE PRESENT COMETS.—Elements of Winnecke's comet of April 5 calculated by Dr. Plath of Hamburg, upon the same extent of observation as those of Mr. Hind, published in this column last week, are almost identical therewith, and consequently negative the idea of ellipticity of orbit, notwithstanding the certain degree of resemblance with the comets 1827 II. and 1852 II., and near equality of intervals. On May 14 the comet approaches within 10° of the pole of the equator, near the 5½ m. star, B.A.C. 1211. It will be visible with telescopic aid some weeks longer.

We subjoin positions of the comet discovered in America by Mr. Lewis Swift on April 11, and by M. Borrelly at Marseilles three nights subsequently, also calculated by Dr. Plath.

For 12h. Berlin M.T.

	R.A.			Decl.		Log. distance.
	h.	m.	s.	°	′	
May 10	6	8	18	+56	7·0	0·1243
,, 12	6	27	1	54	22·5	0·1275
,, 14	6	43	53	52	30·2	0·1315
,, 16	6	59	5	50	32·1	0·1362
,, 18	7	12	46	48	30·3	0·1417
,, 20	7	25	2	46	25·9	0·1479
,, 22	7	36	11	44	21·8	0·1545
,, 24	7	46	15	+42	17·7	0·1617

M. Wolff, of the Observatory at Paris, observed the spectrum of Winnecke's comet on the morning of April 11, which he found analogous to the spectra of various comets he had observed since the year 1868, from the faintest to the brilliant comet of Coggia in the summer of 1874. All have exhibited the three bands, yellow, green, and blue, but M. Wolff remarks that the nature of this cometary matter is completely unknown. He did not succeed in obtaining the spectrum of the third comet of the present year, in which, like several other observers, he noticed a resolvable appearance.

BIOLOGICAL NOTES

ZOOLOGICAL CLASSIFICATION.—In a recent paper in *Pflüger's Archiv*, M. Hoppe-Seyler wonders at the readiness with which systematic zoology has ranked amphioxus with the vertebrates, from mere one-sided consideration of the presence of a *chorda dorsalis* , and the position of the nerve-cord above, and the alimentary canal below. A sound system groups species which are similar not merely in morphological respects, but in their whole organisation. Amphioxus has, beyond the chorda, nothing in common with vertebrates ; it has no closed vascular system with red blood corpuscles, no liver which forms a gall, no proper brain, and it contains no gelatine-yielding tissue, which occurs in all vertebrates and also in the cephalopoda, but in no other invertebrata. In their entire highly-developed organism, the cephalopoda, stand nearest to the vertebrata ; the amphioxus should have a place further down. M. Hoppe-Seyler further points out that comparing the composition of tissues from the lower organised animals upwards, we meet first with mucin yielding tissues, then with those yielding chondrin, then, in the cephalopoda tissues yielding glutin ; the formation of actual bones does not occur in all vertebrata, and is likewise wanting in cephalopoda. Exactly the same order is seen in the stages of development of an embryo, *e.g.* of the hen in the egg, and it is difficult to think that the agreement is accidental.

LUMINOUS CAMPANULARIÆ.—The late Prof. Paolo Panceri recently made minute researches at Amalfi, near Naples, with a view to determine the exact seat of the light-giving organs in Campanulariadæ. The Gulf of Amalfi seems to be a favourite haunt of these minute animals, and Prof. Panceri found them abundantly on the algæ covering the rocks near the shore, particularly upon *Fucus ericoides*. The light of these polyps is intermittent, and only appears when the animals are touched or moved ; fresh water, however, has the property of fixing it for a little time. It was principally with species of *Campanularia flexuosa* that Prof. Panceri made his interesting investigations, and the special question he wished to decide was whether it is the external cellular stratum (or ectoderm), or the internal one (endoderm) of which these animals are composed, which is the actual seat of the light. He found, by means of ingenious microscopical contrivances that the luminous movements of these polyps have their seat in the cells of the ectoderm, and not elsewhere, and that these cells alternately and successively show the light and again become dark, after being touched or placed into fresh water. Not only the bodies of the polyps, but also their slender stems and even the feet with which they adhere to the plants or rocks, contain these luminous cells. Prof. Panceri has published an account of his researches in the January part of the *Rivista Scientifico-industriale*.

RESPIRATION IN FROGS.—Mr. A. C. Horner has sent us an account of some interesting observations he has made on the spawning or deposition of ova in the frog (*Rana temporaria*). We are only able to give the conclusion of his paper :—I will now give a few facts connected with respiration which I have observed in these frogs. They can croak when they are immersed under water, but, as no air-bubbles escape, I was at first puzzled. I find, however, by holding my nose and shutting my mouth, that I can make a somewhat similar sound ; but they seemed to croak louder when only the head and upper part of the body were under water than when their whole body was immersed, and as they distend their sides in the act of croaking, I thought it possible they might be able to draw in air by the rectum or the pores of their skin. When a frog out of water is touched suddenly, he shuts his eyes and distends his abdomen, and the same thing occurs when under water. Yet how is it that they can distend their abdomen without admitting more air ? for they can distend it very fully, and I should think must require to expel all the air from the thoracic into the abdominal cavity. When a frog is under water, his sides sometimes pulsate rhythmically, just as when he is out of water, and about every ten seconds. Perhaps it is connected with the circulation of blood.

THE WOODPECKER.—In the April session of the German Ornithological Society Prof. Alton concluded the recital of his investigations on the habits of the woodpecker. The peculiar drumming sound often caused by it was shown on various grounds to be entirely disconnected with the search for insects as hitherto supposed, and was regarded as a call to the opposite sex. Dr. Brehm defended the woodpeckers against the charge of seriously injuring the trees, and considered the slight damages resulting from them as more than compensated by the colour and animation which they gave to the otherwise sober and quiet forests.

THE FLAMINGO.—At the same session Herr Gadow stated that by a study of the digestive organs of the flamingo he had found that it did not belong to the duck family as hitherto classified, but was to be placed among the storks, being very closely allied to the latter, although properly an intermediate link between the two families.

COPPER IN THE BLOOD.—The presence of copper in the blood of human beings and domestic animals has been placed beyond doubt by the investigations of various chemists, but has generally been regarded as an accidental circumstance due to the use of copper utensils in the preparation of food. M. S. Cloez, of Paris, recently examined the blood of a roebuck shot in the forest of Essarts, and found copper oxide present to the extent of 5½ milligrammes per kilogramme of blood. As this result would tend to show that copper is a normal constituent of the blood, the question which next awaits solution is that of the method of its entrance into the animal system.

THE RESPIRATION OF PLANTS.—We have already noticed the investigations undertaken by Prof. Borodin on the processes of respiration in plants. We find in the seventh volume of the *Memoirs* of the St. Petersburg Society of Naturalists the paper of Prof. Borodin in full, accompanied by a series of graphic representations, by means of curves, of his important measurements. We cannot attempt here to give a *résumé* either of the varied experiments made by the author or of the important questions arising from Prof. Borodin's inquiry, and discussed by him. As to the experiments themselves, we can only state that the reader will find in the paper a thorough discussion of their value and of the value of various methods used for the study of the subject. The main result is that in darkness the energy of respiration of a branch gradually decreases; a temporary action of light, however, increases it, this increase being mostly the result of the influence of the less refrangible rays (red, &c.), and it takes place only when the surrounding air can supply the plant with a sufficient amount of carbonic acid. The decrease of energy of respiration is caused by the decrease of the stock of starch in the plant, and the increase under the influence of light takes place because of the formation, under this influence, of a new stock of starch. Thus, other conditions remaining the same, the energy of respiration depends upon the existence in the branch of non-nitrogenous plastic substance; this is the material for respiration, the exhaled carbonic acid being the result of oxidation of a certain part of non-nitrogenous organic matter. While following the author in his discussion of this subject and of the opinions of Garreau, Pflüger, and Sachs, we further notice the importance of a fresh supply of oxygen in the atmosphere surrounding the plant as resulting from M. Borodin's experiments and the contributions they make to the most important and yet very obscure question as to the influence of temperature upon respiration. These important questions will be the subject of further studies, which the author proposes to continue for many years.

A TASMANIAN CARNIVOROUS PLANT.—Dr. B. Crowther, of Campbell Town, Tasmania, writing to *The Mercury* (Hobart Town), November 26, 1876, states that he was furnished with a plant which grows on rocky ground, whose crevices contain rich organic soil, different from the peaty soil Darwin's grew in. It is quite obvious, he states, on careful examination, that the plant lives to a great extent off the small flies and gnats it obtains. It is about six inches in height, and from its single vertical stem project from one to two dozen small foot-stalks, at irregular and variable distances. On the summit of each foot-stalk is a rounded disc, placed horizontally, about half an inch in circumference, fringed with tentacles of different sizes. In the centre is a hollow, with small fine filaments projecting vertically; on the ends of both the filaments and also tentacles are little reddish glands which secrete a sticky substance. The fly rests on the outer zone, is conveyed by the sticky tentacles to the centre, which at once closes upon the victim so tightly that a bulging may be seen corresponding to the fly inside. After it has been consumed, the trap again opens, showing the *débris* of the fly, which are doubtless washed away by the rain, so as to allow the trap to again set for another victim. The plant described by Dr. Crowther is evidently *Drosera peltata*, Smith, a well-known Australian species (in herbaria). It is not referred to by Mr. Darwin in his work on "Insectivorous Plants;" and any more exact information respecting its habits of life, and the mode in which it captures insects would be a very useful addition to our knowledge of these plants, especially if accompanied by drawings.

BOTANY OF NEW GUINEA.—The distinguished Italian naturalist and traveller, Dr. Beccari, has commenced the publication of a new illustrated work called "Malesia," for the purpose of bringing before the scientific world his numerous botanical discoveries in New Guinea and the Eastern Archipelago. The first number of "Malesia" has just been issued at Genoa, and is occupied with an article upon the palms of New Guinea and the adjacent islands. Fifty species of palms were collected by Dr. Beccari in these countries, many of which were previously unknown.

NOTES

MR. C. J. LAMBERT has presented to the Chemical Society 1,000*l.* and to the Royal Microscopical Society, 500*l.*, from a bequest of 25,000*l.* left by his late father, to be appropriated to benevolent and scientific purposes.

WE regret to learn that Prof. M'Crady who, on the death of Prof. Agassiz, succeeded to the Chair of Zoology in Harvard College, has found it necessary to tender his resignation to the authorities of the University. This step is all the more to be lamented as judging from the terms of the resignation, which we have read, it has been caused by a desire on the part of Prof. M'Crady to raise the standard of zoological education in the college to a higher level than was deemed advisable by the authorities. We hope that some means may be found of retaining Prof. M'Crady's services to the University. He is well known as an eminent original worker in an important department of zoological research.

WE regret to announce the death of a Russian geologist, Prof. N. P. Barbot-de-Marny. Having begun his scientific work in 1852, taking part in Hoffmann's exploration of the Ural, M. de Marny continued until 1876 his valuable work of the geological exploration of Russia. He explored the Kuma-Manych depression, the provinces of Archangel, Vologda, Volhynia, Podolia, and Kherson, and all the lines of railway radiating from Moscow, as well as those of Kief, Azov, Tsaritsin, Orenburg, and Caucasus. In 1874 he took an active part in the difficult exploration of the Aral-Caspian expedition and explored the Amu-Darya. The *Mines Journal* and the *Memoirs* of the Mineralogical and Geographical Societies, as well as those of the St. Petersburg Society of Naturalists, one of the presidents of of which he was for a long time, contain about 110 of his valuable papers, besides which he was the author of some important volumes. His "Formation Stage" was an important addition to our knowledge of the Tertiary of South-eastern Europe. He died at the age of forty-five, leaving a family, a library of books, many MSS., and—no money.

THE President of the Royal Academy is always very catholic in his invitations to the annual dinner, certainly one of the chief events of the London year. On Saturday last science was largely and well represented, and Dr. Hooker, in his reply to the toast of Science, happily performed what at first sight would seem a hard task under the circumstances. Dr. Hooker showed that the incongruity between art and science was only apparent; that art lends valuable aid to science, and that all true art must really be based on scientific principles; and that moreover the two have this in common, that success is unattainable in neither unless by close observation, enthusiasm, and the skilful exercise of the imagination. Some may be inclined to think that the new Grosvenor Gallery is more scientific in its method of selection than the much maligned Academy, but then the objects of the two are very different. At all events the man of science will be furnished with much food for thought and wonder in both. What seems to be generally regarded as the masterpiece in the Grosvenor, Mr. Burne Jones's "Six Days of Creation," may remind geologists of Hugh Miller's famous phantasy, written in the old "reconciliation" days. However this may be, its conception and execution are deserving of study from many points of view, including even the scientific.

The great Museum of Applied Sciences in Moscow will be opened on June 11, the birthday of Peter the Great. The building is ready and the collections have been brought in. It has cost up to the present time half a million of roubles, occupies a space of 13,633 square yards, and is divided into three blocks. Besides spacious rooms for collections in applied science, it contains a large and well-ventilated auditory. Eleven scientific societies will hold their sittings in the Museum.

The foundation of a permanent station for help to wrecked vessels on Novaya Zemlya is now in way of execution. We hope that the station will also be used for taking regular meteorological observations. An Eskimo family, which has already wintered for two years on the island, will remain there permanently, and be supplied by the Russian Government with all necessaries.

Prof. Leith Adams has commenced a course of six lectures on the "Distribution of Animals as elucidating Past Changes of the Earth's Surface," in the Royal College of Science, Dublin.

A prize of 10*l.*, which has been placed at the disposition of the Council by Col. A. A. Croll, is offered by the Society ot Arts, with the Society's Silver Medal, for the best set of Blowpipe apparatus which shall be sold retail for one guinea. All apparatus for competition must be sent to the Society's house on or before August 1, 1877. Details will be found in the *Journal* of May 4.

The Prince of Wales, in company with Mr. Cunliffe Owen, Col. Ellis, Lord Suffield, and M. Blowitz, visited the works of the Paris International Exhibition at the Champ de Mars and Trocadero, last Saturday. He was received by M. Krantz, Director of the Exhibition, the Minister of Trade, and some officials. The Prince of Wales was much pleased with the state of the works, which are progressing so rapidly that it is now possible to have a view of the buildings covering so large a space. He selected a space for the special exhibition of objects which he brought back with him from his tour in India.

In the current number of *Mind*, Mr. G. H. Lewes gives briefly what seems to be one of the chief positions taken by him in his new volume "The Physical Basis of Mind." He finds that according to usage the word "consciousness" is equivalent to sentience or feeling; that it is also used in a special sense as signifying that we not only feel, but feel or are conscious that we feel. Now Mr. Lewes holds that every neural process implies sensibility, indeed *is* feeling or consciousness in the general sense of that term; accordingly consciousness, sentience—these neural processes may be said to have "various modes and degrees—such as perception, ideation, emotion, volition, which may be conscious, sub-conscious, or unconscious." In the last sentence the word "unconscious" describes a mode or degree of sentience which has not given rise to consciousness in the special sense, and Mr. Lewes contends that the word "unconscious" ought to be confined to this usage, that in strictness we should not speak of unconsciousness outside the sphere of sentience. He then proceeds to argue that to describe a neural process as a mere series of physical changes is to say that "organic processes suddenly cease to be organic and become purely physical by a slight change in their *relative* position in the consensus." The matter of fact of which Mr. Lewes has to persuade his readers is, that "the reflex mechanism necessarily involves sensibility," that a neural process is a feeling.

Six years ago Dr. Maudsley contended against the popular opinion that insanity was on the increase in this country, the rapid increase of the registered insane being open to a less gloomy explanation. It is gratifying to find that Dr. Maudsley can in the current number of the *Journal of Mental Science* still maintain with every appearance of truth, that there is no evidence of an increased production of insanity in this country.

At a recent meeting of the Chemical Section of the Society of Arts, Dr. B. H. Paul read a paper on "The Cinchona Alkaloids, their Sources, Production, and Use," in which he traced the history of the cinchonas from the early part of the seventeenth century to their successful cultivation in India and other countries. The chemistry of the cinchona barks is a point about which but little is popularly known. It would seem that a considerable amount of cinchonidine, one of the several alkaloids found in cinchona barks, is often mixed with the sulphate of quinine of commerce, sometimes, indeed, exceeding ten per cent., and though the medicinal efficacy of the quinine is not materially impaired by this mixing, a great difference is made in the intrinsic value, cinchonidine being worth not more than one-eighth as much as quinine. Considering the present high price of quinine, it is pleasant to be told by Dr. Paul that "the sulphate of cinchonidine has been proved to be very little inferior in efficacy—for certain kinds of maladies—to quinine," the price of this alkaloid being two or three shillings an ounce against sixteen shillings for quinine.

M. Jabloskow, a Russian electrician, has exhibited before the Physical Society of Paris a new process for producing electric light. The voltaic arc is quite suppressed and a current is sent merely through a plate of caolin, which ignites and fuses gradually, giving out a magnificent steady light. The transverse dimension which the current is able to warm and ignite varies according to the force of the battery. M. Jabloskow made a most interesting experiment. Cutting in two parts a plate of caolin which had been used for giving a light, he raised two separate lights with the same current. The light given by these two plates was found equal to the light which had been given a few minutes previously. The experiment was considered by all present to be a great success. Experiments on a large scale will be shortly tried at the large hall of the Magazin du Louvre. The generator of electricty was an induction machine of the Alliance type worked by two men.

At the last meeting of the Russian Geographical Society M. Wojeikof reported upon his last journey in Japan. He started from Hakodadi and visited the Aïnos of Jesso Island; he then went to Aomori, in the northern part of Nipon, and travelled to Jeddo, crossing Nipon Island three times from west to east. The northern part of the island is not populous, only the high valleys being settled. The climate of the western shores of Japan is far milder than is generally supposed, the tea-tree reaching here as far as 40° north latitude. The most important result of the journey is the measurement by barometer of the heights of about 600 places.

Russian newspapers announce that our countryman, Mr. Harvey, after having stayed for three days in St. Petersburg, continued his journey for the zoological exploration of the Pechora region. He is accompanied by a painter and a zoological collector.

The last number of the *Isvestia* of the Russian Geographical Society announces that the south-western branch of the Society, established at Kief, is closed by Imperial Order for political reasons.

THE same periodical gives some information as to the journey made last summer by Capt. Pevtsov with a Cossack detachment which protected the caravan with corn, sent by Russian tradesmen from the Lake Zaisan, *viâ* Bulun-Tokhoi, to the Chinese town Gu-chen (Dzungaria, N. lat. 43° 50′, E. long. 90° 14′). The results of this journey are,—a survey of the route, 560 miles long, with maps of the towns, astronomical determinations of the positions of seven points, magnetical observations, barometrical measurements of heights, a complete geological exploration along the route, a collection of about 1,000 species of plants, and a zoological collection numbering 34 mammalia and 123 birds.

THE Geological Survey of Finland, which was undertaken on the scheme of that of Sweden, but was interrupted in 1868, will be continued this year.

A TELEGRAM received by the St. Petersburg Academy of Sciences, announces that the mammoth found in the neighbourhood of Tomsk is very well preserved. A piece of its flesh with fat has been forwarded to the Academy, which, as we learn from a private source, proposes to send M. Poliakof for the exploration of the remains.

THE Russian Geographical Society has undertaken the publication of an historical sketch of geographical explorations in Northern Asia, with accounts of all expeditions, an index of works on Northern Asia, and a map showing the routes followed by all important exploring parties. The work will appear in 1879, that year being the tercentenary of the crossing of the Ural Mountains by Yermak, the conqueror of Siberia.

IN a recent communication to the Belgian Academy, M. van Monckhoven describes some improvements in the photographic reproduction of ultraviolet spectra of gases. He employs two large Geissler tubes placed parallel and communicating together by a capillary tube at right angles to them. The spectroscope consists of three 60° prisms of Iceland spar, cut so that the bisector plane of each of their dihedral angles is parallel to the optic axis of the crystal. With such prisms the ordinary and extraordinary spectra do not encroach on one another. The axis of the capillary tube is then made to coincide exactly with that of the collimator of the spectroscope, and the intensity of the light, which can be utilised during passage of the current from a Ruhmkorff coil, is found to be very much greater than if the tube were placed, as usual, perpendicularly to the axis of the apparatus. The author recommends using a plate of quartz in place of one of the large tubes of glass, so as to prevent too great absorption of rays of high refrangibility. To give an idea of the exactness with which even the most refrangible bright lines are reproduced, M. van Monckhoven presented three plates representing the solar spectrum, the bright lines of hydrogen combined with those of aluminium (of which the electrodes were formed), and the bright lines of a solar protuberance.

WE have received from Prof. E. S. Holden, of the United States Naval Observatory, a list compiled by him of the principal telescopes in the possession of public institutions and private individuals. The list, though imperfect, is a long one, and we regret that the pressure on our space prevents us from printing it. Those who would like to possess it will find it in the *Popular Science Monthly* for March. Among reflectors we notice that Lord Rosse's is still unsurpassed; it has an aperture of 6 ft. and a focal length of 55 ft. Mr. Ellery's, of Melbourne, has a 4 ft. aperture and a focal length of 32 ft.; that of the Paris Observatory an aperture of 1·20 metre and a focal length of 7 metres. Of refractors the two largest are now constructing; that for Yale College Observatory (by Clark and Sons) will have an aperture of 28 in., and the one for Vienna, constructed by Grubb, an aperture of 27 in. The refractor belonging to Mr. Newall, of Gateshead, has an aperture of 25 in. and a focal length of 29 ft.; the corresponding dimensions of the Pulkowa refractor are 14·93 in. and 270·6 in.; Lord Lindsay's, 15 in. and 15 ft.; that of Greenwich, 12·5 in. and 16·6 ft.; the largest in the Paris Observatory, 12 French in. and 5 metres; Rutherfurd's, of New York (a photographic refractor), an aperture of 10·5 French in.; Secchi, of Rome, 7·5 French in., and 14 French ft. Altogether Prof. Holden enumerates upwards of 140 telescopes that are at work on the heavens, and remarks, with some justice, that "it is a melancholy fact that the return from so many instruments is not so great as it should be, and it suggests the question as to whether future benefactors will not do better to provide astronomers to use the telescopes already constructed than observatories in which to put new ones."

To those who take a practical interest in the ventilation of houses we would recommend a pamphlet by Mr. James Curtis, C.E., entitled "Fresh Air in the House, and How to Secure It" (Ward, Lock, and Tyler). Mr. Curtis has evidently studied the important subject of ventilation carefully, and his practical suggestions will be found useful to those anxious to secure a regular supply of fresh air in their houses.

IN the note on Mr. Shrubsole's discovery (vol. xv. p. 561), the word *chalk* should be *chert*.

THE additions to the Zoological Society's Gardens during the past week include two Green-winged Doves (*Chalcophaps indica*), a Hamilton's Terrapin (*Clemmys hamiltoni*) from India, presented by Mrs. M. A. Moore; three Water Ouzels (*Cinclus aquaticus*), European, presented by Mr. G. B. Davies Cooke; an Indian Python (*Python molurus*) from India, presented by Mr. C. A. F. Bowell; six River Lampreys (*Petromyzon fluviatilis*) from British Rivers, presented by Mr. A. H. Cocks, F.Z.S.; a Virginian Deer (*Cervus virginianus*) from North America, a Rock Cavy (*Ceredon rupestris*) from South America, deposited; two Raccoon-like Dogs (*Nyctereutes protyonides*), four Common Foxes (*Canis vulpes*) born in the Gardens.

UNIVERSITY INTELLIGENCE

OXFORD.—An examination will be held at St. John's College on Tuesday, June 12, and the two following days, to elect to two Foundation Scholarships for Classics, and to the Holmes Scholarship, which will be given for Natural Science. The subjects of examination in Natural Science will be Chemistry and Physics; there will be also a pass paper in Classics; there is no restriction of age. The scholarship is tenable for five years, and is of the value of 100*l.* per annum.

The Boden Professor of Sanskrit (Mr. Monier Williams) proposes to give two public lectures (open to all members of the university and their friends) in the large lecture room of the museum, on Wednesday, May 23, and Wednesday, May 30, at three P.M. The subject will be "The Sacred Places, Religious Creeds, and Superstitions of Southern India and Ceylon," and the lectures will be illustrated by diagrams and objects of interest (including a model of the Parsee Towers of Silence) brought from India.

CAMBRIDGE.—The "Rede" Lecture will be delivered in the Senate-house on Friday, May 25, at half-past two in the afternoon. The lecturer is Sir C. Wyville Thomson, and the subject of the lecture will be "On some of the Results of the Expedition of Her Majesty's ship *Challenger*."

LONDON.—At Tuesday's Convocation of the University o London a resolution was proposed thanking the Senate for their decision to admit women to degrees in medicine. To this an amendment was moved that it was undesirable to take this course before the House had considered the advisability of admitting women to degrees in all faculties. This was carried on a division by 142 to 129, and was afterwards adopted as a substantive motion by 144 to 116

EDINBURGH.—Lord Zetland has intimated that with the sum, amounting to between 4,000*l.* and 5,000*l.*, which he has received as compensation for the abolition of patronage in Orkney and Shetland, he intends to found several bursaries in connection with the Faculty of Arts in Edinburgh University. His lordship, in so disposing of the money, has in view the advancement of the educational interests of Orkney and Shetland, of either of which the intending bursars must be natives.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 12.—"On the Constant Vibration of Minute Bubbles." By Walter Noel Hartley, F.R.S.E., King's College, London.

Those who have given great attention to the study of fluid cavities in minerals, have occasionally met with vibrating particles which are apparently bubbles.

Mr. Hartley became acquainted with these at the close of last year, 1875, when Mr. P. J. Butler showed him a ruby containing a cavity partially filled with liquid carbonic acid, the bubble in which, when of small size, was in constant motion.

He also refers to a felstone containing portions of quartz with many cavities. The majority of these were water cavities, but others appeared to be empty; and in one of them Mr. Young had noticed a moving particle, supposed to be a bubble, which made its appearance only in a cold atmosphere. By dropping a little ether on the object, the evaporation cooled it sufficiently to condense a liquid in the cavity, and the moving particle was easily seen with a magnifying power of 400 diameters. By immersion in iced water, the temperature of which was 3°·5 C., the cavity had the appearance of being two-thirds filled with a liquid, the gas-bubble of course occupying the remaining space, and having a sort of trembling motion. The bubble decreased in size, and the motion became more and more rapid as the size became smaller, until it rushed up and down and across the space in which it was confined. The thought immediately occurred that this was not a gas-bubble, but a liquid in the spheroidal condition,—in all probability carbon dioxide in a perfectly dry condition, and perhaps mixed with some incondensable gas, so that its critical point was lowered.

He concludes:—I have proved that gas-bubbles in water as well as in carbonic acid, may be attracted by a source of heat giving an extremely slight rise of temperature. It is impossible to imagine a body which is not gaining or losing, or at the same time both gaining and losing heat; it is therefore impossible to imagine it entirely throughout at a uniform temperature. It is evident then that an easily movable particle which can be set in motion by exceedingly slight rises of temperature will make the transference of heat from one point to another plainly visible; I have shown that the minute bubbles in fluid-cavities are such particles; and I believe that the vibratory motions which I have described afford an ocular demonstration of the continual passage of heat through solid substances. These phenomena really make the molecular vibrations of matter plainly visible.

April 12.—"On Attraction and Repulsion of Bubbles by Heat," by Walter Noel Hartley, F.R.S.E., F.C.S., King's College, London.

The paper deals with the bubbles in fluid-cavities of crystals, and their behaviour when a source of heat is brought near them.

With regard to the attraction of bubbles by heat, the author has noticed this take place in some water-cavities when the bubbles were free to move, and no carbonic acid was present.

With regard to this second point, the repulsion of bubbles by heat, water being the only liquid. It occurs quite as frequently, if, indeed, not more so, in the specimens which the author has examined, than attraction; and it is seen to occur in cavities containing water and liquid carbonic acid.

In a paper which the author lately communicated to the Chemical Society, he has given details of experiments on certain bubbles in water-cavities, which prove that by rise of temperature the bubbles become denser than the water and sink.

Bubbles attracted by heat and those which are repelled have generally been found in separate and entirely different specimens, and it would appear most improbable that they should exist in the same piece of stone side by side.

My work, the author said, was discontinued for a long period of two months, but on being able to look over my specimens once more, I verified all my former observations, and became surprised by the following discovery:—A bubble which was repelled by a gentle heat was attracted after it had been heated more strongly, and then on cooling it was again repelled. It appeared to contain some liquid carbonic acid floating on water with the gas.

It may be considered an argument against the motions being due to any pyro-electric conditions of the minerals, that they have been noticed in crystals of fluor-spar, and that no matter in which direction sections of rock-crystal are cut, the movements are all equally well obtained.

Regarding the repulsion of gas-bubbles, two facts are striking, namely, the very slight rise of temperature (less than ½° C.) on one side of the bubble capable of causing the movement, and the great tension existing within the bubble. The gaseous contents prevent attraction by resisting the repulsion of the liquid from the wall of the cavity. Warmth at one side of the bubble results in increased tension of the gas. This being partial, causes such internal molecular disturbance before it becomes uniformly distributed, that the bubble is rolled away from the source of heat. The bubble then takes up that position consistent with the least internal pressure. In this case it is the same bubble which moves from end to end of the cavity. When repulsion is followed on rise of temperature by attraction, the *modus operandi* is the following:—Repulsion due to the circumstances above mentioned occurs until such a temperature has been reached that, in spite of the presence of gas within the bubbles, the increased vapour-tension of water becomes a motive power by reason of evaporation and condensation, the motion of course being in the reverse direction.

April 19.—"On some Figures exhibiting the Motion of Vibrating Bodies, and on a New Method for Determining the Speed of Machines," by Herbert M'Leod, F.C.S., Professor of Experimental Science, and George Sydenham Clarke, Lieut. R.E., Instructor in Geometrical Drawing in the Royal Indian Engineering College, Cooper's Hill. [See Physical Society.]

Chemical Society, May 3.—Dr. Gladstone in the chair.—The treasurer announced that 1,000*l.* had been placed to the credit of the Society by the son of the late fellow, Mr. Lambert.—The following papers were read:—On some points in gas analysis, by J. W. Thomas. The author finds that nitric oxide is absorbed by caustic potash and pyrogallic acid, and recommends that a known volume of pure oxygen should be introduced after the absorption of carbonic acid and any decrease of volume noted as nitric oxide. He states that an excess of caustic potash should always be present in the alkaline pyrogallate, but that too much of the latter should not be used.—On the decomposition of nitric oxide by pyrogallate of potash, by Dr. Russell and W. Lapraik. The authors state the probable action of the above reagent is to convert nitric oxide into half its volume of nitrous oxide, but simultaneously another more obscure reaction takes place, so that 58 to 76 per cent. of the gas is absorbed instead of 50 per cent.—Contributions to the history of the naphthalene series. No. 1. Nitroso-β-naphthol, by Dr. Stenhouse and Mr. Groves. Nitroso-β-naphthol was obtained by the action of nitrosyl sulphate on β-naphthol and purified by conversion into a barium compound, &c.; it crystallises in brilliant hydrated yellow needles or anhydrous orange brown plates or prisms. It melts at 109°·5 C. By treatment with dilute nitric acid mononitro-β-naphthol is obtained. By acting on the barium compound of nitroso-β-naphthol with hydrogen sulphide a precipitate is formed which, by the action of potassium dichromate, is converted into β-naphthaquinone melting at 96° C.; this substance is interesting as being the first instance of two isomeric quinones derived from the same hydrocarbon.—On asbestos cardboard and its uses in the laboratory, by W. N. Hartley. This substance resembles thick greyish cardboard and is formed principally of asbestos fibres; it can be cut or moulded (by moistening with water) into any shape, and is extremely useful for crucible supports, muffles, &c.

Zoological Society, May 1.—Prof. Newton, F.R.S., vice-president in the chair.—Mr. Howard Saunders exhibited and made remarks on some nests and eggs of the Orphean Warbler (*Sylvia orpheus*) from the vicinity of Malaga, Spain. Amongst the eggs in each nest were one or two of larger size, supposed to be possibly the eggs of the Cuckoo. Mr. Howard Saunders also exhibited two skins of Dupont's Lark (*Certhilauda duponti*) from the same locality.—Prof. St. George Mivart read a paper on the axial skeleton of the Pelicanidæ, selecting *Pelicanus* as his type and standard of comparison. Prof. Mivart first compared it, as regards its axial skeleton, with *Struthio*, and the other *Struthionidæ*, and then compared the other *Steganopodes* with it and with one another.—A communication was read from Dr. M. Watson, Professor of Anatomy, Owens College, Manchester, on the anatomy of *Hyæna crocuta*, in which he described the very peculiar conformation of the female generative organs of that animal.—Mr. A. G. Butler read a paper wherein he gave the description of two small collections of Heterocerous Lepidoptera from New Zealand, recently brought to England by Dr. Hector and Mr. J. D. Enys.—A communication was read from Dr. O. Finsch giving an account of a small collection of birds from the Marquesas Islands. Amongst these were three examples of a new species of Kingfisher, proposed to be called *Halcyon godeffroyi*.—A communication from Mr. Frederick Smith contained descriptions of four new species of Ichneumonidæ in the collection of the British Museum. Amongst these was a new *Bracon*, remarkable for having its ovi-positor more than nine times the length of its body. This was proposed to be called *Bracon penetratror*, and had been received from Yokohama, Japan.—Prof. A. H. Garrod read some notes on the anatomy and systematic position of the genera *Thincorus* and *Attagis*, which he considered should be referred to the Limicolæ in the neighbourhood of *Glareola* and *Cursorius*.

Geological Society, April 25.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Messrs. S. Bewsher, H. G. Bolam, Charles Thomas, and John M'Kenzie Knight were elected fellows of the Society.—On the upper limit of the essentially marine beds of the Carboniferous system, and the necessity for the establishment of a "Middle Carboniferous Group," by Prof. E. Hull, F.R.S. The author, in this paper, divided the whole of the Carboniferous rocks into successive stages from A to G inclusive, taking the Carboniferous beds of Lancashire as a type, and showed that these stages could be identified over the whole of the British Isles. It was only recently that their determination had been made in Ireland, so that until now the materials had not existed for a complete correlation of the series in the British Islands. The following is an abbreviated statement of the representative stages in descending order:

Essentially Freshwater or Estuarine, with one or two Marine Bands.

STAGE G.—*Upper Coal-measures* of Lancashire (2,000 feet) and other English coal-fields. Red Sandstones, &c., of Bothwell and Ayr, in Scotland. (Absent in Ireland.)

STAGE F.—*Middle Coal-measures* of Lancashire, &c., with principal coal-seams (3,000 feet). "Flat coal-series" of Scotland. Present in Ireland (Tyrone, Kilkenny).

Essentially Marine.

STAGE E.—"*Gannister Beds*" (Phillips), with marine shells and thin coals (2,000 feet), in Lancashire. "Pennystone series" of Coalbrook Dale, South Wales, &c. "Slaty black-band" series of Scotland. (Present in Ireland, Kilkenny, Dungannon, Lough Allen coal-fields.) Also in Belgium, Rhenish Provinces, and Silesia, with numerous marine shells.

STAGE D.—*Millstone Grit Series* of England and Wales. 3,500 feet in Lancashire; "Moorstone Rock" of Scotland; "Flagstone-series" of Carlow and Kilkenny; Millstone-grit of Fermanagh and Leitrim, with coals and marine shells.

STAGE C.—*Yoredale Beds*. 3,000 feet in Lancashire; Upper Limestones and "Lower Coal and Ironstone series" of Scotland; Shale series of Kilkenny and Carlow; Ironstone shales of Lough Allen, with marine shells.

STAGE B.—*Carboniferous Limestone*. Mountain Limestone of Derbyshire; "Scaur Limestone" in Yorkshire; "Lower Limestone" (Roman camp) of Scotland; Carboniferous Limestone of Ireland.

STAGE A.—*Lower Limestone Shale* of England. Calciferous Sandstone series ("Tuedian," Tate) of north of England and Scotland; Lower Carboniferous Sandstone, north of Ireland; Lower Carboniferous slate, with Coomhola grits, with marine shells, south of Ireland. (In Scotland, estuarine or lacustrine.)

Palæontological Results.—On making a census of the Molluscan and other fossils from the various stages above that of the Carboniferous limestone (stage B) as determined by the palæontologist of the Geological Survey, some interesting results were obtained, showing the prevalence of marine conditions up into stage E, and a general change in the character of the fauna in the succeeding stages. Including only the area of the British Islands, it was found that no fewer than thirty-seven genera, with seventy-four or seventy-five species, of decidedly marine forms, occur in the Gannister beds (stage E), of which all the genera and about forty species were known in the stage of the Carboniferous Limestone. The series includes *Phillipsia*, which has been found by Dr. F. Römer, in the representatives of stage E in Silesia. On the other hand, of the whole number of species in stage E (Gannister beds), only six are known in the overlying stages F and G, these being characterised by the prevalence of bivalves of supposed lacustrine or estuarine habitats, variously called *Unio* and *Anthracosia*. Of the few species of marine genera known in stage F (Middle Coal-measures), about five or six species are peculiar to itself, according to the determination of the late Mr. Salter. Such a remarkable difference in the fauna of the Upper and Middle Coal-measures, as compared with that of the Gannister beds, constituted, in the author's opinion, sufficient grounds for drawing a divisional line between these two divisions of the Carboniferous series. Of the several existing methods of classification adopted by different authors, none of them appeared sufficiently to recognise the palæontological distinctions and characteristics of the several formations. The large number of genera and species which are now known to range up from the Carboniferous Limestone into the Gannister beds, and no higher, indicated the proper horizon for a divisional line, in fact a palæontological break at the top of the Gannister beds. On the other hand, the mineral and palæontological differences between the Carboniferous Limestone and the overlying Yoredale series were sufficient to justify their separation into distinct divisions; while the Yoredale, Millstone-grit, and Gannister series are related by close mineral and palæontological resemblances. With a view, therefore, of bringing the classification of the Carboniferous series into harmony with the character of the representative faunas, and the physical features of the successive stages, the author suggests that stages C, D, and E, composed of essentially marine beds, should be united into a Middle Carboniferous group; while stages F and G would remain as at present, in the Upper Carboniferous, their fauna being essentially of fresh water. In the discussion which followed, Professors Ramsay, Boyd-Dawkins, Prestwich, and Hughes seemed to doubt the feasibility of permanently maintaining the lines of demarcation laid down in the paper.—On coal-pebbles and their derivation, by H. K. Jordan, F.G.S.

Physical Society, April 28.—Prof. G. C. Foster, president, in the chair.—Mr. W. Ackroyd described some methods of studying selective absorption in relation to the doctrine of aggregation. After referring to the absorption of iodine vapour and iodine violet solutions he showed that an analogy exists between these solutions and the aniline dyes, and a method was indicated by which the approximate size of the particles affecting light might be estimated.—Prof. H. McLeod exhibited several forms of apparatus which he has, in conjunction with Lieut. G. S. Clarke, R.E., arranged for determining the speed of machinery, &c., from observations made on the figures produced by combining their motion with that of a vibrating body; a description of them has already been communicated to the Royal Society. If a uniformly-moving point of light be reflected from a mirror attached to a tuning-fork vibrating in a plane at right-angles to the motion of the point, the reflected image will appear as an ordinary single wave, and a double figure of the form of a series of figures of eight, caused by the overlapping of two waves, will be formed if a series of points of light move uniformly with such a velocity that a point passes over two intervals during an odd number of vibrations of the fork. If equidistant perforations be made in a circle on a disc which is attached to a rotating axis and the number of vibrations of the fork be known, the form of figure reflected on to the screen will, theoretically, give the requisite data for determining the rate of rotation of the disc, and further, a slight increase or decrease in this rate causes the figure slowly to move in the same or opposite direction to the disc. If the fork make 3,600 vibrations and the disc 100 revolutions per minute, the circle must be divided into seventy-two equal intervals, but for such a number as 101 revolutions 71·287 intervals are needed. This fact would introduce some difficulty in preparing an apparatus for measuring the velocity of rotation so as to give the speed in whole numbers per minute, but it may be obviated by ruling convergent white lines on dark paper and so wrapping it round a cylinder that one line is parallel to the axis, an arrangement which gives every possible subdivision of a circle between any given intervals. The figures are then observed by examining these lines through a narrow slit in a light opaque screen attached to a tuning-fork or reed vibrating in a plane parallel to the axis of the cylinder. This observing apparatus is moved parallel to that axis until the figure remains stationary, when the number of rotations is read off on a graduated scale. Conversely, if the number of rotations of the cylinder is known, the period of the tuning-fork can be determined. Incidentally Prof. McLeod explained a simple method of causing a fork to vibrate, and the manner in which they have succeeded in maintaining the vibrations of a reed. It was found that variations in temperature influence the determinations, inasmuch as they cause the period of the fork or reed to vary. When the former is used it becomes necessary to deduct 0·011 per cent. of the result for each degree centigrade of rise above the temperature for which the fork is set, and 0·0277 per cent. when employing a reed.

Anthropological Institute, April 24.—Mr. John Evans, F.R.S., president, in the chair.—Dr. John Rae read a paper on the migrations of the Esquimaux. The chief subjects of Dr. Rae's remarks were two papers read before the Ethnological Society, twelve years ago, the one by himself, the other by Mr. Clements R. Markham. Dr. Rae considered that Mr. Markham's present view was in accordance with what he (Dr. Rae) advanced in 1865, viz., that the route of the Eskimo must have been along the coast of America, across the Strait (northward) to Banksland, and thence to the Parry Islands, &c., where so many traces of them remain. He gave some information regarding various peculiarities of the Eskimos, and exhibited a stone lamp, with a curiously-shaped piece of stone, used for adjusting the wick, which consists of a species of fibrous moss (sphagnum) brought with the lamp from Repulse Bay more than twenty years ago. The lamp when lit gave a clear bright flame, from each of the three bits of moss used, without any perceptible smoke. These form valuable articles of barter by the Eskimos in the neighbourhood of such localities, with the more distant natives, for they seem to be in almost universal use, from Behring Strait eastward to Hudson Bay.—Mr. Robert B. Holt then read a paper on earthworks in Ohio, and Prof. Busk, F.R.S., described some skulls from the same place.—The following gentlemen took part in the discussions :—Mr. Hyde Clarke, Mr. Allen, the president, and others.

PARIS

Academy of Sciences, April 30.—M. Peligot in the chair.—The following papers were read :—On a phenomenon of insolation of the eye, which has not hitherto been explained, by M. Chevreul. A few days before St. Bartholomew's day the Prince de Navarre (afterwards Henri IV.) being with the Duc d'Alençon and the Duc de Guise, at the Louvre, playing dice, they twice saw blood spots on the dice, whereupon they separated, in alarm. M. Chevreul explains the phenomenon as an effect of contrast of colours in sunlight, and gives some experiments in illustration. Material black appears red in reflecting white light.—On carbuncular disease, by MM. Pasteur and Joubert. Are the effects due to the bacteridium or to a virus? The bacteridium may be multiplied indefinitely in artificial liquids, without losing its action on the system, so we cannot suppose it accompanied with a soluble substance (or virus) producing, jointly with it, the carbuncular effects.—Probable consequences of the mechanical theory of heat, by M. Favé. He seeks to explain the phenomena attributed by M. Boutigny to the "spheroidal state," by supposing that the calorific waves of the sidereal ether have a repulsive action on ponderable matter. Take the case of a little water poured into a red-hot capsule. The heated metal gives radiant heat, *i.e.*, impresses the ether with waves which are rapidly propagated. This motion upwards counterbalances that due to the weight of the water. The distance between liquid and metal depends on the *vis viva* developed by the ether. The motion of oscillation is combined with one of varying rotation due to the resultant of the repulsive forces not passing through the centre of gravity of the globule.—On a new deposit of liquid mercury, indicated in the upper valley of l'Herault by M. de Quatrefages, by M. Leymerie.—On a new Arctic expedition of M. Nordenskjöld, by M. de Saporta. He intends prolonging his exploration as far as Behring Straits. The expedition is to leave Sweden in the summer of 1878.—Electro-silicic light, by M. Planté. He calls attention to the bright light produced when one or other electrode of his secondary batteries is applied to a tube or plate of glass. The glass is decomposed, and the luminous effect is probably due to incandescence of the silicium.—On a process of solidification of sulphide of carbon, by M. Mercier. Treating oils with a little protochloride of sulphur, a transparent solid matter is got, with nearly the elasticity of caoutchouc. If a volatile liquid be added at the moment of mixture, as benzine, oil of petroleum, or sulphide of carbon, the solidification takes place all the same, and the volatile liquid is imprisoned as in a net-work, from which it can only escape slowly. The mixture may hold even 70 per cent. of sulphide of carbon.—On the treatment of phylloxerised vines with sulpho-carbonate of potash, by M. Fatio.—On the rooting out of phylloxerised vines, by M. Cornu.—On the comparative structure of the roots of American and indigenous vines, and on the lesions produced by the phylloxera, by M. Foëz.—On the regeneration of phylloxerised vine-stocks by the employment of sulpho-carbonate of potash, by M. Gueyraud.—M. Dumas presented the first document from a Commission charged by the Emperor of Brazil, to determine the geographical positions of the principal points of the empire. It treats of the position of Barra do Pirahy, relatively to Rio de Janeiro Observatory.—Observations of Comets II. (Winnecke) and III. (Swift, Borrelly), by M. Wolf.—On some observations of solar spots, by M. Denza. This confirms M. Janssen's observations.—On the surfaces whose principal radii of curvature are functions of each other, by M. Mannheim.—Investigation of the law which a central force must follow for the trajectory which it produces to be always a conic, by M. Darboux.—On the laws of Kepler; solution of a problem proposed by M. Bertrand, by M. Halphen.—Reply to a note of M. Kirchhoff on the theory of elastic plates, by M. Levy.—Singular solutions presented in the problem of curvilinear motion of a point under the action of a central force, by M. Boussinesq.—On substances capable of being produced at a temperature above that which causes their complete decomposition, by MM. Troost and Hautefeuille. Examples are: protoxide of silver, ozone, protochloride of platinum, sesquichloride, protochloride, and subfluoride of silicium.—Process of industrial preparation of pure salts of alumina, by M. Ducla.—On monochlorised acetones, by M. Etard.—Experiments proving that the septicity of putrefied blood is due to figured ferments, by M. Feltz.—On the fixation of tannin by vegetable tissues, by M. Müntz. The tissue of champignons, especially, may be "tanned" into a kind of vegetable leather.—On gaseous exchanges between plants and the atmosphere; reply to critical observations of M. Barthelemy, by M. Merget.—Researches on the absorption and emission of gases by roots, by MM. Deherain and Vesque.—On the spontaneous and regular movements of a submerged aquatic plant, *Ceratophyllum demersum*, by M. Rodier.—On the presence of mercury in the springs of Rocher (Puy-de-Dôme), by M. Garrigou.—On a case of hereditary hemiteria, by M. Martinet.—On increase of the production of springs, by M. 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THURSDAY, MAY 17, 1877

SCIENCE AND WAR

RECENT wars have had particular interest for the man of science. If we go back some fifteen or twenty years and consider the different wars which have unfortunately occurred since that time, we shall find connected with each one of them certain features which undoubtedly mark progress in the art of killing and wounding. Some argue—and on very good grounds, no doubt—that the more sharp and terrible warfare is made the more speedily must it come to an end, and hence look with favour upon the means taken every day to render weapons more destructive and the soldier more cunning in his dangerous trade. We do not propose to discuss this argument, nor to enter at all into any comparison between the wars of our forefathers and those of to-day, but at a crisis like the present we need hardly apologise for bringing before our readers some points illustrating the marked influence of science upon modern warfare.

Starting from the close of the Crimean war, the first in which the electric telegraph was employed, we find ample examples of the assistance furnished to the soldier by scientific research. One instance taken from the war of 1858 is especially interesting. The Austrians held Venice at the time, it may be remembered, and to protect the harbour, torpedoes were laid down. The torpedoes were fired by electricity, and contained gun-cotton, this being the first instance on record of the employment of electric torpedoes and of the newly-invented nitro-compounds. Nor was this all. The torpedo-system devised at Venice by the Austrian engineers had yet another point of scientific interest. A camera obscura was built overlooking the harbour, and upon the white table of this instrument were reflected the waters of Venice. As the torpedoes were sunk one by one a sentinel in the camera noted the place of their disappearance with a pencil, giving each torpedo a consecutive number. A row-boat in the harbour described a circle around the sunken torpedo indicating the zone of its destructive power, and the sentinel again, with his pencil, made a corresponding ring upon the camera table. In the end, therefore, while the harbour itself was apparently free from all obstruction, a very effective means of torpedo defence was established, the key of which was only to be found in the camera obscura. The sentinel here had wires in connection with every torpedo, and was in a position to fire any one as soon as he observed—by means of the camera—the presence of a hostile vessel within the limits of any of the circles marked upon his white table.

In the American war of 1860, the electric torpedo, invented but two years before, played a most conspicuous *rôle*, and formed indeed with the use of big guns and monitor ironclads, one of the most important features of the struggle, at any rate from a scientific point of view. The war of 1866, when the Austrians suffered such a terrible defeat at the hands of the Prussians, will long be remembered as a combat between the old muzzle-loading rifle and the breech-loader, in which the latter was victorious. The Franco-German struggle of 1870 again, though marked by the employment of no special arm, if we except the mitrailleuse, was assisted by important applications of science; to wit, the reproduction, by means of photo-lithography of the French ordnance maps and plans, which were distributed in thousands throughout the German army, and the establishment in France of *la poste aérienne* to communicate with the besieged garrison of Paris. The regularity with which the mails left Paris *par ballon monté*, must still be fresh in the memories of our readers, the publication of correspondence from the French capital being maintained in our journals during the whole period of the investment. From September 23 to January 28, when Paris was practically cut off from the rest of the republic, no less than sixty-four balloons left the city with passengers, mails, and pigeons, and of these only three were lost, while five were captured. The return-post by "homing pigeons" was hardly so regular, but nevertheless half the number of despatches given in by correspondents at Tours and elsewhere, or in other words 100,000 messages, were by the unflagging energy of the postal authorities carried into the beleaguered capital. The despatches, most of them as brief as telegrams, were distinctly printed in broad sheets and photographed by the aid of a micro-camera; impressions upon thin transparent films were then taken and rolled in a quill attached to the tail of the winged messenger which was to bear them into Paris. Arrived at their destination, the tiny photographic films were enlarged again by the camera, and the despatches being once more legible, were distributed to the various addresses.

The present Russo-Turkish war cannot well be less interesting than those that have so recently preceded it, and we may especially point out two directions in which fresh examples of scientific warfare will probably manifest themselves—in connection, namely, with the cavalry pioneer and the Whitehead torpedo. Both of these will probably be seen in warfare for the first time, and before many days are past we may hear of their doings in action.

The cavalry pioneer must not be confounded with the Prussian Uhlan who played so conspicuous a part in the last war. The ubiquitous Uhlan, terrible as he was, did not work the injury which some of the Cossacks will have it in their power to inflict if accoutred as pioneers. These are selected from the smartest and most daring troopers, lightly armed and well mounted. In a belt round their waists they carry a few pounds of gun-cotton or dynamite, and with this highly destructive explosive they may work incalculable harm. A small charge of gun-cotton placed simply upon a rail and fired with a fuze suffices to blow several feet of the iron to a distance of many yards, thus rendering the railway unserviceable on the instant. A trooper may dismount, place a charge at the base of a telegraph pole, fire it, and be in his saddle again within sixty seconds. Wires may thus be cut and communication stopped in the heart of an enemy's country by fearless riders, who have but to draw rein for an instant to effect the mischief, while lines of railway in the neighbourhood are entirely at their mercy. Even light bridges and well-built stockades may be thrown down by the violent detonation of compressed gun-cotton, and forest roads considerably obstructed by trees thrown across, which are never so rapidly felled as when a small charge of this explosive is fired at their roots.

The influence of the Whitehead torpedo, of which we have heard so much of late, will likewise be felt for the first time during the present war. An implement so ingenious in its character that, as Lord Charles Beresford the other day happily remarked, it can do almost anything but talk, is in the possession of both belligerents, and will doubtless be heard of ere long on the Danube and in the Black Sea. These torpedoes are manufactured at Fiume on the Mediterranean, and, like Krupp guns, are to be purchased by any one who chooses to pay for them.

The British Government manufactures its own Whitehead torpedoes in this country, having paid several thousands of pounds for the privilege. The machinery inside this torpedo is still a secret, which is strictly maintained by our Government, but the principle of the invention is well known. It is a long cigar-shaped machine measuring a dozen feet and upwards. In the head is a charge of some violent explosive, such as gun-cotton, or dynamite, which explodes as soon as the torpedo strikes an obstacle. The motive power is compressed air, which is forced into the machine by powerful air-pumps, immediately before the torpedo is discharged into the sea, no less than 600 lbs. on the square inch being the pressure exerted. The Whitehead is shot from a tube, and moves through the water as straight as a dart, the compressed air working upon a screw in the tail of the machine. The delicate machinery permits the torpedo to swim at any depth below the surface that may be desirable, and it flies straight in the direction it is aimed, at a speed of something like twenty miles an hour. If it fails to strike the foe, then the intelligent apparatus at once rises to the surface, becoming innocuous as it does so, and may in this condition be captured without difficulty.

A torpedo of this sort striking the sides of an ironclad would almost infallibly send her to the bottom, and although it has been proved that a network or crinoline around the ship is capable of retarding the progress of a "fish" of this nature, and exploding the same harmlessly in its toils, it is obviously a very difficult matter thus to protect one's craft. Against heavy torpedoes, indeed, there seems no way of defence at all (the Whitehead generally carries a charge of 70 lb. or 80 lb., but moored torpedoes may contain a 500 lb. charge), and therefore Turkish vessels will have to give Russian ports a wide berth. All must remember how the magnificent fleet of the French was kept at bay by the torpedoes of the Germans in the North Sea in 1870, and the Black Sea ports are no doubt similarly protected. So demoralising is the dread of the torpedo with sailors apparently, that they will dare anything rather than venture into waters which conceal these cruel foes.

H. BADEN PRITCHARD

THE OWENS COLLEGE UNIVERSITY QUESTION

IN his address on Tuesday last week, at the London University, the Chancellor noticed in dignified and sensible words the proposed application of Owens College to the Government for a Charter of Incorporation as a university, either by itself, or as the centre of a family of northern colleges. Nothing could well have been more unfortunate or ill-judged than the furious onslaught of Mr. Lowe, the member for the University, in the *Fortnightly Review*. The complaint of the Manchester people is that the London system, however suitable in itself, hampers the educational activity and usefulness of institutions capable of an independent existence, and it was scarcely decent for the member for that university to step forward in her interests as a mere partisan of the *status quo*. In fact there is no antagonism. Manchester has never denied that it is a good thing that there should be a university in London to examine all comers. She has said that she thinks it a bad thing for institutions with a sufficient permanent teaching staff, a large enough number of students and a solid establishment in the district to which they belong, to have to shape their work according to the ideas of any central university that must suit all comers. Mr. Lowe is the one member of Parliament who should have held his tongue on the matter till he was forced to speak, because a hasty utterance on his part could not but seem to compromise his University. Lord Granville took pains to remove the injurious impression of an unworthy jealousy in London which Mr. Lowe's article could scarcely fail to create. He tells us that London feels "absolutely no objections of a merely jealous character," and that London would have a "very friendly feeling to any university which, after due deliberation and with a sound regard to the real advantages of education, may hereafter be established." In that wise and sensible attitude it is open to the University to consider either of the two schemes suggested for the northern university. The first of them, which is that favoured by the college authorities, is that Manchester should be created a university much as Glasgow is. According to the views of the supporters of that scheme we should be prepared to multiply our universities as the Scotch have done, by chartering one in any large town where its students and its endowments, its history and its reputation offer equally solid guarantees of permanence. The other is that Manchester should be the capital—*primus inter pares*—of a new northern university on the original affiliation basis from which London has departed. The weakness of the affiliation principle is that it is scarcely in nature that it should not gradually relax, so that colleges should be affiliated on easier and easier conditions till it becomes useless to keep up the farce. But both schemes, the latter of which, indeed, is Dr. Carpenter's, are practicable —both worthy of careful consideration and discussion— and it is pleasant to see that the University of London, through her Chancellor, disavows any settled policy of obstruction.

Lord Granville reminded his hearers of what most people have forgotten—the history of the incorporation of the University. It was a subject of excited debate in this country and in Parliament, for ten years from its first inception. The project was started in 1825. Funds were then raised by subscriptions in 100*l.* shares, and the institution was in activity in 1828. In 1830 an application was made to the Crown for a charter, and the charter as prayed for had gone through nearly all the necessary preliminary stages, when its progress was stayed by the opposition of Oxford and Cambridge. In 1833 the application was renewed, and it was supported by an address to the throne from the City of London. It was opposed by Oxford and Cambridge, by the Royal College of Surgeons, by the teachers of medicine and surgery in the London hospitals, and by others. The matter was referred to the Privy Council, and argued before it in 1834. There was no question then of anything so futile as what has been once or twice suggested for Owens College, the title of university, without the privilege of degrees. The Privy Council found the subject surrounded by difficulties, and adjourned its consideration. Shortly after, Lord Melbourne's Ministry, which was friendly, retired from office, and Sir Robert Peel's, which took the view of the old universities, succeeded. An address to the Crown, however, was carried against the Ministry by 246 to 136, on the motion of Mr. W. Tooke, praying that a charter might be granted to the University of London, with no restriction but that they were not to confer degrees in divinity. The Privy Council was asked to report on the subject, but the report was delayed, and before they presented it Lord Melbourne returned to power. In August, 1835, the Chancellor of the Exchequer, Mr. Spring Rice, communicated to the Council of the existing University College that Government proposed to incorporate by charter as a university in London, a body of gentlemen eminent in learning and science, with the power of examining and granting degrees in arts, medicine, and laws to students of *certain colleges in London, therein named*, and of others existing throughout the country to be afterwards recognised, as well as of the schools of professional education. This university was to be supported by an annual grant. There were to be no religious tests. The existing body, which called itself the University of London, received a charter as a college and was named as one of the colleges entitled to submit students for examination. The two charters to the new university and the new college were issued on November 28, 1835. They have been several times modified. The list of affiliated colleges was always large, and as the Senate of the University had no control over the affiliated colleges it grew unwieldy, institutions of the feeblest character receiving affiliation. In 1863 a charter was granted empowering the Senate to admit persons not educated in affiliated colleges to examination, and this decision creates the University of London of to-day as distinguished from the institution of the same name founded in 1835. About half the students now come from affiliated Colleges and half from anywhere or nowhere. The examinations must be fixed in view of this fact. Examiners must take into account as a most vital matter the books on the subjects of their examination which are readily accessible to students, and they cannot shape their examinations in view of the practice in teaching of any one or more of the affiliated colleges. We hope that the proposed university of the north may have a shorter novitiate, and that she may be conducted in as elevated a spirit and with as resolute a desire to promote the interests of literature and science as the University of London has been. It would have been a painful spectacle if the youngest of our Universities, forgetful of her own early struggles, had spent her energies in an opposition which Oxford and Cambridge have thought unnecessary or unworthy of them. The speech of her Chancellor leads us to hope that the claims of the proposed new university will be considered calmly and on their merits.

NICHOLSON'S "LIFE-HISTORY OF THE EARTH"

The Ancient Life-History of the Earth; a Comprehensive Outline of the Principles and Leading Facts of Palæontological Science. By H. Alleyne Nicholson, M.D., D.Sc., M.A., Ph.D. (Gött.), F.R.S.E., F.L.S., Professor of Natural History in the University of St. Andrews. (Edinburgh and London: William Blackwood and Sons, 1877.)

THERE is no feature in which the ordinary geological manuals in common use in this country are more deficient than in the sketches which they give of the leading characteristics of the animal and vegetable life of the successive periods which they describe. The truth of this remark will be made strikingly apparent by a comparison of the works in question with some of the best German treatises on geology, such as those of von Hauer and Credner, and still more if we examine them side by side with that most excellent of text-books, Prof. Dana's "Manual of Geology."

Some writers on geology in this country would indeed appear to hold the opinion that, since the succession of geological formations was first determined in our own islands, an appeal to the facts of British stratigraphical geology must in every case be final in deciding all difficulties which may arise concerning the definition and limits of the different systems of stratified rocks in every part of the globe. Hence the controversies which have taken place in this country concerning the boundaries between the Cambrian and Silurian, the Devonian and Carboniferous, and the Permian and Trias have acquired an altogether factitious importance, and undue weight has been attached to the interpretation of some obscure section, the significance of a local unconformity, or the appearance—often a fallacious one—of a gradual transition between two sets of beds, while far more suggestive facts connected with the relations of the fossil contents of the two series of rocks are too often altogether lost sight of.

But it cannot be too strongly impressed upon the minds of English geologists that the district in which a system of strata is first detected may not necessarily be the one in which it is best adapted to serve as the type of that series; that as a matter of fact the best illustration of the features and relations of the Cambrian and Silurian is to be found, not in Wales, but in Bohemia; and of the Devonian, not in Devonshire, but in the Eifel. English students, too, need to be reminded that the classification of the stratified rocks is based not upon the occurrence of certain physical breaks, in the continuity of a series of beds, which are often, indeed, of very local character and small importance, but upon the great principle that each formation is characterised by a well-marked and distinctive fauna or flora. Concerning the fact, position, and significance of many of the physical breaks in the succession of formations, the ablest field-geologists, such as Sedgwick and Murchison, Jukes and Godwin-Austen, have frequently arrived at very opposite conclusions; and the importance which has been attached to these discussions on points of details has doubtless led many to entertain a notion of the instability of the foundations of the geological systems of classification which is very far from having any real foundation in fact. For it must not be forgotten that, however certain questions now pending concerning the nomenclature of the Welsh strata may eventually be decided—and these questions of nomenclature and priority are, after all, of very secondary importance—the grand fact first clearly determined by the discoveries of the illustrious Barrande in Bohemia, that there can be distinguished in the series of older Palæozoic strata three great divisions, each characterised by a well-defined fauna, is quite independent of these controverted points, and its value cannot be affected in any way by their decision either one way or the other.

It will be manifest from what we have said above that we regard the present work of Prof. Nicholson as dealing with a subject in connection with which the want of a competent text-book in this country has long been a serious evil; and of the general accuracy and reliability of the information supplied by this convenient little volume we can also speak in terms of high commendation.

Prof. Nicholson has wisely availed himself to the fullest extent of woodcut illustrations in aid of his descriptions of the fossil forms; and the 270 engravings, many of them containing illustrations of a number of different species, will be a great boon to the geological student. Some of these woodcuts now appear for the first time, but others have already done duty in the author's previous writings. We cannot unfortunately award anything like equal praise to all these illustrations, for while some of them are of exquisite truthfulness, detail, and finish, certain others are so coarsely executed and so wanting in character, that it is a marvel to us how so accomplished a naturalist as the author could have ever permitted them to disfigure his pages. There is one omission in connection with the illustrations, which will greatly detract, we fear, from the value which they would otherwise have for the student, namely, the absence of indications of the number of times which the scale of the drawings is magnified or reduced from that of the original objects. Every one engaged in teaching is aware what erroneous notions concerning fossil forms are often propagated by want of attention to this detail.

In his discussion of the characters distinguishing the flora and fauna of each of the great geological periods, Prof. Nicholson is usually very clear in his descriptions and happy in his choice of typical forms. The greatest danger which besets the writer of such a work as the present is that of overwhelming the student with masses of detail, unrelieved by those broader generalisations which may serve to aid his memory in grouping the facts about convenient centres. Had Prof. Nicholson in the present work prefaced each of his descriptions of the great geological periods with a succinct statement of its leading palæontological characteristics, and also furnished similar summaries for the greater epochs, we cannot but think that the work would have been far better adapted to the wants of the student, and at the same time its suitability for general readers would have been in no wise impaired.

The references to authorities at the end of the chapters will be found useful by all classes of readers, and the general remarks on the "Principles of Palæontology" with which the work opens will sufficiently prepare those who may be totally unacquainted even with the fundamental facts of geological science for a profitable perusal of the succeeding chapters. The work before us constitutes a popular exposition and summary of the facts of palæontology, suitably arranged for beginners; but as a text-book for the more advanced student of the science, it still leaves much to be desired. We search it in vain, for example, for information on many important questions, such as the classification of the multifarious forms grouped under the name of *Ammonites*, and we sometimes find obsolete names employed for certain genera and species. There are certain obvious errors and omissions which will doubtless be corrected and supplied in a subsequent edition of the work—such, for example, as the table of Cambrian strata on p. 79, and the absence of all notice of the remarkable Devonian fossil, *Calceola sandalina*.

As a compact and popularly written introduction to a very important department of science, Prof. Nicholson's new work may be safely recommended; and it is well worthy to take its place among that series of useful manuals for which we are already indebted to its industrious author.

OUR BOOK SHELF

Geological Survey of Canada. Report of Progress for 1874-75. Alfred R. C. Selwyn, F.R.S., F.G.S., Director. (Published by Authority of Parliament, 1876.)

ALTHOUGH Mr. Selwyn, like his predecessor, Sir William Logan, has the highest possible ideal of the importance of pure geological mapping, the necessity for the rapid exploration of a vast unsurveyed new land simultaneously with the development of rich coalfields, compels him to employ two very different systems of working. With a staff of only ten geologists, two-thirds of whose time is engrossed by topographical preliminaries, the usefulness of the survey as a whole must depend to a great extent on the judicious determination of the degree of importance attached to the details of its various parts. Accordingly, Mr. Selwyn has confined the detailed mapping to the settled eastern sea-board, carrying on at the same time reconnaissances in the central and western regions, where complete maps will not be demanded for some time to come.

During the past year Mr. Selwyn has been able, in addition to his administrative duties, to overtake some field-work, chiefly among the palæozoic rocks of New Brunswick and the coalfields of Cumberland and Sydney. The Report contains two geological maps of portions of the Cape Breton Coalfield, by Messrs. Robb and Fletcher, exhibiting all the completeness of the British coalfield maps.

Mr. R. W. Ells furnishes a map and report on the hematite ores of Carleton County, New Brunswick. The ore appears to occur in veins along the strike of highly-inclined Silurian rocks.

Mr. Henry G. Vennor has been surveying in the Laurentian region of Frontenac and Lanark Counties, and embodies the results of his labours in a map and report. It appears that apatite mining in this district has recently ceased to be a profitable industry. Mr. Vennor sees the cause of failure in the injudicious and costly manner in which the mining was carried on. Iron ore (magnetite) occurs at Eagle Lake in a bedded form, associated with hornblendic and dioritic rocks.

Mr. Robert Bell and Mr. Joseph Spencer describe the country between the head-waters of the Assineboine River and Lakes Manitoba and Winnepegosis. During a rapid survey of this little-known tract, they recorded the occurrence of Laurentian schists and rocks of Huronian, Devonion, and Cretaceous age. They also made many valuable observations on the superficial deposits, as well as on the physical geography of the region. An interesting point in their report is the frequency of old beaver-dams in places where there is now little or no water—an evidence of the former greater humidity of the climate.

In British Columbia Mr. James Richardson continued his explorations. He traversed metamorphic crystalline rocks (auriferous) extending over seven degrees of latitude and six of longitude. The complicated structure of the Nanaimo coalfield was further investigated, but the work is not yet complete.

Mr. J. Lionel Smith reports on the salt manufacture and trade of Ontario, and makes some interesting and useful comparisons between the various processes for the treatment of the brine in Canada and elsewhere.

Mr. J. Harrington closes the volume with notes on Canadian rocks and minerals.

R. L. JACK

The Schools of Forestry in Europe. A Plea for the Creation of a School of Forestry in Connection with the Arboretum at Edinburgh. By John Crombie Brown, LL.D., &c. (Edinburgh: Oliver and Boyd.)

THIS pamphlet is written in the form of a letter or address to the Lord Provost of Edinburgh and the promoters of the Arboretum at Inverleith, and is in short a strong argument in favour of the formation of a school of Forestry to be connected with the Arboretum. Dr. Brown shows that in France, Spain, Italy, Austria, Poland, Russia, Finland, Sweden, and in fact in almost every country except Great Britain, its Colonial dependencies and the United States of America, such schools exist under Government authority, and it is in these very countries that such schools would be of immense utility. The proposed curriculum of three years' study sketched out by Dr. Brown as likely to prove advantageous is, in the main, good, but we think that the French and German languages should be taken before the end of the third year. The notices of the arrangements and systems of studies in the various Continental forest schools are not without interest. Dr. Brown concludes his "plea" with a comparison of the English and Continental forests; the extent of the latter, together with the threatened lack of fuel by the extinction of forests as against our supplies of this necessary article from coal mines, being, no doubt, among the principal causes of the decrease of forest training in this country. The lack of special literature on the subject in the English language also compares badly with that of the Continent.

Unser Sonnenkörper nach seiner physikalischen, sprachlichen und mythologischen Seite hin betrachtet. By Dr. Schmidt. (Trübner, 1877.)

DR. SCHMIDT has more learning than method. In fact, he belongs to that school of paradoxers who are less common in Germany than in this country. He proposes to show that the sun is a cold inhabited body, heat being developed by the friction of its rays against the earth and other celestial bodies. Upon this physical theory he superimposes his mythological one. Words which have a slight resemblance in sound and meaning are gathered together from all parts of the world and assumed to be connected in spite of their belonging to different families of speech. Out of this hodgepodge are extracted such conclusions as that the sun-god was believed to illumine the dead in Hades or that the snake represented the return of Apollo to the light of day. But the philology of the writer may be easily appreciated when we find him speaking of "the Armeno-Caucasian family, to which belong not only Semites and Aryans, but also some Turanian tribes," and intimating that the roots of the Chinese language are allied to those of the "Armeno-Caucasian." As might have been expected, Dr. Schmidt is not always right in the words he quotes from the numerous languages, ancient and modern, which he has laid under contribution.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Passage of Plants Across the Atlantic—Haplomitrium Hookeri, Lyell

PROF. UNGER arrived at the conclusion that in Tertiary times there was a passage of plants from America to Europe. A plant found by myself last year in the Island of Dominica, West Indies, led me to think it probable that there had been an extension of at least one plant in the opposite direction. The plant to which I refer is one of the Hepaticæ, *Haplomitrium hookeri* of Lyell. It differs so much from other Hepaticæ that I was able approximately to identify it on the spot where I found it in considerable abundance. Should it prove to be specifically distinct, my remarks may still, to some extent, hold good. It was growing in a dark, moist, shady spot on the north side of a mountain at an elevation of about 4000 feet. *H. hookeri* is generally distributed over the North of Europe, but I cannot find that it has ever before been found out of Europe. Dr. Oliver kindly informs me that there are only European specimens in the herbarium at Kew. I have failed in obtaining information of its occurrence either in North or South America, or in the intermediate islands. Nees ab Esenbeck, in his "Synopsis Hepaticorum," whilst recording a large number of Hepaticæ from the West Indies, mentions *H. hookeri* only from Europe. Now it is by no means an inconspicuous plant, and it seems altogether unlikely to have been overlooked by such careful observers as Swartz and others who have studied the Hepaticæ of the West Indies. Hence I draw the following inferences, to which may be attached a greater or a less amount of probability. 1. That the biological centre for *H. hookeri* is Northern Europe. 2. That it has thence crossed the Atlantic in a rather narrow zone. 3. That it did not reach the Continent of America. This, of course, is subject to correction. It may have been found there. From the great extent of territory and variety of climate on the mainland, I think if it had ever reached America it would still be found there. 4. That it may have reached the West Indies and have died out from Cuba, Jamaica, and other islands, through the prevalence of dry seasons, before the lower Cryptogamic plants were studied by competent botanists. 5. That it has remained in Dominica because of the altogether peculiar moisture of the climate in that island. 6. That it has not hitherto been found in Dominica because, from some reason unknown to myself, botanists seem to have neglected this *true* pearl of the Antilles, matchless in the beauty of its natural scenery, and in the wealth of its Cryptogamic flora.

H. hookeri is noticed as peculiar in not recovering its freshness when moistened after having been dried. This I found to be the case. On being carefully moistened about eight months after it was collected and dried, it remained flaccid, whilst the rest of the mosses and Hepaticæ from Dominica, when similarly treated, looked as fresh as when they were gathered. But *H. hookeri* exhibited another peculiarity even more remarkable, for it alone of all the Muscineæ that I brought home, grew and produced fruit after so long a period of desiccation. The fruiting parts of a specimen which I sent to the herbarium at Kew were entirely developed in a moist case on the table at which I am now writing. It seems as if the plant, incapable of the imbibition or intussusception of moisture sufficient to restore the freshness of its foliage, nevertheless retained, in a very unusual degree, its capacity for such development as might secure the continuance of its species. Such a speciality no doubt favours the suggestion that *H. hookeri* may have crossed from the East, but I confess myself inclined to be suspicious when coincidences run too much on "all fours." I found many mosses in Madeira and several lichens in Jamaica, which I have been quite unable to distinguish from British species. These may be common cases of widely distributed forms. *H. hookeri* does not appear to be of this class.

HENRY H. HIGGINS

Rainhill, May 2

Patenas in Ceylon

I REGRET that through an accident I was able only yesterday to read Mr. Heelis's reply to my letter on the subject of patenas in Ceylon. I have not a copy of my own letter by me and therefore cannot speak with certainty, but I believe that I only suggested that the cropping out of the thick band of quartzite amongst the gneiss was sufficient to explain the existence of many of the larger patenas in the Kandyan Province. The immense majority of the smaller and more isolated patenas I am fully aware cannnot be explained on my supposition, nor can they at present be explained on *any* reasonable supposition. I do not think, however, that even the most superficial observer can have any doubt as to the large patena mentioned in my letter between Pussellawa and Rambodde, covering several thousands of acres, being entirely due to the quartzite band that lies above it. In regard to the Dimbula patenas it is no doubt true that gneiss is almost always found underlying the soil, but this does not prove that the patena soil is derived from the gneiss. The depth of the rock below the surface is against this view, especially when taken in connection with the fact that I was never able to trace in the case of patenas as I did in scores of cases of jungle land, in railway and road cuttings throughout the Kandyan Province, the gradual changes from the hard rock upwards to the surface, which show that the soil has been produced by the disintegration of the gneiss *in situ*. The denuding forces at work among these mountains are so excessive (according to an estimate made by myself at Pussellawa the denudation was no less than ten inches in thirty years on land cleared for coffee) that strata probably of many thousands of feet in thickness have been carried away to the low country and the sea. It is not, it seems to me, at all an improbable supposition that in Dimbula and Ouvah a band of quartzite has during this denudation been disintegrated, and that its remnants are found now in isolated places resting on the gneiss. The limestone mentioned by Mr. Heelis as occurring in the Ouvah patena district proves, I think, a point in my favour, for the same kind of limestone is more plentiful in the neighbourhood of the quartzite band between Pussellawa and Rambodde than in any other district with which I am acquainted, there being no less than five entirely isolated spots near these villages where it occurs. This limestone is highly crystalline and of the same age as the gneiss, for I have found it at the upper fall at Rambodde passing almost imperceptibly both above and below into the gneiss. It is here about 450 feet above the upper surface of the quartzite band, where it crops out in the lower fall. Its stratified character may be readily seen at Pussellawa by the bands of mica-fragments that run through it in almost horizontal directions. I have never heard of this limestone covering any extensive area except at Matalé, where there must be some hundreds of acres of it. In other localities that I have visited it covers only an acre or more frequently only a fraction of an acre. The soil produced by its disintegration is, I believe, the richest in the island, as is shown by the fact that the limestone after being burnt is frequently used as a manure for coffee trees, and that the jungle growing below such rocks is generally of the richest description. I can scarcely therefore think that any considerable area of patena soil in Ouvah is formed by the disintegration of limestone, although it is quite consistent with what occurs at Rambodde that limestone should be extensively found in the neighbourhood of a large patena. As to the quality of the soil on the Ouvah patenas the test generally applied by planters is that of the power of the coffee tree to produce fruit. This is manifestly not a perfect test. Climate counts for a great deal, and the climate of Ouvah is recognised as the most favourable in Ceylon for the production of coffee, whilst that of Dimbula is acknowledged to be too humid for the perfect fruiting of the plant. I remember a pertinent remark made to me by a successful planter in regard to the relative values of soil and climate in the growing of coffee. "Give me the climate and I can make the soil." It is an exaggeration, but there is sufficient truth in it to illustrate well the point I am urging.

Finally in regard to abandoned clearings falling back not into "chena" and jungle but into patena land, I must confess I never met with an instance of it, and with Mr. Heelis' permission I would suggest that the Dimbula cricket ground is scarcely a case in point. It is probably the interest of the owner to keep it in grass and to prevent seeds accidentally carried to it from taking root. But supposing it were surrounded by forest and left to itself for twenty years, would it at the end of that period be still in grass, or would it have returned to "chena"? If it were genuine patena land, it would remain so, for plants accidentally imported into it would find no nourishment, but if it were impoverished jungle soil, I am inclined to the opinion that there would still be sufficient unextracted nourishment to enable at least the hardier species to grow in a stunted form until humus was deposited, when forest would succeed. Whilst differing from Mr. Heelis on the several points of his letter I cannot omit to thank him for the courtesy with which he has expressed his opinions.

R. ABBAY

Ouseburn, May 10

The Greenland Seal Fishery

ANOTHER year has passed and no steps have been taken to put some restriction upon the cruel and wasteful manner in which the seal fishery is prosecuted. Warning after warning has been given, and still nothing has been done. In 1868 Dr. Brown wrote (*Proc.* Zool. Soc., p. 440): "Supposing the sealing prosecuted with the same vigour as at present, I have little hesitation in stating my opinion that, before thirty years shall have passed away the seal-fishery, as a source of commercial revenue, will have come to a close." This season the Dundee vessels have been turning their attention to the Newfoundland seals, for, says a paragraph in the *Daily News*, "Capt. Adams has for some years been of opinion that that ground [the Greenland seal-fishery] is practically used up, and hence his visit to Newfoundland." The small success of the Greenland sealers this season fully corroborates Capt. Adams's opinion, and forms a practical comment upon Dr. Brown's prediction!

From the same source (*Daily News*) I learn that "advices of a very gratifying character have been received from Newfoundland. The *Panther* has taken 20,000 seals, the *Neptune* 30,000, the *Arctic* 24,000, the *Aurora* 15,000, and high expectations have been formed regarding the success of the whole fleet. These four vessels have secured 89,000 seals; Capt. Gray says 20 per cent. may be added to the number of seals actually taken for those mortally wounded and lost, and that as these are breeding seals each old one will leave a young one to die of starvation. (See letter in *Land and Water*, May 9, 1874.) The result will be that these four vessels destroyed 213,000 seals! Similarly "gratifying" advices have been received from the other vessels of the fleet.

If the Royal Society for Prevention of Cruelty to Animals and the anti-vivisection advocates really wish to do service in the cause of humanity, let them reprint Capt. Gray's letter and distribute it broadcast, nor let them cease their efforts till a proper close time is obtained for these persecuted animals. Apart from all questions of humanity, common prudence would dictate that so rich a source of revenue, which, if properly cared for, may last an indefinite period, should be secured from the rapacity of those who will otherwise soon bring about its extinction. Now is the time for considering the steps which should be taken to bring the matter before the Governments concerned; if left till later in the year hasty legislation will probably, as in the last attempt, end in failure.

Norwich THOMAS SOUTHWELL

A New Lecture Experiment for Proving the Compound Nature of White Light

THE old method of showing the compound nature of light by the composition of artificial colours on the lecture-table, is to arrange the various colours in the proper proportion on a disc and to revolve this disc rapidly; but a pure white cannot be produced by this method since there is necessarily a partial absorption of rays on every part of the disc.

My method is to arrange seven lanterns, in the first place, so as to project their several circles of light side by side on a white screen, then to colour each circle by introducing slides of glass stained to imitate the seven colours of the spectrum (the proper intensity of colour being found by trial); we thus get seven circles on the screen coloured from red to violet and arranged side by side. Then by turning the several lanterns so that the projected circles shall exactly overlap each other we get one circle of white light, proving that the seven colours together make white light.

The same effect can be produced with five colours only if properly selected; and even two, the ordinary cobalt blue and deep orange, will nearly do. If these two last be made to partially overlap the effect is very striking. WM. TERRILL

Swansea, May 6

The Araucaria

IN your first number for March last you express your surprise that we should still be ignorant regarding some important phases of salmon life; but there is a question relating to facts much more within the sphere of our daily observation on which authorities differ as much. Does the common Araucaria (*A. imbricata*) require one year or two for the growth of a shoot on the main stem, estimating a shoot as the growth between two whorls of branches? Every gardener whom I have consulted on the subject in Scotland, from north to south, says positively that it requires two years, while the few of whom I have had any opportunity of inquiring in the south of England, decide equally positively in favour of one year. Prof. Balfour agrees with the former in as far as Scotland is concerned, while a gentleman residing on the border between the two countries, informs me that some of his have grown at the rate of a shoot in two years, others of a shoot annually, while a few show only a shoot for every year and a half since they were planted. It has been suggested to me that the difference, if it really exist, may be due to the more favourable climate of England; but araucarias may be seen growing as freely and as healthily in Ross-shire as in Kew Gardens. It would be satisfactory to have more general information on the subject from England and from the Continent of Europe, and still better to have it from the native countries of the tree.

There is another question equally important regarding it, namely, when the shoots are biennial, as they undoubtedly are in many cases, is there a timber ring in the stem for every year's growth, or one for every whorl of branches? On that point also the evidence is contradictory.

JAMES ELLIOT

The Hibernation of Swallows

IN connection with the Duke of Argyll's letter on this subject (NATURE, vol. xv. p. 527) there is an interesting communication in the *Ornithologisches Centralblatt* of May 1 from Herr J. Rohweder, under the head of "Ornithological Notes from Schleswig-Holstein." Herr Rohweder certifies to the competency and trustworthiness of the observer who communicated the facts to him. After the house-swallows (*Hirundo urbica*) in the autumn of 1870, from the beginning to the middle of September, had held their usual assemblies by hundreds on the sunny side of the roofs, stormy and rainy cold weather suddenly supervened. As suddenly did most of the swallows take their departure for the south. The few that remained behind flew about restlessly and anxiously, unable in the cold north wind to obtain sufficient insects to appease their hunger. Within a day after the others these also disappeared. Three days after, during which time no swallow was observed, Herr Rohweder's informant saw peeping out of the entrance of some nests under the projecting roof of the east side of his house, here a wing, there a tail or a few feathers. A ladder was obtained and the nests tapped, but no motion. On pulling at one of the overhanging wings a swallow was dragged out. It was alive, but seemed paralysed. After the swallow was held in the hand a while it fluttered about a short space and then fell to the earth. A second bird behaved in the same way, and a third showed few signs of life. A fourth appeared quite lifeless. In other nests six, and ten, and even fourteen swallows were found huddled together. Their condition was similar to those first found. The birds near the entrance of the nest appeared in a state of sound sleep, while those furthe in showed no signs of life. The former soon were able to fly, with difficulty, a larger or shorter round, only one flying to a considerable distance; the latter were thrown on a neighbouring heap of straw. On the following day, when the observer returned, no birds were found. The exact locality of these observations is not given. X.

Two Remarkable Meteors

WHILST walking on Sunday night with a friend, about 10·35 my attention was directed to a beautiful meteor of a ruddy hue, not unlike Mars. It appeared a little to the south of Arcturus, and after passing along with a slow motion in an easterly direction, throwing out sparks meanwhile, disappeared near β Herculis. In size it seemed to be about four times as large as Jupiter, and continued visible for three or four seconds. About half a minute afterwards it was followed by another from the same quarter, which took almost exactly the same direction as the other. In colour and appearance it resembled the first, but was not quite so large. It remained visible about three seconds. The sky at the time was beautifully clear, and there was little or no wind.

Rottingdean, Brighton, May 14 W. H. S. J. HOPE

Yellow Crocuses

(Translation)

I have observed here that sparrows have shown a very considerable partiality for yellow crocuses during this spring. My neighbour and I vied with each other in our spring beds; he excelled in yellow crocuses and hyacinths, I in white and blue crocuses. One beautiful Sunday the whole of his crocuses were found bitten and torn by sparrows, and, what is noteworthy, also some yellow crocuses which had somehow wandered into my lot, while the blue and white remained almost untouched. Should this be regarded as an oversight, or was it a matter of taste?

So far the fact is incontestable, but it has not before been observed by me, though I am an old amateur. To be sure, for the last six years, I have always been, about the time of blooming, absent at the Reichstag, and perhaps, therefore, have forgotten early single observations. It may not be possible to obtain a positive explanation. The dryness of the spring, perhaps the colour-sense of the bird, or even a more or less delicate mixture of the plant-sap may account for it—quien sabe!

Hamburg, May 12 W. VON FREEDEN
Editor of the *Hansa*

Sound and Light

I SHOULD like to learn if the following phenomenon is well known and alluded to in scientific writings. While lying awake a few mornings ago, with my eyelids closed, I was startled by a railway whistle. At the same instant I perceived a blaze of light on a dark ground seemingly a few yards off. I made inquiry of my wife (who is of a much more nervous temperament than I) if she had ever observed such a coincidence, and was informed that in her case it is not a very unfrequent occurrence. I likewise reported the circumstance to some scientific friends, but they had neither read nor heard of noise being the occasioning cause of sensation of colour.

While the pen is in my hand I may mention, in reference to Mr. Renshaw's communication (p. 530), that sparrows are in the habit of demolishing the flowers of my yellow crocuses.

Bushy Hill, Cambuslang HENRY MUIRHEAD

Cloud Colours

A VEIN of thought is sometimes as a vein of the most fine gold, and observation is everything in meteorology as it is in geology, in which two difficult sciences we are much interested in this country, and of which your contributor is the unpretending student.

Now I first learned my lessons in weather science from the remarks of Admiral Fitzroy, the author of the Weather Book, which should be well known and read in this country. For years we have marked what an intimate correlation there is between the colour of the clouds and coming weather. Thus we have the cold dark blue and grey, and the reddish yellow masses of cloud as indicative of cold and snow, and we have the light bright grey with bright edges as accompanying or indicating hard frost. Then again we have the inky-coloured cloud, flying in shreds, as indicative of wind and rain, and also the mottled cloud of the same colour or thereabouts, as the sure indicative of rain. We have the sickly-looking green, the deep blue gloom, the muddy angry-looking red, and other such tints, as forecasts of storm, snow, rain, &c.; and frequently before a north-easter we have the grey bluish and whitish clouds setting from north-east, somewhat like the spread-out fingers of the hand. Our sunsets are often grand beyond my pen. The lavish wealth of crimson and gold is magnificent. It strikes us now to ask what relation chemistry and gases have with the cloud colours. I leave that to older minds than mine, beyond the banks of Newfoundland.

But we see, from all that has been said, the *vast* importance of noting the *colours* of the clouds. We depend much in this country on the colour of the clouds for weather prediction. Ice, however, at this time of year, by refrigerating the atmosphere, often interferes with calculation. H. C.

Hailer Grace, Newfoundland

THE PROGRESS OF EVOLUTION[1]

THE new journal mentioned below is edited jointly by Dr. Otto Caspari, of Heidelberg, Prof. Dr. Gustav Jäger, of Stuttgart, and Dr. Ernst Krause (Carus Sterne), of Berlin; and on the list of its contributors are the names of Charles Darwin, Ernst Haeckel, Friedrich von Hellwald, and many others whose scientific creed is Darwinism.

The editors in their introductory statement say that a new day has dawned for natural science, since our great countryman applied the natural laws which govern the whole universe to the phenomena of the development of life, and showed the fallacy of assigning that central position in nature to man himself which had been attributed to him for ages, as Copernicus did in the case of our planet three centuries ago. Man, who seemed to stand above nature hitherto has, without being drawn down from his eminent position, been incorporated with nature as one of her integral parts. The new monistic philosophy caused a wonderful reaction, and an animated reciprocal intercourse arose between the subjective and objective sciences. All the sciences which treat of man, from anthropology, ethnology, and the psychology of peoples, to the history of culture and states, national economy, the philosophy of law, history, and religion, and the sciences of morals and dietetics, proved to be natural sciences quite as much as mineralogy, biology, the practical education of man, and the cultivation of plants and animals.

The result of this general intercourse of the different sciences, has been a continued and encouraging confirmation of the monistic principle contained in the theories of descent and development; the literature, however, which was generated by the reaction, is dispersed and can be collected only from the various scientific journals. Thus, a general desire for collection and concentration has sprung up amongst all those who look upon the theory of development as a considerable progress of the human mind.

The new *Kosmos* will bring together what has hitherto been unconnected; will point out the gaps still existing, and thus lead to their being speedily filled; will reduce contrasts and contradictions to their true nature, and will oppose pernicious dogmatism. *Kosmos* will, with regard to the special domains of natural science, bear a certain critical and polemical stamp, its editors being aware that even science is best developed and strengthened in the fight for its existence, and that in the end the "fittest" theory will survive. All articles in the new serial are written in popular language, and are intended for a large circle of readers.

The first number contains a series of very interesting articles, of which we may mention—Philosophy and its Union with Natural Science, by Otto Caspari; On Inheritance, by Dr. Gustav Jäger; On Modern Anthropology, by the same; On the Chronicles of the History of Development, by Ernst Haeckel; The History of Creation and Chorology two Centuries ago, by Carus Sterne; On the Significance and Objects of Ethnography, by Friedrich von Hellwald; and an excellent review of Darwin's work on Cross and Self-Fertilisation, by Dr. Hermann Müller.

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[1] *Kosmos; Zeitschrift für einheitliche Weltanschauung auf Grund der Entwickelungslehre.* (1 Heft, April, 1877.)

ENGINEERING EDUCATION IN JAPAN

THE technical education of engineers is a subject which has engaged public attention for a long time past and is one of great national importance. It is somewhat singular that this country, foremost as it has always been in matters of engineering enterprise, should be so behindhand in the systematic education of its engineers, there being no establishment in England devoted to that object which is recognised by the profession. Under the system that has been in vogue up to a comparatively recent period a youth intended for an engineer is taken from school at the age of sixteen being thereby deprived of the most valuable years of his education, and placed in some engineering manufactory, where he remains, perhaps, till he is twenty. In those four years his so-called "training" consists in going through the manual routine of the various workshops and "picking up" what knowledge he can by keeping his eyes open and living on good terms with the workmen. His last year is usually spent in the drawing-office, where, by a similar process of "picking up," he learns how to draw if not to design machinery or works of construction. At the end of that time his education is supposed to be complete, and he either remains as a draughtsman until something better is offered him, or he enters the office of another engineer for the purpose of improvement. All this time the far more important theoretical training is neglected altogether, no classes or examinations are held, no lectures or other instructions are given, and though some few energetic young men in some way make up this loss by private study they are a great exception, and the hours of manual work are usually so heavy (from 6 A.M. till 5 P.M.) as to render working in the evening both fatiguing and unprofitable.

The Continental system goes to the other extreme, teaching the theory and discarding the practice. This system is as bad as the other, for experience has shown that in engineering works a practical man without scientific training seldom makes such serious blunders as a scientific man without practical experience. It can only be by a judicious combination of the two systems, allowing science and practical experience to work hand in hand together in the education of an engineer that the best results can be looked for, and in these days of close competition, not only between man and man, but between country and country, it is of the utmost importance to a nation that its engineers should be instructed upon the best and soundest principles. The Indian Government recognised this when it established the Royal Indian Engineering College at Cooper's Hill for the systematic training of engineers for the Public Works Department of India; and it is remarkable that the profession of engineering should stand alone in England as having no recognised *Alma Mater* of its own. Many years ago an engineering college was established at Putney upon a good system, but it was badly managed, and after becoming a nuisance to the neighbourhood, was ultimately shut up; at the present time, with the exception of the technical classes at the Crystal Palace and at King's College, which, in a small way, are doing good work, there is no institution in this country devoted to the education of engineers.

While England is so far behindhand in this important question, a great work has been done by the Japanese Government in the establishment of an Imperial College of Engineering at Tókei, an institution which gives to its students a highly scientific training, combined with actual practical experience in engineering workshops which give employment at the present time to over three hundred workmen, but which are being largely increased and are turning out all classes of engineering work.

The system adopted is as follows:—The course of training extends over six years. The first two years are spent entirely at college; during the next two years, six months of each year are spent at college and six months in the practice of that particular branch which the student may select; the last two years are spent entirely in practical work. The system of instruction in the college is partly professorial and partly tutorial, consisting in the delivery of lectures and in assistance being given to the students in their work.

Candidates for admission must be Japanese subjects under the age of twenty, and must pass a preliminary examination, the best fifty being chosen as cadets, of which there are two classes. A student may elect to enter either as a Government cadet—in which case all his expenses are defrayed by Government, under whom he binds himself to serve for seven years at the expiration of his six years' training—or he may enter as a private cadet, paying his own expenses, in which case the obligation to serve subsequently under Government is dispensed with. In all other respects he is on the same footing as the Government cadet.

The whole system of training may be divided into three courses:—(1) General and Scientific, (2) Technical, and (3) Practical. The general and scientific course, which is taught during the first two years, includes (1) English language and composition, (2) geography, (3) elementary mathematics, (4) elementary mechanics, (5) elementary physics, (6) chemistry, and (7) mechanical drawing.

The Technical course consists of the following branches of engineering:—(1) Civil engineering, (2) mechanical engineering, (3) telegraphy, (4) architecture, (5) chemistry and metallurgy, and (6) mining. This course is taught during the third and fourth years of the curriculum. The practical course, in which the students are engaged during the last two years in the practice of the special branch each may have selected, consists of working in the laboratories of the college, and in the engineering works connected with it established at Akabane, where they serve a regular engineering apprenticeship. While this course is going on lectures on special subjects are given, and the students are required to prepare reports upon the work in which they have been engaged.

In the Technical course are included the higher mathematics and natural philosophy, engineering, civil and mechanical, geology, mineralogy, surveying, naval architecture, strength of materials, practice in the chemical, physical, metallurgical, and engineering laboratories, and in the drawing office and workshops.

The main building, which is a very handsome structure, consists of a central portion containing the large examination hall and library, drawing offices and class rooms, and on each side of this extends a wing containing other class rooms and lecture halls. This is the College proper, and surrounding it are separate buildings set apart for the dormitories, Professors' houses, museum and laboratories of which there are four devoted respectively to chemistry, physics, metallurgy, and engineering. The buildings have been very admirably arranged by the Principal of the College, Mr. Henry Dyer, C.E., and the architectural details have been carried out with great skill by Mr. C. A. de Boinville.

The staff of the College consists of a Principal and nine English Professors, assisted by Japanese teachers, and the Institution is under the jurisdiction of the Minister of Public Works.

A calendar of the College is published annually, which contains information relative to the admission of students, courses of study, and examination papers, as well as catalogues of the splendid collection of instruments in the laboratories, and of the books in the library, which seems to be exceptionally rich in almost every branch of general and scientific literature.

C. W. C.

SUSPECTED RELATIONS BETWEEN THE SUN AND EARTH[1]

III.

In the first of these articles I tried to show that the magnetism of the earth is affected by the state of the sun's surface. I shall now try to show that the meteorology of the earth is likewise affected by the same cause.

Mr. Baxendell, of Manchester, was, I think, the first to point out that the meteorological convection currents of the earth appear to vary according to the state of the sun's surface. More recently Mr. Meldrum, of the Mauritius Observatory, has brought this connection very forcibly before us by showing, from the results of his observations, that there are more cyclones in the Indian Ocean during years of maximum than during years of minimum sun-spots. This will be seen from the following table:—

TABLE II.

Comparison of the Yearly Number of Cyclones occurring in the Indian Ocean with the Yearly Number of Spots on the Sun.

Character as regards Sun-spots.	Years.	Total number of Cyclones.	No. of Cyclones in max. and min. Periods.	Character as regards Sun spots.	Years.	Total number of Cyclones.	No. of Cyclones in max. and min. Periods.
Max.	1847	5	23				
	1848	8			1862	10	
	1849	10			1863	9	
	1850	8			1864	5	
	1851	7		Min.	1865	7	21
	1852	8			1866	8	
	1853	8			1867	6	
Min.	1854	4	13		1868	7	
	1855	5		Max.	1869	9	31
	1856	4			1870	11	
	1857	4			1871	11	
	1858	9			1872	13	
Max.	1859	15	39		1873	12	
	1860	13					
	1861	11					

Prof. Poey has confirmed this conclusion of Mr. Meldrum by showing that there is a similar periodicity as regards the cyclones which make their appearance off the coast of Central America.

In the next place Dr. Arthur Schuster has found that the years of minimum sun-spots coincide very nearly with the good wine years in Germany. This will appear from the following table.

TABLE III.

Exhibiting the near Coincidence between the Years known as good Wine Years in Germany and the Years of minimum Sun-spots.

Dates of Minimum Sun-spots.	Years known in Germany as good Wine Years.
1784·8	1784
1798·5	(?)
1810·5	1811
1823·2	1822
1833·8	1834
1844·0	1846
1856·2	1857 1858
1867·2	1868

Again, it has quite recently been remarked by Dr. Hunter, Director-General of Statistics to the Government of India, that the famines in Southern India have a period of recurrence which is nearly eleven years, being thus of the same duration as that of sun-spot frequency.

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[1] Continued from p. 28.

Here we have evidence from various quarters of a connection of some sort between the state of the sun's surface and the meteorology of the earth, and it becomes a question of great interest what is the nature of this connection.

In the first of these articles a diagram was exhibited showing the close relation that exists between the state of the sun's surface and the range of oscillation of the magnet freely suspended at the Kew Observatory. If instead of taking the daily magnetic ranges we take the daily temperature ranges, that is to say, the differences between the maximum and minimum thermometers, we find an apparent reference to the state of the sun in these also, inasmuch as these ranges appear to be greater at times of maximum than at times of minimum sun-spot frequency. Nevertheless the correspondence is not nearly so well marked as in the case of the magnetic declination, and there is no doubt much local irregularity. But here the following question of much interest and importance crops up. Do these fluctuations of the daily temperature range at the Kew Observatory coincide in point of time with the corresponding solar fluctuations? or do the former lag behind the latter, as is the case with the magnet? The practical bearing of this question is easily seen, for if temperature oscillations and other meteorological fluctuations are simultaneous with the corresponding solar changes, we can hardly expect that a study of the sun's surface will ever enable us to forecast meteorological occurrences; but if on the other hand the solar changes precede the meteorological ones, we may hope, when the nature of the connection between them is fully understood, to make use of solar observations in order to predict the greater meteorological occurrences. Now it appears to the writer that there are certain well-marked fluctuations of temperature range at the Kew Observatory which coincide very closely with corresponding magnetic fluctuations, and which therefore lag behind the solar fluctuations nearly six months (see Article I.); but this interesting and important question can only be determined by further investigations.

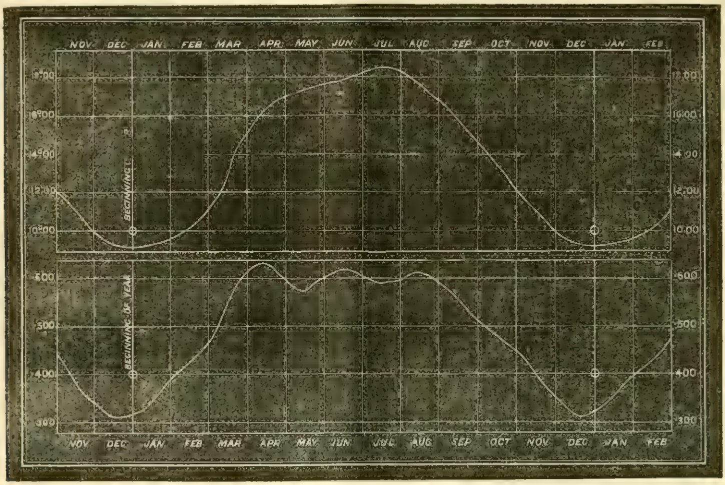

DIAGRAM K.—The Upper Curve denotes Temperature Range, the Lower Curve Declination Range.

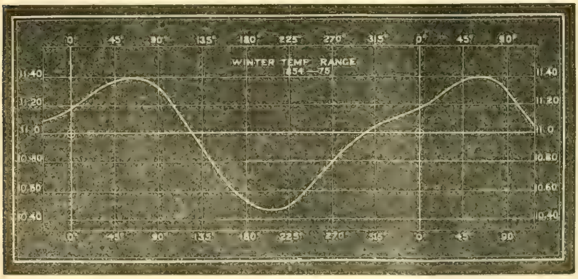

DIAGRAM L.

I may here remark that meteorologists are beginning to suspect a somewhat intimate connection between the magnetism and the meteorology of the earth. Mr. Baxendell was, I think, the first to point out that there is a diurnal inequality in the direction and velocity of the wind apparently connected with the daily changes of magnetic declination. On this subject the writer has recently received a letter from Mr. J. A. Broun, the well-known meteorologist and magnetician, who says, "My present opinion is that meteorological phenomena are due to solar actions; that the heating action is not the only one; but that the action which produces variations in the earth's magnetic force affects the conditions of the atmospheric gases, introducing forces which we cannot in the present state of our knowledge appreciate, though the facts appear to me to prove their existence."

It will be seen, by Diagram K, that there is a very marked likeness between the annual variation of the temperature range and the annual variation of the declination range at the Kew Observatory.

There yet remains a question which is nearly allied to the present inquiry. If the sun affects the earth in a variety of ways, and if the planets affect the sun, why should not the moon affect the earth? Now it is known to affect terrestrial magnetism, producing a well-marked variation of a tidal nature, that is to say with two maxima and minima in each lunar day, and there are also indications of a variation with only one maximum and minimum.

Again, Mr. Park Harrison was the first to point out that terrestrial temperature is influenced by the relative position of the sun and moon.

The writer of this article has found in the daily temperature range at the Kew Observatory an unmistakable reference to the phase of the moon.

In summer when the full moon is low in the heavens, we have a less decided reference, which seems to imply a maximum of daily temperature range about new moon and also about full moon. But in winter, when the full moon is high, we have a very decided reference showing a maximum of daily temperature range about new moon, and a minimum about full moon.

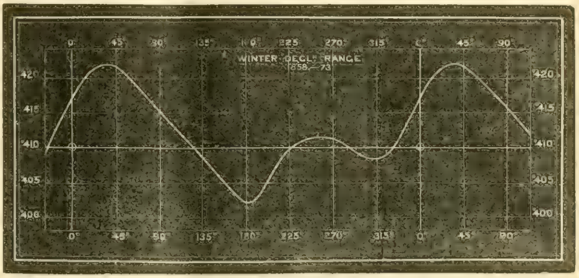

Diagram M.

Again, in the magnetic ranges at Kew the same features occur, namely, in summer a maximum range at new and at full moon, and in winter a maximum at new and a minimum at full moon.

The winter lunar variations of the temperature and declination ranges at Kew are exhibited in the Diagrams L and M, from which it will be seen that there is a very decided likeness between the two.

These last diagrams are especially interesting because they exhibit an influence which appears to be similar *in form* to that which the planets may be supposed to produce upon the surface of the sun. This, however, is a question which can only be decided by further investigation.

If we now bring together the results of these three papers we may compare the three problems, solar research, terrestrial magnetism, and meteorology, to three corners of a triangle that are bound together. Of their three relations we are, it may be said, perfectly certain of the connection between solar research and terrestrial magnetism. The connection between solar research and meteorology is perhaps not so well defined, but our evidence is here supplemented by independent traces of a connection between magnetism and meteorology. Thus the three things hang together, and scientific prudence points to the desirability of their being studied together as a whole, a consideration which will not, I trust, be overlooked in the contemplated reorganisation of British meteorology.

I would desire now to conclude by asking, in all honesty, Have we not here a plea for the establishment of some institution that will keep a daily watch upon that luminary which is thus seen to affect us in such a variety of ways?

Balfour Stewart

THE SOUTH AFRICAN MUSEUM

OUR notice of the condition of the South African Museum, and the various sums allotted to research by the Government of the Colony, has called forth some criticisms on the part of the Cape-town *Standard and Mail* of April 7. "What NATURE and other scientific organs in Europe mean by 'research,'" it states, "is not what the responsible advisers of the Cape mean by their favouring grants. It would not be saying too much, nor putting it too strongly, to assert that there is no scientific research carried on in connection with any botanical gardens in South Africa. In regard to our museums there is some genuine work being done; at all events in the South African and Albany museums original observations are being recorded. As to our libraries which absorb 2,000*l.* per annum of the public money, the less said, perhaps, the better. The South African Library, as far as standard works in such branches of science as anatomy, chemistry, mineralogy, natural philosophy, &c., are concerned, is simply deficient, and unaccountably so, considering the demands of these departments and the standing of some of the directors. The only sums voted for purely original scientific work are those for 'Geological Researches,' for the publication of Dr. Bleek's Bushman Researches, and for the Meteorological Commission. With the exception of the first of these, which amounts to 1,500*l.*, research in the sense NATURE must mean, is fostered by only some four or five hundred pounds." The writer then goes on to describe the consequences of Dr. Bleek's death; the linguistic and ethnological researches he was carrying on have been stopped, and instead of appointing a qualified scholar to fill his place, the Government allowed his office and salary "to be absorbed into the general and ignoble management of the South African Library, which is only a representative of Mudie, being conducted in the charitable idea of providing, at three pounds sterling per annum, the current literature of the day to subscribers who for the same reading would have to pay in a circulating library about four times the amount. . . . 'Novels are the solace of my life,' was the plea (of Mr. Goodliffe) from the chair in favour of continuing a national institution subsidised by the Government of the Colony, and therefore supported from the revenue of the country, as a receptacle for the custodianship of the popular writings of the period. The scientific work of South Africa has been done by amateurs holding no professed natural history appointments." The Gill College Herbarium now receives a subsidy of 100*l.* a year, but "Prof. Macowan worked at the botany of the Colony for thirteen years before he received any grant to enable him to prosecute the study, or to cover the expenses of preserving a large herbarium." The Colonial Herbarium in Capetown "has a collection of types of the very highest value to Cape botany—those arranged and classified by Dr. Harvey. It has the collections of Dr. Pappe, the late Colonial botanist, consisting of thousands of species, which were bought by a former Government for some 200*l.* Other collections more or less valuable are also in the Herbarium." But "the greater part of these interesting and valuable plants has been destroyed by rain leaking through the roof of the library buildings into the room where they are kept, and by the ravages of moths, &c. In a short time the herbarium will be simply nothing but a mass of uninteresting fragments. We understand that some time back the Parliament voted a small sum to be expended in putting the herbarium into order. How far anything could possibly have been done by those in charge may be learned from the fact that Dr. Rehman, the Austrian botanist, found whole fasiculi destroyed."

SPONGY IRON FILTERS

IN a paper presented by Prof. Frankland, F.R.S., and read before the Royal Society, Mr. Gustav Bischof describes numerous experiments made with spongy iron filters and with charcoal filters. He states that chemical analysis is incapable of discriminating between living or dead, fresh or putrescent organic matters. The microscope reveals their nature more fully; but it is nevertheless frequently a matter of great difficulty to decide as to the existence or non-existence of *Bacteria* of putrefaction, or their germs, in water.

We must refer our readers to the paper for a full account of the experiments and the conditions under which they were performed. Mr. Bischof states that they show that *Bacteria* present in drinking water are *not* killed in passing through charcoal and are killed in passing through spongy iron.

He adds: "I believe that the action of spongy iron on organic matters largely consists in a reduction of ferric hydrate by organic impurities in water. We know that even such organic matters as straw or branches are capable of reducing ferric to ferrous hydrate. We know that even such indestructible organic matter as linen and cotton fibres are gradually destroyed by rust stains. This action is slow when experimenting upon ordinary ferric hydrate, but it may, *in statu nascendi*, be very energetic, the more so if we consider the nature of the organic matter in water. Ferric hydrate is always formed in the upper part of a layer of spongy iron, when water is passed through that material. The ferrous hydrate resulting from the reduction by organic matter may be re-oxidised by oxygen dissolved in the water, and thus the two reactions repeat themselves. This would explain why the action of spongy iron continues so long.

"It is, however, quite certain that there is also a reducing action taking place when ordinary water is passed through spongy iron. This is clearly indicated by the reduction of nitrates.

"Our knowledge of those low organisms, which are believed to be the cause of certain epidemics, is as yet too limited to allow of direct experiments upon them. It is not improbable that, like the *Bacteria* of putrefaction, they are rendered harmless when water containing them passes through spongy iron; but until we possess the means of isolating these organisms, this question can only be definitively settled by practical experience."

CENTROIDS AND THEIR APPLICATION TO SOME MECHANICAL PROBLEMS[1]

THE principal object of the following paper is to suggest the use of a more general form than is commonly employed in the statement of some of the more important theorems of elementary mechanics. Such a generalisation, if in itself satisfactory, has two-fold advantages; it both facilitates the direct solution of problems otherwise apparently complex, and it enables a common method to be employed in an infinite variety of cases, each of which otherwise has to be treated in its own special way. The methods to be described are purely geometric, and admit in all cases of graphic solutions. In the study of mechanism and in all applications of mechanics to engineering work this is a matter of considerable importance, for graphic methods have such enormous advantages in these cases that they must supplant all others when they give equally good results.

By the centroid of any body A relatively to another B is meant *the locus of the instantaneous centres of A in its motion relatively to B.*[2] The expression includes two things, which must be distinguished from each other;—(i.) the locus as part of the moving body A, (ii.) the locus as part of the body B relatively to which A's motion is observed, and which may for convenience be regarded as fixed. These loci may be entirely different as to form, but in all their properties they are absolutely similar and reciprocal. It would therefore be wrong to give them different names, they can be distinguished, when necessary, as the centroid *of a body*, and the centroid *for the motion of a body* respectively. The centroid of A is therefore the locus *upon* A of its inst. centres relatively to B; the centroid for the motion of A is the locus *upon* B of the same centres.

The following are the most important characteristics of these curves. As the bodies to which they belong move the centroids roll upon each other, and every point in each becomes in turn the inst. centre. Their rolling, therefore, represents continuously the whole motion of the bodies (considered as changes of position merely), quite irrespective of their form; in other words it defines the path of motion of all points in the bodies. The two centroids have always one point in common—their point of contact—this point being the instantaneous centre. This point may be included in both bodies, and has no motion relatively to either. Any motion which it has must therefore be common to both, so that it may be entirely neglected in investigating their relative motions. In problems affecting the motion of either body relatively to a third this is often of much use.

For the sake of definiteness it has been presupposed in the foregoing paragraphs that the motions referred to were conplane, or, more generally, took place about some fixed point. When the motion is conplane this point is at infinity, and the centroids are plane curves, sections of the cylindric ruled surfaces formed by the successive positions of the instantaneous axes. When the distance of the point is finite, the centroids are, of course, spheric curves, the instantaneous axes forming cones of which the point mentioned is the vertex. These theorems were given by Poinsot in his "Théorie Nouvelle de la Rotation des Corps." It may be interesting just to mention also the case of general motion in space, where (as Belanger seems first to have pointed out), the solids of instantaneous axes, or *axoids*, as Reuleaux calls them—are general ruled surfaces twisting on each other. Each generator of the surface is a "screw," and on each in turn a twist occurs. The surfaces are in general non-developable.

For the sake of brevity, only conplane motions will be considered in this paper. This class of motions includes nine-tenths of those occurring in mechanism. Two or three special cases of frequent occurrence may first be mentioned. If the relative motion of two bodies be a simple rotation, the centroids are a pair of coincident points, one of which must still be considered to roll on the other. The instantaneous centre here becomes a permanent centre. It is convenient, however, to treat the point not only as a permanent centre, but as a special (limiting) case of the centroid. If all points in a body move in parallel straight lines, the centroid for the motion of the body is a point at infinity, and the centroid of the body is also a point at infinity coincident with the former. If the path of the body were infinitely long, the two points would roll round each other. If, on the other hand, a body move parallel to itself, every point in the centroid for its motion (and therefore all points in its own centroid) must be at infinity. The two centroids must again be coincident, so that the motion is represented by the line at infinity rolling on itself.[1]

Proceeding now to notice the bearing of the theory of centroids upon some of the theorems of elementary mechanics, these may be taken in order of simplicity, commencing with those which involve only the notion of change of position. If, then, the line joining any moving point with the point of contact of its centroids be called its instantaneous radius, we can state the general theorem thus: *The direction of motion of every point in a body is normal to its instantaneous radius.* While this obviously includes the simpler special cases already examined, its form allows of direct application to the most general cases, and especially to all cases in mechanism. Two corollaries out of many which are deducible from it may be mentioned as of some special interest: (i.) The inst. radii of a point moving in a straight line are parallel; and (ii.) the inst. radii of a point moving in a circle must pass through one point. In either case the centroids may be quite general curves, as is easily seen. These corollaries have important practical applications in mechanism, especially in "parallel motions," both real and approximate.

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[1] Abstract of a paper read before Section A of the British Association at Glasgow, by Prof. Alex. B. W. Kennedy, C.E., of University College, London.

[2] The word *centroid* was suggested to the author by his colleague, Prof. W. K. Clifford.

[1] Some physical conception of this case can easily be obtained by rolling one hyperbola upon another. The change in the appearance of the rolling as the point of contact recedes along either branch is very striking.

The familiar theorem that *the relative velocities of points in any body vary as their instantaneous radii* needs merely to be mentioned. It is to be regretted that it is not more generally used, for while it does not increase the difficulty of comprehending simple cases, it is of enormous advantage in simplifying such (apparently) complex ones as not unfrequently occur in mechanism.

The expression for static equilibrium is also tolerably familiar:—*the sum of the moments of all the forces acting upon a body about its inst. centre must* = 0. For practical purposes, however, it is generally more convenient to state the proposition:—*the resultant of all the forces acting upon a body must pass through the point of contact of its centroids.* The application of this proposition to all the simpler cases is self-evident, and at the same time it reduces complex cases to their smallest possible dimensions, rendering most very easy, and in many cases greatly aiding the comprehension of the alterations in conditions of equilibrium corresponding to consecutive alterations in the positions of mechanisms as their links move. It may just be noted that as the two forces of a couple have for their resultant a force (infinitely small) acting along the line at infinity, the proposition gives at once that where the inst. centre of a body is at infinity it is in equilibrio under any number of couples of any magnitude. In the case of a body moving parallel to itself, therefore (see *ante*) all couples may be neglected so far as its static equilibrium is concerned, whatever their magnitude or sense.

The following are, in conclusion, a few of the kinetic propositions the solution of which is greatly aided by the use of centroids:—

(I.) If a force[1] constant in direction and position act upon a body, then (i.) if it cut the centroid for the motion of the body in one or more points motion will take place until the first of these becomes the point of contact, and will then cease; (ii.) if it pass entirely without this centroid, there will be continuous motion. As corollaries to (i) may be mentioned (*a*), if the centroid for the motion of a body be a curve of the 2nd, 3rd . . . *n*th order, the body has a maximum of 2, 3 . . . *n* positions of equilibrium under some one or more forces constant in direction and position. Also (*b*), if a body have not more than a single position of equilibrium under any such force, the centroid for its motion must be a straight line.

(II.) If the position of a force relatively to the body upon which it acts remain constant, then (i.) if it cut the centroid of the body in one or more points, motion will take place until one of these becomes the point of contact, (ii.) if it lie entirely without the centroid of the body, there will be continuous motion. This gives corollaries as to positions of equilibrium similar to those just stated.

(III.) If a force constant in direction act always at the same point of a body, motion will continue until the instantaneous radius of the point becomes parallel to the direction of the force. There is here no case of continual motion; the theorems as to number, &c., of positions of equilibrium are similar to those given above.

English writers have used these curves very little. Among modern continental writers who have employed them may be mentioned Dwelshauvers-Dery (Liége) who uses them in his "Cinématique" for questions relating to relative velocities; Schell (Carlsruhe) in his "Theorie der Bewegung u. d. Kräfte; Reuleaux ("Theoretische Kinematik," and elsewhere), who gave them the name (*Polbahnen*), by which they are known in Germany, and who has used them ably and extensively for kinematic problems; and lastly Pröll, who has made use of them in his recent "Versuch einer graphischen Dynamik." The writer has not, however, found them anywhere unreservedly adopted, and has, therefore, made this attempt to show how easily centroidal methods adapt themselves to the general treatment of mechanical problems, especially those connected with mechanism, and at the same time how well suited they appear to be for educational purposes.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF 1882, MAY 17.—Hallaschka, in his "Elementa Eclipsium," describes this eclipse as broadly total, whereas, it will be, in reality, total, though the zone of totality will not be a broad one. An error in the moon's semidiameter led to the statement in Hallaschka's work. The following elements of this eclipse, calculated upon the same system that has been applied in the examination of other solar eclipses in this column, will probably be near the truth:—

Conjunction in R.A., May 16, at 19h. 41m. 11·7s. G.M.T.

	° ′ ″
R.A.	53 56 35·4
Moon's hourly motion in R.A. ...	36 14·5
Sun's " " " ...	2 28·7
Moon's declination	19 38 46·4 N.
Sun's "	19 19 38·8 N.
Moon's hourly motion in decl. ...	4 56·0 N.
Sun's " " " ...	0 33·8 N.
Moon's horizontal parallax	58 15·1
Sun's " "	8·8
Moon's true semi-diameter	15 52·4
Sun's " "	15 48·8

The central and total eclipse begins at 17h. 53·8m. in longitude 3° 11′ W., and latitude 10° 40′ N.; it occurs with the sun on the meridian in 63° 44′ E., and 38° 35′ N., and ends at 21h. 18·8m. in 138° 51′ E., and 25° 25′ N. The following are points upon the central line in that portion of its track where observations are most likely to be made:—

Long.	Lat.	Sun's Zenith dist.	Long.	Lat.	Sun's Zenith dist.
° ′	° ′	°	° ′	° ′	°
29 49 E.	25 36 N.	49·9	51 27 E.	35 30 N.	23·7
34 5	27 48	44·1	54 58	36 38	21·2
36 3	28 47	41·5	68 48	39 31	21·2
41 33	31 27	34·4	77 23 E.	40 2 N.	26·7
48 20 E.	34 21 N.	26·7			

The central line therefore commences in the west of Africa, and traversing that continent in the direction of Upper Egypt, it passes over the Nile below Thebes, thence over the extremity of the peninsula of Sinai, near Ras Muhammed, and almost directly over Hillah, the site of the ruins of Babylon, to Teheran. The position of this capital according to Gen. Stebitzky (*Astron. Nach.*, No. 2,113) is in longitude 3h. 25m. 41·7s. E. of Greenwich, and latitude 35° 41′ 7″, this point referring to the station of the Indo-European telegraph; so that the central line of shadow according to our elements passes sixteen miles to the south of it. Calculating directly for this longitude we have the following results:—

	h. m. s.	
Totality begins May 16 at	22 36 29	local M.T.
" ends "	22 38 13	
Duration	1 44	

The sun at an altitude of 67°. So that the greatest duration of totality in this eclipse about 12° east of Teheran is about 1m. 46s.

The central line subsequently traverses China, passing off at or close to Shanghai, at which place a total eclipse of short duration may be observed.

The next total solar eclipse on July 29, 1878, which crosses the United States is pretty fully noticed in the various Ephemerides, though in due time the American astronomers will no doubt provide a chart showing on a larger scale the breadth and position of the zone of totality over their country. Then follows the total eclipse of January 11, 1880, in which the track of the central line lies almost wholly upon the Pacific, the total phase being visible for a brief duration only near the coast of California, above San Francisco. The total eclipse of May, 1882, of which the elements are here given is the next in order of date.

THE COMETS OF 1402.—It is singular, considering the attention which the Chinese paid to the observation of comets, their annals containing reference to several hundreds of these bodies, should not have recorded the appearance of the two evidently great comets of 1402. In particular is this the case with the first comet, which, according to the descriptions in the European chronicles collected by Pingré, was first seen early in February, and increasing daily in brilliancy, would appear, if we may rely upon the historians, to have presented a wonderful aspect shortly before Easter. On Palm Sunday, and two following days, we are told "its increase was prodigious;" "le dimanche, sa queue fut longue de vingt-cinq brasses; le lundi, de cinquante et même de cent; de plus de deux cents le mardi." It then ceased to be visible at night, but during the eight following days it was seen near the sun, which it preceded; its tail had then shortened to "une ou deux brasses," but its brilliancy was such that the light of the sun did not prevent its being seen at noon-day. It continued visible till the middle of April.

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[1] Or here, and in the following propositions, the resultant of any number of forces.

Some years since the late Mr. John Williams, Assistant-Secretary of the Royal Astronomical Society, and author of the valuable work upon Chinese cometary astronomy, at the request of the writer, made a strict search for mention of a comet or comets in 1402 in several Chinese authorities in his possession, but without any success; nor is there any reference to a comet in this year in M. Biot's translations. Failing thus to obtain any data for calculation beyond the vague indications of the comet's positions given in the "Cometographie," the writer endeavoured to utilise them to form some idea of the orbit, and found that with perihelion passage assumed on March 21, in longitude 208°, ascending node 117°, inclination 55°, least distance 0·38, and direct motion, the principal circumstances of the comet's appearance, so far at least as regards track across the heavens, might be represented; but its extraordinary brightness is not easily accounted for. The comet is mentioned in Kaempfer's History of Japan, which renders it the more curious that the Chinese annals should have no account of it. Struyck thought it was a return of the comet of 1661, but in his day that body was thought to be identical with Apian's comet of 1532, an idea which was negatived by Mechain's subsequent calculations and by the non-discovery of the comet about the year 1790, notwithstanding Maskelyne's efforts to insure observations if it returned at that time.

METEOROLOGICAL NOTES

Variations in the Relation of the Barometric Gradient to the Force of the Wind.—In a very suggestive paper recently communicated to the Meteorological Society of London, Mr. Clement Ley shows that the mean velocity of the wind corresponding to each barometric gradient is much higher in summer than in winter, and that this is the case at all stations examined, with all winds, with all lengths of radius of isobaric curvature, and with all values of actual barometric pressure. The diurnal and seasonal variation in the relation of the gradient to the force of the wind is unquestionably one of the fundamental questions of meteorological research, and we hope Mr. Ley will soon again return to its discussion, with ampler data for a more satisfactory handling of the subject than he has yet had before him. That the mean diurnal oscillations of the barometer cannot be neglected in the inquiry is very evident. Thus, while in June at 8 A.M. the barometer at Kew is 0·015 inch above the daily average, on the coast at Falmouth it is only 0·001 inch; but while at 3 P.M. it is 0·015 inch below the average at Kew, it is still 0·001 inch above the average at Falmouth. Crossing to the Continent and contrasting Helder on the coast with Namur inland, it is seen that in June at 8 A.M. the barometer at Helder is 0·004 inch under the average, while at Namur it is 0·008 inch above it, but at 3 P.M. it is at Helder 0·007 inch above, whereas at Namur it is 0·011 inch below the average. An interesting part of the paper is that descriptive of the mean diurnal variations in the velocity of the wind, in which, among other interesting features, it is pointed out that at the coast stations, the mean horary curve in summer approximates in type to the winter curve at the inland stations, the diurnal maximum being about 2 P.M. In connection with this it is interesting to note that while at Valentia and Falmouth the anemometric maximum occurs in summer about 2 P.M., the barometric minimum does not occur till from three to four hours later. The point might be even still more strikingly put by a reference to the observations made at Pola, near the head of the Adriatic Sea, where during June, July, and August, 1876, the anemometric maximum occurred from 10 A.M. to noon, and the barometric maximum from 11 A.M. to 1 P.M. The two maxima are thus all but contemporaneous, a result directly opposed to the view generally entertained that in such cases the barometric maxima are contemporaneous with the anemometric minima. London presents very considerable facilities for the working out of this question in its two well-equipped observatories at Greenwich and Kew, and in the number of meteorological stations situated within a radius of fifty miles, in connection with the Meteorological Office, Mr. Glaisher, and the London Meteorological Society. Observations made at these stations at 9 A.M., 3, and 9 P.M., would render possible the drawing of the isobarics over the south-east of England, with an approach to correctness sufficient to give the barometric gradients for Greenwich and Kew as may meet the requirements of the problem. Isobarics drawn from the Daily Telegraphic Reports alone, while sufficient in a first tentative inquiry, are, owing to the great distances between the stations, necessarily very hypothetical, and therefore much too rough for any satisfactory investigation of this important subject.

Climate of Pekin.—A memoir on this subject, read by H. Fritsche before the Imperial Academy of Sciences of St. Petersburg on August 17, 1876, has just been published in the *Repertorium für Meteorologie*. The memoir is an able and exhaustive discussion of the elaborate meteorological observations made at Pekin from the beginning of 1841, and published by the Russian Government under the superintendence successively of Kuppfer, Kaemtz, and Wild. H. Fritsche has thus been able to give in a very complete form the hourly and general monthly averages for temperature, pressure, and humidity, and very satisfactory, though necessarily less complete, averages of wind, cloud, rain, snow, hail, and thunderstorms. The mean temperature and pressure of each day of the year has been worked out in detail, and several of the more important extremes are also tabulated. This well-discussed material has a peculiar meteorological value, arising from the position of Pekin with reference to the continent of Asia, since it results from that position that Pekin may be regarded as situated during the winter months in an extensive anti-cyclone, the prevailing winds being from the continent seawards, and from at least April to July, in an extensive cyclone when the prevailing winds blow from the sea in upon the continent. Hence its dry winter climate, the mean monthly rainfall amounting only to 0·14 inch, and its wet summer climate, the average rainfall in July being nearly 20·00 inches. Hence also snow falls only on eleven days during the year. Thunderstorms occur on twenty-seven days, from the end of April to the beginning of October, reaching the maximum in June, July, and August, when a thunderstorm occurs on an average about every fifth day. The same season marks the period of hail, which is, however, of rare occurrence, being only once in two years. Of special interest are the hourly averages in their relation to the winds and weather of this part of Asia. Thus, while the climate of Pekin loses much of its continental character during the summer months, the hourly barometric curves lose their strictly continental character, the morning minimum, for instance, falling close to, or even slightly below, the mean of the day, thus tending to be assimilated to the curves of the sea-side climates about the latitude of Pekin.

Why the Barometer does not always Indicate Real Vertical Pressure.—Mr. Robert Tennent writes from Edinburgh to point out why the barometer does not always indicate real vertical pressure. He points out that as the upper currents of the atmosphere when in motion are more mobile than the lower, and less retarded by friction than the lowest, there are frequent movements or "liftings" from the lower to the upper layers, and this affects the barometric column, "the normal upward diminution of pressure which takes place when the atmosphere is at rest being greatly altered when its upper portion is in rapid motion." Mr. Tennent says "the practical conclusion from this is obvious. On weather charts the constant rise and fall of the barometer which is there reported, is to a large extent simply due to the passage of air over a resisting surface; over a surface devoid of friction these mechanical effects would be entirely removed, its rise and fall would be greatly reduced, and might be considered as being solely dependent on the effects of heat and vapour. The gradients and isobars which are dependent upon it would also be similarly affected. The barometer does not indicate the real weight of the atmosphere, it only exhibits the amount of its elasticity from which its real weight can only be deduced when the dynamical element of motion does not enter into one of its currents." . . . "As a general rule, in the British Isles equatorial winds are accompanied by these rapid upper movements, while Polar winds move with a greater uniformity in the velocity of their various layers, and sometimes even those on the surface move more rapidly when copiously supplied from a vertical source. There is hence a remarkable difference in their *mode of inflow*. Equatorial winds as they increase in force are hence accompanied by 'lifting' and a fall of the barometer. Polar winds are not attended by 'lifting,' and if their supply is copious and partly from a vertical source, their increase in force is accompanied by a rise of the barometer. The range of the thermometer is equally great both above and below its mean. But with the barometer the extent of its range above the mean is not more than one-half of that which takes place when it is below it. When it is below the mean, equatorial winds generally prevail which are accompanied by 'lifting' and extensive range. Above the mean, Polar winds prevail which are not attended by 'lifting' or such extensive fluctuations. Hence, as a general rule, equatorial winds exhibit fictitious or dynamical pressure, while Polar winds possess more nearly real or statical pressure, being unaccompanied by the mechanical oscillations due to the passage of air over a resisting surface."

THUNDERSTORMS AT ANTIBES ON MARCH 26, 1877.—Col. Gazan has given a brief account, in the *Bulletin International* for April 18, of three thunderstorms which occurred at Antibes, in the south-east of France, on March 26, possessing certain characteristics well deserving of attention. About 7 A.M. a flash of lightning occurred followed by a clap of thunder, and at 7.10 A.M. a fall of hail without rain, lasting ten minutes. The hailstones were for the most part regularly round, quite opaque, and not bigger than common peas, the largest not much exceeding half an inch in diameter. Clear patches of blue sky in the east and south-west formed a striking contrast to a nimbus cloud in the west, which was connected with the upper clouds. The clouds were absolutely motionless, the air so calm that not a leaf was stirring, and the fall of hail exactly perpendicular. At 11.10 A.M. the sky was entirely overcast, and under the same conditions as before a fresh fall of hail took place, mingled with heavy rain, the hailstones being more equal in size and generally smaller. About 2.30 P.M. a pretty smart shower of rain fell, which, immediately after a flash of lightning followed by thunder, increased in violence, and was accompanied with hail. The largest of the hailstones did not much exceed the largest of those of the two falls preceding. During the whole time there was not a breath of wind until just before the end of the last thunderstorm, when a light westerly breeze sprang up. Col. Gazan infers from the quiescent state of the air, as shown by the absence of motion in the clouds, the perfect calm at the earth's surface, and the regular distribution of the hailstones over the ground, that the three thunderstorms were formed immediately over the place and that the phenomena were unattended with any gyratory movement whatever—conclusions which, if correct, have important bearings on the theory of thunderstorms, and therefore are well deserving of the most careful examination on the part of observers of the phenomena of atmospheric electricity.

METEOROLOGY IN ITALY.—The *Rivista Scientifico Industriale* publishes a "project for the constitution of an Italian Meteorological Society" from the pen of Prof. D. Ragona, director of the Modena Observatory, in which the writer points out the importance of meteorology, and adds that this science owes much to Italy, as it was in that country that the most valuable meteorological instruments, viz., the barometer, thermometer, and raingauge, were invented. More than 100 meteorological stations are already in existence, some of which are renowned for exactness of the determinations and delicacy of the researches they have made. They have also the advantage of great variety in their elevations, more than sixty of them being situated between 200 and 2,500 metres above sea level. Several influential persons have already consented to become members of the new society, and amongst them are the Minister for Agriculture, Industry, and Commerce, and Prof. G. V. Schiaparelli, of Milan. We wish the project every success.

METEOROLOGY IN FRANCE.—The prefects of three different departments have published a circular notifying to the mayors of the several communes under their authority the required conditions for receiving daily the weather-warnings issued by the observatory. It is the first time that official action has been taken for the propagation of the system inaugurated by M. Leverrier. The progress made under his direction is very remarkable, and meteorology is becoming very popular in every part of France. The system is to continue on the voluntary principle.

SUNDAY WEATHER WARNINGS.—The weather telegrams sent every Sunday by the British Meteorological Board have been discontinued, as it is only during winter that the taking of observations has been authorised. The head of the Meteorological Office has written to M. Leverrier notifying the fact, and expressing a hope that the Sunday service will be resumed next September. This decision has given rise to some sarcastic paragraphs in the French leading journals, which doubt whether storms will be found strict Sabbatarians even in summer.

GEOLOGICAL NOTES

GEOLOGICAL MAP OF BELGIUM.—Considerable discussion has lately taken place in Belgium regarding a detailed geological map of that kingdom which it has been proposed to construct. The Academy of Sciences, the Geological Society of Belgium, and the Association of Engineers have all formed committees of inquiry as to the best methods of preparing the map. It may interest geological readers to know the scheme which after prolonged discussion has been agreed upon by the Geological Society of Belgium. The organisation of the staff is proposed to include a geological committee charged with the actual survey, and consisting wholly of geologists; a cartographical committee composed of cartographers and geodesists, to take charge of the engraving and publication of the map in chromolithography; a director, as president of both committees, to be appointed by the King, on the recommendation of the geological committee. Each committee is to be independent of the other, and to have the utmost liberty within its own proper sphere of action. The Government, on the recommendation of the Royal Academy of Belgium, names the first five members of the geological committee, and the additions to this number are made by the Government on the recommendation of the committee itself. The geological committee may nominate for appointment by the Government as associate members, the assistants which it will require for the execution of the work, and it will regulate their remuneration subject to ministerial approbation. This committee will settle the legend of the map, as well as all details which can be regulated in advance; it will determine by whom and under what conditions the geological work is to be carried on, and it will decide upon the memoirs or other works connected with the geology of the country, which are to be published as accompaniments of the map. Each published sheet of the map will bear the name of its author. The geological committee will communicate through the director with the cartographical committee before the final printing off of the sheets of the map. The cartographical committee will comprise five members, including the director-president, all appointed by the Government. The Director will convoke the committees as often as he considers necessary and at least once in three months. It will be his duty to superintend the execution of the work determined by the committees, and to give an account of its progress at every quarterly meeting. He will also present annually to Government a report upon the whole work connected with the map and upon the employment of the funds placed at his disposal. These regulations embody the views of the majority of the Geological Society of Belgium, but from the keen and prolonged debate on the subject (well reported in the *Bulletin*), it is clear that some members of the Society shrewdly foresee the difficulties which are sure to arise if these regulations are finally adopted by the Government. The whole scheme is too cumbrous. Unless the president happens to be a man of singular powers, it will be a matter of herculean labour to get a harmonious and complete result out of the independent work of two committees, who need not be summoned above once a quarter, and who are not compelled to have any direct communication with each other until just before the final issue of each sheet of the map. The actual survey will be made, in part at least, by paid assistants. Their work will be subjected to the criticism of the geological committee, the majority of which may change from time to time, thus affording no guarantee of uniformity of system. The maps, after coming out of the ordeal of this committee, will pass under that of the cartographers, who, it seems, are to have full power to bring out the maps in any style or shape they choose, and who may possibly be quite unacquainted with geological requirements. We can anticipate the astonishment with which some fine day one of the assistants may peruse a published copy of his own "feuille." Perhaps his name engraved at the bottom of the sheet may be the only indication he will recognise of his association in a work with which his connection ceased when he handed his field-maps over to the geological committee. It is to be hoped that the Government will reduce this somewhat complicated machinery. A responsible director, with, if need be, a small council of geologists, palæontologists, and map-makers with whom he might from time to time consult, would be sufficient to organise a staff of field-surveyors and to carry out in fullest detail and in complete harmony a geological survey of the country.

ICE-WORK IN LABRADOR.—Mr. H. Y. Hind, who has already published much valuable information regarding the glacial phenomena of British North America, has recently visited part of the north-eastern coast of Labrador, and has prepared some notes of the chief geological results of the journey. His contributions to our knowledge of the glaciation of that part of the world are of special interest, and will no doubt be welcomed by those geologists who still maintain the potency of icebergs and floating-ice over glaciers and ice-cap. He describes the "pan-ice" of the Labrador coast—that is, the frozen sea-water of the bays and shallow seas along the coast, and shows that though in winter it has no lateral motion but merely rises and falls with the tides, in spring and summer it breaks up into pieces or "pans" from a few square yards to many acres in extent. These "pans" pressed by the south-east Arctic current against the coast, and accommodating themselves to all its sinuosities, are pushed over the low islands and promontories with irresistible force, grinding and polishing the hard rocks, rasping the sides of steeper cliffs, and driving before them every boulder and pebble which may be lying on the surface, as well as any blocks which they may be able to detach from the solid rocks. The same kind of action takes place in the shallow seas, the bottom of which, down to a depth of twelve or fifteen feet, is smoothed and planed by the drifting ice. While the prevalent drift is from the north-west out of Davis Strait, a change of wind sometimes brings the endless chain of loose ice back again. The rocks are again abraded and the loose blocks are driven to and fro until they acquire the true boulder-form. In the sheltered depressions of the sea-floor accumulations of *débris* must be taking place like some varieties of boulder-clay. Mr. Hind remarks that this form of ice-work goes on over hundreds of miles of coast. He assumes that it has been the means of smoothing and polishing the rocks of Labrador up to a height of many hundred feet above the sea during the gradual elevation of the land. At the same time he states that though he believes the deep fjords to have been excavated by glaciers, he has found after the most careful search only one example of glacial striæ. An obvious objection will occur to many readers; it may be that the smoothing and polishing of the hills of Labrador has not been done by pan-ice but by solid sheets of land-ice which moved over the country, no doubt grooving and striating it from end to end. All that pan-ice has effected may have been merely the rubbing down of the exposed parts of this general glaciated surface, and the consequent removal of the striæ. The sea-bottom off the Labrador coast freezes in sixty and seventy feet of water, forming what is called "anchor-ice." Seals taken in seal-nets from depths of ten or fifteen fathoms are often found frozen solid when brought to the surface, where, however, they thaw in a few hours. The Labrador climate, as is well known, owes much of its severity to the constant supply of ice drifted past it from the north. Mr. Hind examined thousands of icebergs near at hand last summer, and in only one or two instances did he detect upon them any foreign material. He concludes that true icebergs have little opportunity of transporting rock and *débris*, though he admits that where they ground they may be deepening the water by their incessant rolling and grinding, as the swell of the sea sways them to and fro. He speaks of a loose fringe of such stranded bergs on banks at a distance of ten or fifteen miles from the outermost islands, extending for hundreds of miles along the coast of north-eastern Labrador. These banks intercept the icebergs and prevent them approaching nearer to the land, so that it is only the broken fragments of the smaller "foundered" bergs which enter the fjords and channels.

HUMAN REMAINS IN A RAISED BEACH.—During the recent long excursion of the geology class of the University of Edinburgh, an interesting find was made in the raised beach to the west of Pittenweem, on the coast of Fife. The storms of last winter have cut away some new slices of the coast, and laid bare fresh sections of the low raised beach which fringes the more sheltered parts of that coast-line. Portions of the skull, arm, and shoulder-bones of a full-grown skeleton were observed protruding from an upper argillaceous layer of the undisturbed gravel of this raised beach. In examining them, one of the phalanges of a child was likewise obtained. Some additional bones were picked up on the beach, but the greater part of the skeleton had no doubt been removed by the waves. From the position of the bones seen *in situ*, it was inferred by the students that the body had originally been cast ashore by the sea with one arm extended beyond the head, and that in this posture it had been covered up with mud and gravel. The stratum, containing the remains, lay about 4½ feet above the present high water-mark, and was covered with earthy sand.

NOTES

We regret to see what we must characterise as an unwarranted attack made upon Sir Wyville Thomson in the current number of the *Annals and Magazine of Natural History*, as to the disposal of the specimens obtained by the *Challenger* Expedition. Dr. Martin Duncan appears to have taken for granted that an extract of a private letter which some indiscreet friend of Mr. Alexander Agassiz published in *Silliman's Journal*, and which then found its way into the English journals, is "official." He would have done well to have ascertained whether this was really the case before allowing himself to comment on Sir Wyville Thomson's proceedings in such severe terms. So far as we are aware, out of the many naturalists actually engaged to work out the results of the *Challenger* Expedition, only *three* are not Englishmen, two being Americans, and one German. These three gentlemen are of the very highest repute in their respective branches, and Sir Wyville Thomson has, in our opinion, done well for science to secure their services.

A large and influential deputation of members of both Houses of Parliament, headed by the Duke of Richmond and Gordon, President of the Scottish Meteorological Society, waited on the Chancellor of the Exchequer on Tuesday to advocate that society's claims to State assistance. Sir Stafford Northcote said that the Treasury was prepared to grant 1,000*l*. for services rendered to Government during the past twenty years, and as regards the future he promised to consider the matter.

Mr. J. Russell Reeves, F.R.S., after whom that magnificent bird Reeves' pheasant was named, died on the 1st instant at Wimbledon, aged 73. As a young man in the H.E.I.C.'s service in China, Mr. Reeves contributed not a little to our knowledge of the flora and fauna of that country, several new plants and animals having been sent home or described by him. His love for natural history continued to the time of his death, and for some time he kept up a good aviary at his house at Wimbledon.

The Rhind lectures, delivered in Edinburgh by Dr. Arthur Mitchell, on the condition and antiquity of the cave-man of Western Europe, in other words the early, or earliest European of whom we have any knowledge, were brought to a close on Friday last. Dr. Mitchell showed that the cave-man's weapons of the chase and war were made of bone or horn, and highly finished, while his implements of stone were extremely rude, and calculated chiefly to serve as tools in the making of his bone implements, thus placing him in the bone rather than in the stone age of civilisation. From an elaborate examination of the objects which the cave-man has left, displaying an art-faculty, and from the study of the crania of the cave-people themselves, he showed that they must have possessed a high capacity for culture in all directions, and must have been as complete in their whole manhood as living Europeans. From an exhaustive examination of the cave-fauna, and of the actual fauna of Western Europe, Dr. Mitchell gave reasons, which certainly call for grave consideration on the part of archæologists, for believing that the antiquity of the cave-man of Western Europe is to be measured by a few thousands, and not by tens or hundreds of thousands of years.

The Anthropological Institute will hold a Conference at 4, St. Martin's Place, Trafalgar Square, on May 22, on the Present State of the Question of the Antiquity of Man, when the following papers will be read:—Prof. Boyd Dawkins, F.R.S.—"On the Evidence Afforded by the Caves of Great Britain;" Prof. McKenny Hughes—"On the Evidence Afforded by the Gravels and Brick Earth;" Mr. R. H. Tiddeman.—"On the Hyæna Bed in the Victoria Cave." Communications have also been solicited from foreign anthropologists.

The Paris Acclimatisation Society distributed its medals last Saturday at the Vaudeville. One of them was awarded to Mr. Alfred Mosenthal, Consul of the late Transvaal Republic, for his admirable work on the acclimatisation of the ostrich. Successful experiments on his system have been made on a large scale in Algiers.

Mr. Etheridge writes to the *Times* with reference to his examination of the red and green shales found below the depth of 1,073 feet in the boring at Meux's Brewery, and of which Prof. Judd spoke in a recent article in Nature on Deep Well-borings in London. He states that the evidence now shows them to be of palæozoic age, and of the continental type of Devonian rocks containing the molluscan fauna of that period.

At the April session of the German Geological Society Herr Speyer exhibited a number of fine palæontological specimens belonging to the Permian formation, obtained at a depth of 242 metres from borings in the vicinity of Memel. The twenty-five species found embraced eleven molluscs, five entomostracæ, two bryozoa, &c. Although nearly all of them are represented in the Lower Permian of Thuringia, Hesse, and Wetterau, but one-third of the number are found in the corresponding English formations. The above-mentioned borings yielded in the midst of the Permian formation occasional specimens of dolomite, with crinoidal stems and imperfect remains of brachypods, belonging properly to the Devonian.

The monument to Liebig to which we have previously referred, was unveiled at Darmstadt, his birthplace, on the 12th inst., the seventy-fourth anniversary of his birth.

The Annual Meeting of the Cumberland Literary and Scientific Association was held at Keswick on the first three days of the present month. This association, as we have previously intimated, is formed of a large number of local Cumberland societies, and both its first and its recent meetings have been highly successful. The idea of thus associating the various local societies of a county is admirable, and we would strongly recommend its universal adoption. The president at the last meeting was the Bishop of Carlisle, who gave a really interesting and fairly liberal address on the "Analogies and Contrasts between Human and Divine Science," the greater part of which consisted of an account of some recent advances in physical science. Several other papers were read, nearly all of them scientific, and more or less on subjects connected with the district. The new president is Mr. Isaac Fletcher, M.P., F.R.S., and the next meeting will be held at Cockermouth in May, 1878.

Commander Perrier read a paper at the last meeting of the Geographical Society of Paris, on the determination of the longitude of Algiers by telegraphy. The exact longitude is 2′ 50″·21 east from Paris, the probable error being only 0″·01. The time required for the transmission of the electricity from Paris to Marseilles was found to be only $\frac{2}{100}$ of a second; the distance between these two cities being 863 kilometres, it shows that the velocity of the electricity was not less than 46,000 kilometres per second. Similar experiments tried on the submarine cable between Algiers and Marseilles proved that the time required to travel was $\frac{23}{100}$ of a second; for a distance of 926 kilometres this shows a velocity of only 4,000 kilometres. But the battery used for signalling in the aërial line was composed of 100 elements, and only ten elements were used in the sub-Mediterranean cable. The triangulation of Algeria is an accomplished fact, and the calculations will be finished two or three months hence. When the operations shall have been completed it will be possible to know the exact length of an arc of meridian passing through the Paris Observatory, and extending from Shetland to Laghouat. The amplitude will be exactly 30°. An arc of parallel will be measured, also extending from Nemours on the Marocco frontier to Bona, in the vicinity of Tunis. The mean latitude will be 36° and amplitude 10°.

THE following courses of instruction for science teachers will probably be organised this summer at South Kensington:—1. Chemistry (Elementary), from July 4 to July 26, by Mr. W. Valentin, F.C.S.; 2. Sound, from June 19 to July 11; 3. Light, from July 12 to August 3, both by Prof. Guthrie, F.R.S.; 4. Steam, from July 4 to July 26, by Prof. Goodeve, M.A., and Prof. Shelley; 5. Biology, from June 13 to July 5, by Prof. Huxley, Sec. R.S. Details may be obtained by application to the Science and Art Department.

THE award of the Public Schools' Prize Medals of the Geographical Society for the present year has been as follows:—Physical Geography—Gold medal, Walter New, Dulwich College; Bronze medal, Arthur Smyth Flower, Winchester College. Political Geography—Gold medal, William John Newton, Liverpool College; Bronze medal, John Wilkie, Liverpool College.

ARTIFICIAL flowers called *barometers* are being now exhibited in a number of Parisian opticians' shops. They are coloured with a material composed of chloride of cobalt. When exposed to sun and dry air the leaves become deep blue; when the air is saturated with moisture they become pinky. All the intermediate shades are easily observed.

A REPORT from Dr. v. Bary on his recent excursion into the Tuareg region of the Western Sahara was read at the last meeting of the Berlin Geographical Society. His researches yield but few grounds in support of the theory that the Sahara was formerly the bed of a sea. He is inclined more to the belief that North Africa has long been free from a covering of water, as no traces of Tertiary formations were found, and the sand-downs cannot be regarded as proofs of the former existence of a sea. The traveller found the valley of Mihero not only remarkable for the number of crocodiles existing in its pools, but also on account of the rich growth of trees in striking contrast to the surrounding deserts. A mass of luxuriant climbing plants prevents the passage of beasts of burden.

FINNISH papers report that vast masses of smoke are issuing from a mountain adjoining the river Tana, and that the snow in the vicinity has been melted away. The region has hitherto been free from evidences of volcanic activity. The theory has often been advanced that the gradual elevation of the shores of the Gulf of Bothnia is due to volcanic forces, and it is possible that these are finally seeking a vent.

THE royal tigress in the Berlin Zoological Gardens, lately brought forth a litter of two, which she utterly refused to take care of. They were accordingly placed amidst the family of a Newfoundland dog, who welcomed the new-comers warmly and bestows upon them all necessary maternal attentions.

AN extensive movement of subsidence has taken place at Marano Marchesato, in the territory of Cosenza (Calabria). Vast chasms have opened, a great number of houses have been destroyed, and many others threaten ruin. The movement extends to the north, passing the hills of S. Fili e Bucita as well as to the river that divides Marano from Rende, the waters of which are partly escaping through large fissures in its bed. At the observatory of Cosenza there have been noticed for some time a barometrical depression of 10 mm., an extraordinarily low state of temperature with variable winds, fresh snow on the mountains, and a very abundant rainfall. The magnetic instruments, too, show an extraordinary agitation.

THE town of Iquique, in Peru, was visited by a destructive earthquake on May 10. The damage done is not so great as was at first anticipated, and it is stated that no lives have been lost. Early on the following day, between 2 and 3 A.M., a shock of earthquake is reported to have occurred at Comrie and the surrounding district of Perthshire; the shock, as usual, came apparently from south-west, proceeding to north-east, and was accompanied with a noise resembling that of distant thunder or the discharge of cannon. On May 2 several shocks of an earthquake were experienced in the neighbourhood of Löfta, in Sweden, causing some degree of damage.

IN the April session of the Berlin Anthropological Society, Baron v. Schleinitz, commander of the late German exploring expedition, gave an extended account of his anthropological studies among the inhabitants of New Guinea and the islands of the Melanesian Archipelago, which possess an interesting character on account of the isolated character of the region. The natives belong almost exclusively to the pure Papuan race. Three sharply-distinguished types were noticed. The first, prevalent in the northern part of New Guinea, is characterised by a thin, ill-shaped, hairy body, smooth face, thick lips, woolly hair, prognathous features, thin calves, &c. A second, occupying the islands of New Hanover and New Ireland, is slightly modified. The colour is a light brown, scarcely darker than that of South Europeans; the body is better proportioned and more fully rounded; clothing is not worn by the men and rarely by the women. A comparatively strict observance of morality, the rights of property, and family relations was, however, observed. A third race, found on the western coast of New Guinea, evidently possesses a slight mixture of Malaysian blood. They are russet brown and dolichocephalous, with intelligent and handsome features, and well proportioned form. Many of the tribes inhabit villages built on piles and well secured against attack. Polygamy is prevalent in certain regions, and a legalised system of marriage appears to be general.

WE have received from Prof. Henrici the sum of 1*l.* 10*s.* towards the Gauss Monument Fund.

THE additions to the Zoological Society's Gardens during the past week include a Hoolock Gibbon (*Hylobates hoolock*) from Assam, presented by Mr. John Scrymgeour; two Mauge's Dasyures (*Dasyurus maugæi*) from Australia, presented by Capt. J. C. Harris; an Antarctic Skua (*Stercorarius antarcticus*) from the Antarctic Seas, a Ceylon Hawk Eagle (*Spiczaëtus ceylonensis*) from Ceylon, presented by Capt. W. Vincent Legge; two White eared Conures (*Conurus leucotis*) from South America, presented by the Lady Greville; three Touracous (*Corythaix persa*) from West Africa, presented by Mr. J. G. Tayler and Capt. R. H. Crewe; a Saddle-billed Stork (*Xenorhynchus senegalensis*), a Black Sternothere (*Sternothærus niger*) from West Africa, five Kappler's Armadillos (*Tatusia kappleri*) from South America, purchased; a Wild Boar (*Sus scrofa*), born in the Gardens.

UNIVERSITY INTELLIGENCE

OXFORD.—The Master and Fellows of Balliol College give notice that they are willing to receive as members of the College without further examination, selected candidates for the Indian Civil Service, not exceeding in number ten, and to assist in their education. Any candidate who wishes to avail himself of this proposal is requested to communicate with the Master of Balliol.

CAMBRIDGE.—The Adams Prize awarded biennially for th best essay on some subjects of Pure Mathematics, Astronomy, or other branch of Natural Philosophy, the competition being open to all persons who have at any time been admitted to a degree in the University of Cambridge, has been adjudged to Edward John Routh, M.A., F.R.S., St. Peter's College. The subject of the essay is "The Criterion of Dynamical Stability." The value of the prize is about 250*l.*

A Warden of Cavendish College in the place of the Rev. T. J. Lawrence, resigned, will be elected on Tuesday, June 5. The College is intended for students somewhat younger than ordinary undergraduates, and the teaching and discipline correspond with those of the higher forms in a public school. The salary is 500*l.*, or a capitation fee of 5*l.* when the number of students exceeds 100. Candidates are requested to communicate with the Rev. Prebendary Brereton, Little Massingham, Rougham, Norfolk.

GLASGOW.—The Town Council has given a subscription of 5,000*l.* to the funds of the University of that city.

BRISTOL.—It will be seen from our advertising columns that a Principal is wanted for University College. For so young an institution the salary offered is very fair, and we hope that a thoroughly good man will be obtained for the post, one who, if not a man of science himself, at least regards it as of equal importance with literature.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 12.—"On certain Molecular Changes which occur in Iron and Steel during the separate acts of Heating and Cooling," by Prof. Norris, M.D., Queen's College, Birmingham.

An exhaustive study of the various conditions has led to the elucidation of the nature of hardening, softening, tempering, annealing, &c., and has further shown that numerical values may be assigned to these states.

The research has further established the existence in steel and in iron containing *free* carbon of a contraction or shortening which is excited by heat, and which proceeds simultaneously with the dynamical expansion, and masks its true amount. This is divisible into *high* and *low* temperature contraction.

The presence of a cooling expansion or crystallisation, which comes in during the dynamical contraction, and masks its true amount.

These effects, due to crystallisation and decrystallisation, are the causes of the so-called kicks, or temporary contractions and expansions which occur during the heating and cooling of the steel.

That the low-temperature contraction and cooling expansion are due to decrystallisation and crystallisation which occur during the acts of heating and cooling, while the kicks themselves are simply the thermal effects associated with these changes, and are proportionate to their extent.

That protracted annealing, that is, *extremely slow cooling*, brings about molecular separation of the carbon and iron; and steel in such a state contracts greatly when high temperatures are reached, producing the *contraction returns* seen at the end of the heating, and which are due to the condensation produced by the recombination of the carbon and iron. Steels in this state are less susceptible to cooling expansion (crystallisation), and therefore to low temperature contraction on subsequent heating.

April 26.—"Researches on Emeralds and Beryls.—Part II. On some of the Processes Employed in the Analysis of Emeralds and Beryls," by Greville Williams, F.R.S.

"On the Nature and Origin of the Beds of Chert in the Upper Carboniferous Limestones of Ireland," by Prof. Edward Hull, M.A., F.R.S., Director of the Geological Survey of Ireland. With "Chemical Notes," by E. T. Hardman, F.C.S., of the Geological Survey of Ireland.

From a review of the whole circumstances, it appeared that the origin of the chert-beds was to be attributed to the replacement of the original limestone or calcareous "ooze," due to organic agency by silica, and that the rock is truly a pseudomorph, a view held by several observers.

The manner in which this replacement had been brought about was then touched upon. It was shown that there was reason for believing that at the close of the period during which the carboniferous limestone was formed over the area of Central Ireland, the sea-bed was elevated so as to be covered by the waters of a shallow sea exposed to the sun's rays, and of a warmer temperature than when at a greater depth. The waters appear to have been charged with a more than usual supply of silica in solution, derived (as Mr. Hardman suggests) from the surrounding lands, formed, for the most part, of highly siliceous materials. As silica is less soluble than carbonate of lime, chemical replacement would naturally take place, the carbonate of lime being dissolved out and its place taken by the silica. The warm condition of the sea-water, its exposure to sunlight, the porous character of the coralline, crinoidal and other forms, and the soft and "oozy" condition of the foraminiferal mud would give easy access to the sea-waters, and the process of silicification would take place analogous to that described by Dr. Martin Duncan, F.R.S., as having occurred in the West Indies.

Linnean Society, April 19.—G. Bentham, F.R.S., vice-president, in the chair.—M. Cassimir De Candolle read an important paper on the geographical distribution of the Meliaceæ. His general conclusions with regard to the Melia family may thus be summarised: (*a*) The number and the mutual affinities of the various genera of Meliaceæ decrease from the Asiatic region towards Africa and America on one side and towards East Polynesia on the other; (*b*) Between the Meliaceæ of America and Africa there exists analogy, whilst Polynesian species belong to Indian type; (*c*) New Caledonia contains within itself a remarkable number of distinct species, the type of which, however, is Indian; (*d*) in Australia three Indian genera are found, along with three genera exclusively belonging to Australia; (*e*) No species of Meliaceæ have hitherto been collected in the most eastern islands of Polynesia; if subsequent observations reveal such it will be interesting to know whether they pertain to Indian or American type.—Another contribution on the geographical distribution of the Indian fresh-water fishes (Part II. The Siluridæ), read by Dr. Francis Day, curiously enough in some ways points to a similar conclusion to that derived from the plants above-mentioned. Dr. Day showed that of the twenty-six genera of Siluroidæ represented in the Indian Empire, ten are found in the Malay Archipelago, two more reach Cochin China or China, whilst *Clarias* only is common to India and Africa, and moreover it likewise is found in the Malay Archipelago. He infers that the said freshwater fish of India are more closely related to a Malayan than to an African fish fauna.—Mr. R. Irwin Lynch, of Kew Gardens, brought before the notice of the Society some observations on the disarticulation of the branches of *Castilloa elastica*, the caoutchouc tree of Central America. He has noticed that the lateral branches are detached from the ascending stem of the plant in a regular manner from below upwards in the same way as leaves, and this happens always at the point of insertion. In certain Euphorbiaceous genera which have leaf-like branches, these fall as does a leaf, and they bear in their axils a bud from which alone the permanent branches are produced. They are themselves subtended by a leaf reduced to a scale.—Capt. Chimmo followed by two communications, one concerning the mode of obtaining and the structure of the so-called *Euplectella* of the Philippines, the other a description of a supposed new Rhizopod.

Anthropological Institute, May 8.—John Evans, F.R.S., president, in the chair.—Special thanks were voted for the present to the Library of a complete set of the volumes relating to the voyage of the *Novara*, published and presented by the Austrian Government.—On an exhibition, by Mr. R. Biddulph Martin, of objects from a large refuse heap in the neighbourhood of Smyrna, Mr. Hyde Clarke, Col. Lane Fox, and the president offered remarks.—Mr. A. L. Lewis communicated a description of the remains of a stone circle at Colderham, Kent, illustrating his remarks by a well-prepared plan.—Dr. John Rae read a paper on the skulls of the Esquimaux, attributing the fact that two distinct types of skull exist among these peoples to an admixture of blood. An interesting discussion followed, in which Dr. Beddoe, Col. Lane Fox, and others took part.—Dr. Beddoe, F.R.S., communicated a paper on the Aborigines of Queensland, whom he described, on the authority of Mr. Christison, who had had many years' knowledge of them, and employed them very largely in sheep-farming, to be, in many respects, not so black as they have been painted.

Royal Microscopical Society, May 2.—H. C. Sorby, F.R.S., president, in the chair. A number of donations to the society were announced, including a sum of 500*l.* presented by Mr. C. Lambert, from a bequest of 25,000*l.* left by that gentleman's late father, to be appropriated to benevolent and scientific purposes.

—The first of a series of lectures founded in honour of the late Prof. J. Quekett was delivered by Sir John Lubbock, Bart., M.P., "On Some Points in the Anatomy of Ants." Commencing by reference to the occasion, and appropriately giving a short history of the life and labours of Prof. J. Quekett, the lecturer proceeded to describe in a minute and interesting manner the general structure and microscopic anatomy of these insects, pointing out the differences found to exist between individuals of different species, and also between the various classes of the same species. Attention was specially drawn to the structure of the antennæ, and to certain organs presumed to be those of hearing, also to the structure of the mouth, with its extensive muscles and mouth-sac. At the conclusion of the lecture, the "Quekett Medal" of the society, struck for the occasion, was presented to Sir John Lubbock by the president, amidst great applause from the fellows.

Victoria (Philosophical) Institute, May 7.—Dr. C. Brooke, F.R.S., in the chair.—A paper on the indestructibility of matter by Prof. Challis, F.R.S., was read.

ROME

R. Accademia dei Lincei, March 4.—The Roman Tuscia and the Tolfa, by M. Ponzi.—On graphical statics, by M. Battaligni.—On some cavern-myriapods of France and of Spain, by M. Fonzago.—Studies on some anouran amphibians of Piedmont, by M. Lessona.—On a new function of the liver and the effect of ligature of the vena porta, by M. Tommaso-Crudeli.—On the Meibomian glands, by the same.—On the chemical constitution of the cyanamides, by MM. Fileti and Schiff.—On the tenacity of copper, steel, brass, and aluminium, at various temperatures, by M. Pisati and others.—On the dilatation, capillarity, and viscosity of fused sulphur, by M. Pisati.—On organisation of the meteorological services of forecast for agriculture; on publication of meteorological observations; and on history of the atmosphere, April to September, 1876, by M. Tarry.—On the small proof plane, by M. Volpicelli.—Ephemerides and graphic representation of the height of the water surface of the Tiber, measured daily in 1876, by M. Belocchi.—On titanite and apatite of the plain of Spedallacio, near Sarsalba, and on mancinite, by M. Uzielli.—Indian corn and pellagra, by M. Selmi.

PARIS

Academy of Sciences, May 7.—M. Peligot in the chair.—The following papers were read:—Two general laws of geometric curves, by M. Chasles.—Studies of Mr. Sylvester on the algebraic theory of forms, by M. Hermite.—Note *à propos* of M. Favé's communications on the theory of heat, by M. Resal. He opposes M. Favé's views.—On determination of the difference of longitude between Paris and Berlin, by M. Mouchez. The first series of astronomical observations are on the eve of completion.—Researches on the law of Avogadro and Ampère, by M. Wurtz. Oxalate of potassium loses its water when heated in dry air under a certain pressure, but does not lose it, if heated under the same pressure, in chloral vapour or in a mixture of air and water vapour. We may infer that hydrated chloral vapour does not act like dry air, but like a mixture of anhydrous chloral and water vapour.—Chemical researches on the green matter of leaves, by M. Fremy (third paper). He thinks it proved that the colouring matter of leaves is a mixture of phylloxanthine and phyllocyanate of potash. During life chlorophyll acts by decomposing CO_2. When the leaves die and fall the colouring matter is destroyed and gives up to the ground the salt of potash it contained.—Change of colour of chlorophyll; its passage to blue and to red or orange, by M. Trecul.—On meteorological predictions sent from the United States, by M. Faye. Some think the success of these owing to the *rôle* of the Gulf Stream, which they suppose to be the grand route taken by storms in traversing the ocean. M. Faye shows this to be a mistake; the path of storms depending on currents in the higher regions of the atmosphere.—On the identity of anthrax in all the species of domestic animals, by M. Bouley. Contagion is its most essential character.—M. Bernard made some remarks in presenting his *Leçons sur le diabete et la glycogénèse animale*.—Reports on the geodetic and topographic works executed in Algeria, by M. Roudaire. This relates to his measurement of the meridian of Biskra, and survey of the region of the Chotts.—Practical reduced form of the development of Taylor, by M. Rouyaux.—Integration of linear differential equations of any co-efficients, with or without second member, by M. André.—Solar spots observed at Madrid in April, 1877, by M. Ventosa. Another confirmation of what M. Janssen observed.—On M. Janssen's communication on the sudden formation of a very important sun-spot, by M. Gazan. He thinks the spot was not formed suddenly; all spots are preceded by violent agitation of the luminous matter. He passes some other strictures.—Researches on accidental double refraction, by M. Maré. The double refraction produced by hardening is identical with that produced by a regular heating of the contour of the plate.—On the interior resistance of thermo-electric elements, by M. Rolland. The experiments were made with Clamond's pile. The curve of resistances oscillates continually; its course, at first rather irregular, becomes nearly normal only after about twenty minutes; it is then fixed at a height which it retains during about ten minutes. When the pile cools the curve again oscillates irregularly.—On acid acetates, by M. Lescœur.—On some derivatives of acetylacetic ether, by M. Demarçay.—On nitrosalicylic acid, by M. Phipson.—Action of toxical and antiseptic vapours on the fermentation of fruits, by MM. Lechartier and Bellamy. The action of vapour of phenic acid, cyanide of potassium, and camphor destroys or diminishes considerably the vitality of fruit cells.—On the same subject, by M. Gayon. He tried chloroform, ether, and sulphide of carbon with similar results. Sulphide of carbon and camphor (in the two series of experiments) acted less powerfully than the other substances; they allowed a little fermentation.—On two new niobates, by Mr. Lawrence Smith. These, found in North Carolina, he names *Hatchettolite* and *Rogersite*.—Researches on the mode of formation of Cyclopia, by M. Dureste.—Note on the peristaltic movement of the intestine, by M. Guerin. The matters in the intestine are not moved along by an action *a tergo*, resulting from simple circular contraction of the muscular membrane, but by a double propulsive and suctional action, realised through contraction of the circular and longitudinal planes of the intestine.—On a whale, properly so called, caught in the Bay of Tarentum, by M. Capellini.—On the mines of New South Wales, by M. Simon.

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THURSDAY, MAY 24, 1877

SCIENCE AND WAR

II.

At no other time has there been so much want of unanimity among the Great Powers of Europe on the subject of Ordnance. There are to be found at the present moment cannon of a dozen different descriptions in the gun parks of European nations, differing from each other not only in respect to their construction, but in the metal of which they are made. So far as small arms are concerned, we know there is but one opinion; some nations prefer one breech-loader to another, but all agree in the employment of breech-loaders. In the case of cannon, however, it is different. Germany relies upon breech-loading ordnance, while Great Britain has forsaken the system and gone back to muzzle-loaders; Austria makes her guns of bronze, Germany of steel, Russia favours steel and brass, America cast iron, while England has cannon of steel encompassed with iron, and France weapons of iron girt with steel.

The balance of favour is beyond question with the breech-loader at the present moment. All the new artillery of the Russians and the Turks is of this kind, while the field-guns both of the Germans and Austrians are upon the same system. France has done nothing lately for the regeneration of its ordnance, and there remains but Great Britain and Italy to represent muzzle-loading artillery. But Italy, although she has adopted the British system for very heavy guns, is by no means a confirmed believer in it, and will doubtless hesitate before following our example very far, beset, as she is, with neighbours armed with breech-loaders.

Of all the Powers, it is, curiously enough, steady-going Austria, which has taken the boldest and most independent course in the matter of artillery. It was but at the end of 1875 that the Austrian War Office decided to adopt the Uchatius cannon for field artillery, and yet at this moment every artillery regiment of the vast Austro-Hungarian army is armed with the new weapon. Within eighteen months no less than 2,000 of these cannon have been cast and finished, and now the Vienna arsenal is engaged in the manufacture of heavy guns of the same character. Never was a more energetic step taken. A new cannon of some sort was held to be absolutely necessary to uphold the prestige of the army, and a Commission having been intrusted with the selection of an arm, pronounced without delay in favour of the scheme brought forward by Gen. von Uchatius. In October, 1874, the first round was fired from an Uchatius gun, and a twelvemonth afterwards the sweeping reform which was to introduce an entirely new artillery throughout the Austrian service was decided upon. Government sanctioned an expenditure of 1,800,000*l.* to be spent in two years, and Gen. von Uchatius was directed to give all the assistance in his power towards the fulfilment of the design.

The Uchatius gun is made of so-called steel-bronze. Chilled bronze would be a better name, since Uchatius casts his metal in a chilled, or metal mould, in the same manner, pretty well, as Sir William Palliser produces his famous chilled projectiles. Bronze, as everybody knows, has been a favourite metal with gun-founders from the earliest days, and in the East, especially, magnificent castings of this nature have been produced. About 90 per cent. of copper and 10 of tin is the mixture commonly employed in making ordinary bronze, but 8 per cent. of tin is the proportion preferred by Uchatius. The difficulty in casting bronze, as those who have any experience know full well, is that of securing homogeneity, soft particles of tin becoming isolated in the mass, and giving rise to the defect known as "tin-pitting." Whether we have lost the secret of bronze-casting, or whether in former times they were more skilful at the work, certain it is that founders of the present day are unable to secure so uniform an alloy as formerly. This was very apparent when some eight or ten years ago our own Government adopted, for a brief time, bronze artillery. The addition of a small percentage of phosphorus did not mend matters, and the highest authorities on the subject were at a loss to suggest an effective remedy. Our bronze guns, too, had another defect which could not be overcome. After firing the bore became affected, and the weapon, as it was termed, "drooped at the muzzle." These were the two defects indeed that led mainly to the abandonment of the bronze gun in this country, and they are, too, the difficulties which Gen. von Uchatius appears to have overcome. He has got rid of "tin-pitting" and his guns do not "droop at the muzzle."

Uchatius found that by subjecting the alloy in a liquid form to considerable pressure, he was enabled to secure a perfectly homogeneous mass, a result which was also furnished, he discovered when he had gone a step farther, if the molten metal was rapidly cooled. Steel-bronze is apparently made much in the same way as the toughened glass, of which we have heard so much lately. After being cast in a mould, the alloy is thrust into a reservoir of oil, heated to a high temperature, so that the metal suddenly cools, but only down to a certain point. Then the casting is withdrawn and allowed to get cold more gradually. A regular and crystalline structure is in this way produced, which has none of the defects of ordinary bronze. It is a moot point whether phosphorus enters into the composition at all. Chemists tell us they can find no trace of it, but this is no absolute proof that a small percentage of the element was not originally contained in the alloy, being burnt out after it had done its work of harmonising the two metals. The inventor is rather reticent on the point, but in any case, it is very certain that he produces a uniform and homogeneous alloy of a hard crystalline nature.

One other expedient Uchatius has recourse to in making his cannon. When he has cast his gun and chilled it, he proceeds to dilate the bore. Wedges of steel, shaped in the form of cones are forced into the tube of the gun one after another, until the calibre of the weapon has been increased by something like seven or eight per cent. This expansion or dilation of the tube has not only the effect of hardening or steeling the core, but also of rendering the gun more elastic and capable of resisting more effectually the strain put upon it at the moment of firing. The gun, after this process, is in a state of elastic tension, and it is said that there is a pressure from without, inwards, equal to that which was exerted to dilate the gun in the first instance; and that this is actually the case can scarcely be doubted, since it is a fact that a section of the gun before being quite severed, will tear itself loose with considerable violence, and will be found on separation to have partially returned to its former calibre.

So far as practical trials have been conducted with the weapon, the Austrian Government have every reason to be satisfied with the Uchatius gun, which compares favourably with the Krupp steel cannon in the matter of accuracy and durability; while as regards its cost, it is far cheaper than any other rifled ordnance. A steel field-piece costs upwards of 100*l.*, even when not protected with rings, while the iron-steel weapon manufactured in this country, costs about 70*l.* sterling; the steel bronze cannon of Gen. von Uchatius, on the other hand, are made for 35*l.* apiece.

In construction, the Austrian gun is so similar to that of Herr Krupp, of Essen, that the latter claimed compensation for an infringement of his patent when the manufacture of the Uchatius gun was first commenced. The Essen works, our readers may know, supply not only Germany with steel breech-loaders, but have provided the present belligerents with all their modern artillery. Russia has still many brass cannon on hand, and Turkey a goodly number of Armstrongs, but both powers mainly depend upon their steel Krupps. These stood the German army in such good stead during the last war that their reputation is firmly established. They are of crucible steel, and the breech, instead of being upon a hinge, or in the form of a block, moves round in a D-shaped socket, the escape of gas being further prevented by rings of phosphor-copper.

The manner in which the ordnance of this country is constructed is sufficiently familiar to our readers. A tube of steel is encompassed by jackets of wrought-iron, and in this way the toughness of the latter is combined with the hardness of the former. All our guns, as we have said, load at the muzzle, while those of Russia, Germany, Austro-Hungary, and Turkey, are breech-loaders. Italy, in the case of the 100-ton guns with which she intends to arm her two stupendous turret-vessels, the *Duilio* and *Dandolo*, has adopted our method of construction, except that she employs smooth, instead of studded, projectiles. With the employment of a gas-check at the base of the shot to prevent windage and so secure the full force of the exploding charge, the use of studs in a shot appears to be unnecessary, a sufficient spin being imparted to the projectile by the soft metal of the gas-check before-named, which causes the shot to rotate after the manner of a Snider bullet. So satisfactory, indeed, were the Italian trials of these projectiles last year that it is by no means improbable that we, too, may give up the use of studded shot.

As to the comparative value of breech-loaders and muzzle-loaders, we shall not offer an opinion. No doubt a muzzle-loader is the stronger weapon, because its breech is solid, but our cousins, the Germans, urge very justly that since their guns do not burst, they are quite strong enough. Advocates of the muzzle-loading system argue again that their weapon is more simple in construction and for this reason is to be preferred; but on the other hand the sponging and loading of a gun is more easy to effect, if it opens at the breech. Indeed, in the case of very heavy guns located in a casemate or on board ship, the Germans reproach us with the assertion that we must needs have recourse to all sorts of complicated and awkward machinery in loading, while in their case a simple pulley or crane is all that is necessary. Either, say they, we must expose our gunners through the open port when loading, or, as in the case of the *Thunderer*, rely blindly on hydraulic apparatus to work the guns for us. So stands the question; perhaps the present war will bring us a solution of it.

H. BADEN PRITCHARD

THE FORESTS OF PEGU

Preliminary Report on the Forest and other Vegetation of Pegu. By Sulpice Kurz, Curator of the Herbarium, and Librarian, Royal Botanical Gardens, Calcutta. (Calcutta: C. B. Lewis, 1875.)

INDIAN forest reports have of late years become as plentiful as the proverbial blackberries. The frequent appearance of them is a consequence that might be expected when we consider the wide range of country which comes under the supervision of the Forest Department of India. So far as bulk or quantity of printed matter is concerned, no one can say that these forests are not fairly represented in the Government papers which appear in the course of a year, but the quality of these reports is another question. They too often contain merely the dry details of work carried on during the year, and are interesting only to those immediately connected with the special department from which the reports emanate. Occasionally, however, a report is issued which in reality is something more, containing much valuable information on subjects connected with forest conservancy, and amongst such Mr. Kurz's may be classified. It is, in fact, rather a description of the vegetation of Pegu, to which are added appendices occupying quite two-thirds of the whole bulk of the volume. Taking the actual report itself, which, as indicated in the title, is of a preliminary character, the matter in which will be worked out in Mr. Kurz's forthcoming book, we find it divided into two parts, first, the "General Report," and second, the "Special Report." The general report is again divided into two sections—(A) A general aspect of the country, its geological and climatological features, in connection with the flora. (B) A botanical description of Pegu, with special reference to its forests. After a very brief topographical sketch of Pegu, Mr. Kurz considers the geological aspect of the country from a botanical point of view, which, unlike that of the true geologist, is not to consider the age of the rocks, &c., but simply their extent and quality, from which inferences may be drawn of the vegetation found growing upon each formation. The geology of Pegu is described as being very simple and uniform, the hills being composed solely of sandstone, skirted at their base by a strip of diluvium, "interrupted by a deeper or shallower alluvium wherever choungs come down from the hills, and succeeded by the vast alluvial plains, through which the Irrawaddy and Sittang flow." The laterite formation is described as being of the highest importance in the various floras of India. The term laterite, as generally used by foresters in Burmah, comprises several heterogeneous rocks and soils, all characterised by a more or less ferruginous appearance, but really connected in no other way than that they are all permeated by hyperoxide of iron. "No other formation," Mr. Kurz writes, "except metamorphic and volcanic ones, can boast of such a variety of species, in spite of its apparent sterility, as laterite. It is this rock that affects vegetation so much that the great difference between the floras of Malacca, Borneo, Sumatra, &c., on the one hand, and that of Java on the other side, is produced. It is also this formation which allows so many Australian genera, like *Melaleuca*, *Bœckea*, *Tristania*, *Leucopogon*, &c., to spread so far to the north-west, some of which, like *Tristania*, spread as far north as the Ava frontier. If all laterite plants were to be erased from a list of the plants of Pegu proper the flora would be rendered very uninteresting indeed."

The seasons of Pegu seem to be similar to those of Lower Bengal; the cold season, however, is shorter, and the hot, dry, as well as the rainy seasons, are earlier by a month than in Calcutta. The dry season ranges from about December to April, the cold part of which terminates usually, and sometimes, abruptly about the end of February; during this period the thermometer rarely rises above 88° in the shade, sinking as low as 57° and sometimes to 55° and 54° before sunrise. Heavy dews prevail and fogs are plentiful in the early hours of the day, the after parts of which are clear and bright. During this season rain is almost unknown. The hot part of the dry season comprises the months of March and April, during which time, chiefly in the former month, an occasional heavy thunderstorm moderates the intense heat. In the early part of May the regular monsoon rains, which cease during November, set in. The thermometer, during the hot season, rises rapidly to 95° and 100° in the shade. The nights, however, are cool and refreshing. In the hottest province of the country, at the height of the season, the thermometer never registered above 74° before sunrise. Great heat and dryness prevails in the open country, and on the ridges, "while in the narrow valleys of the eastern slopes of Yomah, and in the Martaban hills, where evergreen forests skirt the streams, dew often falls so heavily, that one becomes quite wet when marching in the early mornings through the herbage along their bank. But after an ascent of 100 or 200 feet, we meet with the same dryness again in the deciduous forests, as in the open lands. It is here that we can almost every morning observe a white sheet of vapour in the depths of the valleys, resting on the forests, which enables us to appreciate clearly the *rôle* which evergreen forests play in the attraction of the currents of vapour."

The foregoing abstracts will serve to show the nature of Mr. Kurz's report, a good deal of which is interesting. It might, however, have been considerably condensed without losing any of its value, indeed its value would have been much enhanced.

Whatever advantages or disadvantages Burmah may present to travellers generally, to a botanist the advantage must be very great in having specimens ready gathered, and only requiring to be picked up, for we are told that owing to the extreme violence of the gales which prevail at the end of April and the beginning of May, the amount of old trees, branches, &c., thrown down is often astounding, offering an easy and fruitful harvest of specimens of woody plants, otherwise quite out of reach on account of their height. At other seasons apes and squirrels are most useful agents for procuring flowers and fruits of lofty trees. Another advantage is that in the cold season there is an absence of mosquitoes. Space will not allow us to do more than mention the appendices which we have before referred to. In the introduction to the first, which is a list of Burmese trees arranged in their natural orders, with brief remarks as to the quality and appearance of the wood, &c., we are treated to some remarks on the value or otherwise, of native names of plants. Mr. Kurz says, "there are still very respectable botanists and practical men who look upon native names for plants as something absolutely reliable, some even believe that native names are preferable to scientific ones, because the former are permanent, and are not altered from one day to another, as is the case in science." Our own experience is that while a native name, is often a great help to the accurate determination of the genus or species, it is, on the other hand, often a delusion and a snare, for it frequently occurs that the same name is applied indiscriminately to plants even of distinct natural orders. This, perhaps, is not so much the case with Indian plants as with plants from other countries. Mr. Kurz points out the difficulty that must always present itself in India, where the same plant is known in different localities by different names, whereas the scientific name would be identical all the world over, or if not actually identical, certainly traceable. This lengthy report concludes with some extracts from the author's journal of his tours. It is illustrated with a sketch map of Pegu, and two plates of a new genus *Mayodendron*, named in honour of Lord Mayo, and one of the appendices contains a botanical description of this plant.

INFLUENCE OF CLIMATE ON PULMONARY CONSUMPTION

Influence of Climate in the Prevention and Treatment of Pulmonary Consumption. Lettsomian Lectures for 1876. By Charles Theodore Williams, M.A., M.D., Oxon. (London: Smith, Elder, and Co., 1877.)

THE fact that consumption is the great destroyer of men and women in the prime of life, nearly one-eighth of all the deaths which occur being due to it, is more than sufficient to warrant any amount of minute and patient inquiry which might result in the prevention and more successful treatment of this terrible disease. Dr. Williams treats the subject in its climatic relations, and gives what is, in many directions, an elaborate and able discussion of a large amount of fresh data adduced with reference to the therapeutic action of British, Mediterranean, African, Indian, Australian, sea-voyage, and other climates, differing widely from each other as regards temperature, humidity, elevation, and exposure to sudden changes of weather. In concluding the inquiry he draws some valuable conclusions as to those patients who ought to winter abroad, and those on the other hand who may remain at home, those who are most likely to be benefited by sea-voyages, and those most benefited by dry climates, and how far the temperature and elevation is to be taken into account. The question of moist climates, whether hot or cold, is also examined with the view of ascertaining whether such climates are desirable at all for consumptive patients. The results would have been rendered even more valuable if the author had availed himself more freely of the labours of others who have written on the same subject.

But it is the remarkable conclusion arrived at regarding the winter climates of the south of England which arrests attention. This conclusion is that Torquay, Bournemouth, Ventnor, and the whole of the western end of the English Channel possess climates less beneficial to consumptive patients than are the climates of Hastings, St. Leonards, and the eastern end of the Channel; and it is thence inferred "that it is the stronger influence of the Atlantic warm current and its accompanying winds on the shores of Devonshire and Cornwall, which though it raises their winter temperature many degrees, clothing their hillsides with verdure and causing what would otherwise be waste places to bloom with rare exotics, deprives them of the stimulating and bracing influence which is possessed by the less beautiful shores of Sussex with its breezy downs and colder winter climate."

It may be doubted if the facts warrant this very broad conclusion. It has been shown by Buchan and Mitchell in their discussion of the weather and mortality of London, published in *Jour. Scot. Meteorol. Society*, vol. iv., p. 205, that the three periods of the year most fatal to consumptive patients are November and the first half of December, when the temperature is rapidly falling and is at the same time low; in January when the temperature falls to its annual minimum; but chiefly from March to about the middle of June, when the air is driest. Now these results, which are based on the enormous population of London and the long period of thirty years, would have led to the expectation that the winter and spring climate of Torquay was certainly not less beneficial to consumptive patients than that of St. Leonards.

It seems not improbable, from an examination of the whole facts, that the discrepancy may be accounted for by the comparatively small number of patients whose cases have been discussed by Dr. Williams, viz., 243, of whom 100 were under medical treatment at Torquay, 58 at Ventnor, 57 at Hastings, and 20 at Bournemouth, and to an important point missed in the discussion, viz., the very different types of weather which have prevailed in the different years and the varying mortality from consumption attending on these types of weather. It is indispensable in such a discussion that tabular statements be prepared, showing the number of patients under medical treatment for consumption at each place during each month of each year, and the results of the treatment as respects each patient, in order that the results may be compared with the meteorology of the place and year to which they refer. Till this be done we cannot be said to be in a position to make any comparative statement of the therapeutic effects on consumptive patients of the climates of the different sanatoriums of the south of England; it being evident, for instance, that the relatively high position of Hastings as a sanatorium for consumption may be wholly due to a chance excess of patients sent there during exceptionally mild seasons, and the relatively low position of Bournemouth to the mere accident of one or two consumptive patients more than the average being there in a particular season when the weather happened to be peculiarly severe.

OUR BOOK SHELF

Annals of the Astronomical Observatory of Harvard College. Vol. xiii.

WE must congratulate the authorities of Harvard College on the publication of the Annals of the Observatory. The volume is the result of a large portion of the work of the institution during the time that it has been without a director, since the death of Prof. Winlock in June 1875. An account is given of the several funds available for purpose of publication, and then follow the biographical notes of W. C. Bond, G. P. Bond, and J. Winlock, the several directors since the foundation in 1815. We then come to the details of the instruments in use and plans of the observatory and grounds. The work done from the year 1855 appears to have been chiefly the measurement of binary stars, transit observations, investigation of lunar phenomena, drawings of nebula, photographs of the sun, and spectroscopic observations, the latter consisting largely of the examination and drawing of the chromosphere. In Part II. we find some thirty-four exquisitely-finished plates depicting the results of the foregoing observations, published at the expense of the Bache fund. These were made by or under the direction of the late Prof. Winlock. It seems a pity that the authorities do not publish from time to time a selection of these papers on special subjects. Part II. would be widely bought by astronomers if its contents were given separately, and the plates, which are, perhaps, the finest accessible, were practically not buried in an odd volume of a lengthy series of "annals."

Cultivated Plants; their Propagation and Improvement. By F. W. Burbidge. (Edinburgh and London: William Blackwood and Sons.)

THAT Mr. Burbidge possesses the pen of a ready writer no one can deny when it is borne in mind that in a very short time he has produced several books on horticultural or gardening subjects. His "Domestic Floriculture," published by the same firm as the present volume, was, up to that period, the best of his productions; for though it was not of a scientific character, it was of a nature calculated to elevate window gardening from the mere habit of simply allowing a few ordinary plants to struggle for an existence through adverse circumstances to a system in which all might take an interest.

The present volume is one of a different character from any of those which have preceded it. Mr. Burbidge, in fact, says in his Preface that the primary intention of the book was as a popular handbook on plant propagation and improvement, with a hope also that it might "serve young gardeners as a stepping-stone to works of a higher scientific character, and more especially to those of Charles Darwin." Nevertheless, the chapters or sections devoted to "Hybridising and Cross-breeding," "Natural Fertilisation and Cross-breeding," and "Artificial Fertilisation and Cross-breeding," will be useful as bringing together from various and widely scattered sources, what has been done in these cognate branches of scientific research. In these sections we think Mr. Burbidge has done his work well, the references to the quotations being fully given not only to English but also to French and German works.

The great bulk of the volume is devoted to a "General review of some of the most popular groups of cultivated plants, with notes on their propagation and natural affinities." In this the arrangement of the orders is somewhat novel, for instead of being classified in a scientific manner they are placed alphabetically. The habits and peculiarities of the principal plants in each order are briefly described as well as their economic uses, together with notes on the most general method of propagation.

The book will no doubt meet with a wide circulation; the chapters on propagation, grafting, and budding, being of a practical character, will be useful to other readers besides those of a purely scientific turn. As a further illustration of this we may point to the chapter on Seed-saving, in which we are reminded of the excellence of the produce of the Continental seed farms, especially those of Erfurt, which are noted for their Primulas, Stocks, Balsams, Asters, &c.; we are also told that the seed of such common plants as Cineraria and Calceolaria is, when of a "good strain," worth from 10*l.* to 15*l.* per ounce, and Primula seeds even more. The book has a good index, always a special point of value in one intended for reference.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The "Hibernation" of Birds

I TRUST your correspondent "X" will not object to my pointing out that the "hibernation" story which he retails in NATURE (vol. xvi., p. 43) has in common with dozens, not to say scores, of others, the defect of being delivered at second-hand, without even the slightly confirmatory evidence which the name of the observer of the marvel and of the place where it occurred would supply. As such it may surely be dismissed to the limbo of (I will say) legends. That which has been furnished by the Duke of Argyll (vol. xv., p. 528) rests on a better base, and is almost the first I have met with for which a respectable man vouches. Far be it from me to deny the possibility of a miracle being wrought in Persia, but I confess that without impugning Sir John McNeill's veracity, I simply refuse to believe the story except I regard as miraculous the incident he relates. Now there is a common supposition that miracles are only performed with some worthy end in view, and, moreover, that unless a miracle is recognised as such by the witnesses thereto its effect is nullified. This supposition may, however, be wrong, for it is hard to conceive what may have been the end of this miracle, and its supernatural quality is clearly not acknowledged by the distinguished persons present. One would find greater satisfaction, perhaps, if the Duke of Argyll had expressed his belief in it, but he contents himself with saying that he had "frequently heard" his brother-in-law relate the circumstance. The identity of this phrase with that said to have been used by another great man on another occasion is very striking, and since the story, though old, is short, perhaps you will let me tell it. I have heard that King George IV., some years before his death, was strongly possessed with the notion that he had been present at the battle of Waterloo. Once as he was recounting his personal adventures on that eventful day, he perceived some uncourtly sign of incredulity shown by one of his audience. Turning to the late Duke of Wellington, who was of the company, he appealed to him—"Isn't that true, Arthur?" The adroit reply was—"I have *frequently heard* your Majesty say so."

May 19 THE REVIEWER OF PALMÉN'S WORK

Barlow's and Laslett's Determination of the Elasticity and Strength of Timber

ON comparing the results of Mr. Barlow's determination of the modulus of elasticity of Teak timber, given in his "Strength of Materials," p. 82, sixth edition, with the results of different Dutch investigators on Djati timber of Java, I was struck by the great difference between those results, those of Mr. Barlow being very much higher than the others, though Teak of British India and Javanese Djati are merely different names for the same species, *Tectona grandis*, L., of the family Verbenaceæ. My own experiments, which will, I hope, be afterwards published in my inaugural dissertation, show also the complete identity between Teak and Djati. This led me to detect a serious error in Mr. Barlow's calculations. He measures the deflection produced by a certain weight of a batten 7 feet by 2 inches square, supported at both ends on two props, the bearing distance being 6 feet, as is stated on p. 67 in the general description of his arrangements. The value of E is found in this case by the formula—

$$E = \frac{l^3 w}{16 a d^3 \delta}$$

in which l is the bearing distance; in the case of Mr. Barlow, equal to 6 feet. Now all Mr. Barlow's E's are calculated by introducing $l = 7$ feet instead of $l = 6$ feet, as should be; the consequence is that all those values are too great. So for Teak timber the value of E is found to be = 603,600 lbs., while the true value is $E = 380{,}023$ lbs. on the square inch. Also in the formula for the strength—

$$S = \frac{l w}{4 a d^2}$$

l is the bearing distance, Mr. Barlow calculates $S = 2{,}462$, taking $l = 7$ feet, the real value being $S = 2{,}110{\cdot}5$ for $l =$ 6 feet.

Mr. Thomas Laslett, in his "Timber and Timber-trees" (London: Macmillan and Co., 1875), following the arrangements of Mr. Barlow, commits the same error. On p. 42 he tells us that in all his experiments pieces were taken $2'' \times 2'' \times 84'' =$ 336 cubic inches, and that each piece was placed upon supports exactly 6 feet apart. But for l is taken, instead of the bearing distance 6 feet, the whole length 7 feet. So all the numbers for the moduli of elasticity of the different woods calculated by Mr. Laslett are too great in the proportion $7^3 : 6^3$, and the numbers for the strength in the proportion $7 : 6$.

The reduced values for E from Mr. Laslett, namely, $E =$ 362,870 and $E =$ 305,876, and that from Mr. Barlow, $E =$ 380,023, agree tolerably well with the mean results of Dutch investigators, $E =$ 404,210, and much better than does the uncorrected value, $E =$ 555,180. Other determinations of the E of Teak timber are not known to me.

The results of Mr. Barlow were already published in the year 1817. Since that time several editions of this valuable work have appeared; in the year 1867 the sixth edition, revised by his two sons. General Morin gives in his "Résistance des Matérieux" all the results of Mr. Barlow on timber, reduced to metric weights and measures. Also MM. Chevandier and Wertheim, in their "Mémoire sur les Propriétés Mecaniques du Bois." It is scarcely to be believed that none of these eminent men, nor any one else, have remarked this error in the calculation of Mr. Barlow's often used numbers. S. FIGEE

Haarlem, Holland

Basking Shark

I THINK it but just to Prof. Bocage to ask you to publish the inclosed letter, which only reached me on the first of this month, owing to its having been sent to a wrong address. I regret that I overlooked Signor Capello's memoir on *Selache maxima*, which was so plainly indicated in the *Zoological Record* for 1869. I had, indeed, the "Catalogo dos peixes de Portugal que existem no Museu de Lisboa, por F. de Brito Capello," which was published in No. vi. of the Lisbon *Journal of Science*; but No. vii., which the author (whose kindnesses to me when at Lisbon, in 1868, I cannot forget) sent to me, I never got, and hence one cause of my oversight. E. PERCEVAL WRIGHT

Trinity College, Dublin

Lisbonne, le 14 février, 1877

MONSIEUR ET HONORÉ CONFRÈRE,—Dans l'article que vous avez publié dans la NATURE sur le "Basking Shark," vous avez, comme le Professeur Paul Gervais et d'autres, attribué à M. Steenstrup la découverte des appareils tamisants ou fanons branchiaux du squale pèlerin ou *Squalus maximus*. Si vous vous donnez la peine de consulter No. vii. du *Journal des Sciences Math., Phys. et Nat. de Lisbonne*, vous y trouverez, à p. 236, la description de cet appareil; vous trouverez également ces appareils figurés sur la planche qui accompagne cet article et qui contient aussi la figure du poisson. La description et la figure des appareils branchiaux ou des fanons branchiaux du *Sq. maximus* (et des espèces congénères) ont été donc publiés par M. Capello, aide-naturaliste au Muséum de Lisbonne, en août de 1869; c'est-à-dire 4 ans avant la publication de l'article de M. Steenstrup. Si vous consultez le *Record of Zoological Literature* pour 1869 (vol. vi.), vous y trouverez, p. 139 :—

"*Selache maxima.*—A *detailed* description and figure of an example from the coast of Portugal is given by M. Capello under the name of *Cetorhinus blainvillii*, *Jour. Ac. Sc. Lisbon*, No. vii., p. 233."

Je vous envoie par la poste le No. vii. du *Jour. des Sc. de Lisbonne.*

J. N. BARBOSA BOCAGE

Gold in Carboniferous Conglomerate

MANY of your readers are aware that the fact of the occurrence of gold in Lower Carboniferous conglomerate as in New South Wales is not at all new. The Gay's River Gold Field of Nova Scotia, where the gold occurs in Lower Carboniferous conglomerate resting on the edges of Cambrian slates having small veins of auriferous quartz, was first pointed out by Prof. Hartt and elaborated by myself in a paper communicated to the Nova Scotian Institute of Natural Science in 1866. In Dawson's "Accadian Geology," of 1868, the same fact is referred to; also in Seluria, Prof. R. Jones received specimens of the conglomerate from me in Paris, 1867, to satisfy Sir R. J. Murchison of the fact. In the collection of ores and concrete minerals sent by H. S. Poole, Esq., Government Inspector of Mines to the Centennial Exhibition, in my charge, was a very instructive specimen of slate with a little of the conglomerate attached, having a beautiful display of gold. This was much admired. The conglomerate of Gay's River is overlaid by limestone with Lower Carboniferous fauna and gypsums. The conglomerate is worked still with good results.

D. HONEYMAN

Provincial Museum, Halifax, Nova Scotia

Japanese Mirrors

A SHORT time ago a friend showed me a curious effect, which I had previously heard of, but had never seen. The ladies of Japan use, in making their toilet, a small round mirror about $\frac{1}{12}$ to $\frac{1}{8}$ inch in thickness, made of a kind of speculum metal, brightly polished and coated with mercury. At the back there are usually various devices, Japanese or Chinese written characters, badges, &c., standing in strong relief, and brightly polished like the front surface. Now if the direct rays of the sun are allowed to fall upon the front of the mirror and are then reflected on to a screen, in a great many cases, though not in all, the figures at the back will appear to shine through the substance of the mirror as bright lines upon a moderately bright ground.

I have since tried several mirrors as sold in the shops, and in most cases the appearance described has been observed with more or less distinctness.

I have been unable to find a satisfactory explanation of this fact, but on considering the mode of manufacture I was led to suppose that the pressure to which the mirror was subjected during polishing, and which is greatest on the parts in relief, was concerned in the production of the figures. On putting this to the test by rubbing the back of the mirror with a blunt pointed instrument, and permitting the rays of the sun to be reflected from the front surface, a bright line appeared in the image corresponding to the position of the part rubbed. This experiment is quite easy to repeat, a scratch with a knife or with any other hard body is sufficient. It would seem as if the pressure upon the back during polishing caused some change in the reflecting surface corresponding to the raised parts whereby the amount of light reflected was greater; or supposing that of the light which falls upon the surface, a part is absorbed and the rest reflected, those parts corresponding to the raised portions on the back are altered by the pressure in such a way that less is absorbed, and therefore a bright image appears. This, of course, is not an explanation of the phenomenon, but I put it forward as perhaps indicating the direction in which a true explanation may be looked for.

The following account of the manufacture of the Japanese mirrors is taken from a paper by Dr. Geerts, read before the Asiatic Society of Japan, and appearing in their *Transactions* for 1875–76, p. 39 :—

"For preparing the mould, which consists of two halves, put together with their concave surfaces, the workman first powders a kind of rough plastic clay, and mixes this with levigated powder of a blackish "tuff-stone" and a little charcoal powder and water, till the paste is plastic and suitable for being moulded. It is then roughly formed by the aid of a wooden frame into square or round cakes; the surface of the latter is covered with a levigated half-liquid mixture of powdered "*chamotte*" (old crucibles which have served for melting bronze or copper) and water. Thus well prepared, the blackish paste in the frame receives the concave designs by the aid of woodcuts, cut in relief. The two halves of the mould are put together in the frame and dried. Several of these flat moulds are then placed in a melting box made of clay and "chamotte." This box has on the top an opening, into which the liquid bronze is poured, after it has been melted in small fire-proof clay crucibles. The liquid metal naturally fills all openings inside the box, and consequently also the cavities of the moulds. For mirrors of first quality the following metal mixture is used in one of the largest mirror foundries in Kiôto :—

Lead	5 parts.
Tin	15 ,,
Copper	80 ,,
	100

For mirrors of inferior quality is taken—

Lead	10 parts.
Natural sulphide of lead and antimony	10 ,,
Copper	80 ,,
	100

* * * * *

"After being cooled the melting-box and moulds are crushed and the mirrors taken away. These are then cut, scoured, and filed until the mirror is roughly finished. They are then first polished with a polishing powder called *to-no-ki*, which consists of the levigated powder of a soft kind of whetstone (*to-ishi*) found in Yamato and many other places. Secondly, the mirrors are polished with a piece of charcoal and water, the charcoal of the wood, *ho-no-ki* (*Magnolia hypoleuca*) being preferred as the best for this purpose. When the surface of the mirror is well polished it is covered with a layer of mercury amalgam, consisting of quicksilver, tin, and a little lead. The amalgam is rubbed vigorously with a piece of soft leather, which manipulation must be continued for a long time until the excess of mercury is expelled and the mirror has got a fine, bright, reflecting surface."

R. W. ATKINSON

University of Tokio, Japan

THE DECENNIAL PERIOD OF MAGNETIC VARIATIONS, AND OF SUN-SPOT FREQUENCY

A CENTURY and half ago Graham discovered that the north end of a magnetic needle moved from morning till afternoon towards the west, returning thereafter to its most easterly position in the morning again. Van Swinden, who, half a century later, studied this phenomenon during several years, occupied himself greatly with the deviations from the diurnal law. One of these, the occurrence of the greatest westerly position before noon or after 4 P.M., he found to happen most frequently in 1776, the number of times increasing from 1772, and diminishing from the year of maximum till 1780. He then asked the question whether there was not a period of *eight years*. Van Swinden's results were greatly affected by imperfections of his instrument, and we can only consider that the excess of irregular days in 1776 was probably chiefly due to real causes.

Though several series of magnetic observations were made during the eighteenth century, and two series early in this (those of Beaufoy and Arago), yet, as far as I can discover, Kaemtz seems (in 1836) to have been the first to remark that the mean value of the diurnal oscillation of the magnetic needle was not constant, but varied from year to year: this conclusion he founded on Cassini's observations, which gave the mean oscillation 9′·71 in 1784, and 15′·10 in 1787. The illustrious Gauss drew more distinct attention to the fact, for, in studying the observations made at Göttingen in the years 1834 to 1837, he pointed out that the mean diurnal oscillation for each month in the second year was greater than that for the corresponding month of the first year; and that a similar increase was to be found in the third year compared with the second. This increase Gauss did not think could go on long, and he predicted that by continuing the observations for several years, an oscillation in the mean value would present itself. It is not a little curious that in discussing the Göttingen observations for the next three years, Dr. Goldschmidt should have failed to remark that the maximum was attained in 1837, and that thereafter the mean diurnal oscillation was diminishing. This was reserved for Dr. Lamont, the distinguished astronomer of Munich, who, in the end of 1845, by adding the mean oscillations obtained from his own observations in 1842-1845, to those already found for the preceding years at Göttingen, was able to state that the minimum was then attained, but that a longer series of observations was required, in order to determine the law of the oscillation.

It was only in the end of 1851, when the maximum oscillation (which occurred in 1848-49) was decidedly past, and the mean oscillation had again begun to diminish in value, that Dr. Lamont published his conclusion that the diurnal oscillation of magnetic declination (as well as of magnetic force) obeyed a law whose mean duration was nearly 10$\frac{1}{3}$ years. For the determination of this mean he employed the epoch of maximum oscillations shown by Cassini's observations in 1787 (already noticed by Kaemtz), and he assumed that there were six periods from that date till 1849.

Schwabe had previously, from his persevering observations of the number of spots on the sun's surface, arrived at the conclusion that these obeyed a decennial law, so that the number was a maximum in 1828, 1837, and 1848, while it was a minimum in 1833 and 1843. The agreement of the epochs, 1843 and 1848, with those of minimum and maximum magnetic disturbance deduced by Sir E. Sabine from the observations made in the colonial observatories, was at once remarked by him, as well as that of Lamont's epochs with those of Schwabe.

This coincidence was also immediately afterwards, and quite independently, brought to public notice by Dr. Wolf, of Bern (now of Zurich), and M. Gautier, of Geneva. It is, however, with the important labours of the former of these philosophers that we are most concerned. Dr. Wolf began at once a systematic search for observations of sun-spots, and examined hundreds of volumes printed and in manuscript, dating from the first discovery of the existence of spots on the sun's surface. All the observations thus accumulated he has endeavoured to connect and to reduce to a common unit; and from the numbers thus obtained he has concluded that the sun-spot period, as well as that of the magnetic variations, occupies on the average 11$\frac{1}{9}$ years.

One great cause of the difference between the results of the Munich and Zurich astronomers is to be found in the interval 1787 to 1818. According to the former, three periods *ought* to have occurred in this interval; according to the latter, only one maximum happened, *in fact*, between the two of 1787 and 1818. Dr. Wolf has concluded, from the magnetic observations of Gilpin (1786–1806), that a minimum of the diurnal oscillation of the magnetic needle occurred in 1796, and a maximum in 1803, and these epochs he has supported by the observations of the numbers of sun-spots, as well as of those of the aurora borealis, a phenomenon known to be associated with magnetic disturbance, and to have the same epochs of frequency. On the other hand, Dr. Lamont has maintained that Gilpin's observations are without value, as his needle was supported on a steel pivot, and sometimes did not move freely; he has also objected to the observations of sun-spot frequency made during the time in question, that they were made rarely, without any common system, and by few observers, some having at times seen no spots when others saw many.

If we could assume with the astronomer of Munich that Gilpin's observations and those of sun-spot and auroral frequency made at the same time are worthless, all our knowledge of the epochs of magnetic oscillations since 1818, and of sun-spot frequency since 1826, would induce us to conclude that there were really three periods during the thirty-one years 1787–1818. If, however, any value can be given to the observations during that interval, it is not allowable to assume that the durations of the periods have always been the same, the more especially that we know the period has varied in length from eight to twelve years within the last half century. That some value is due to observations of three different phenomena has been allowed by most writers, and Dr. Wolf's period of 11$\frac{1}{9}$ years has, in consequence, been accepted by many of the most eminent men of science who have had occasion to allude to the subject.

Having had to study this question in connection with the results of observations made during twenty-three years at Trevandrum, I have examined with care the magnetic observations of the last and the present century, determined the exact times for which the yearly mean diurnal oscillation of the magnetic needle was a maximum or minimum, and have arrived at the following conclusions:—[1]

1st. That there are not sufficient grounds for rejecting the observations of Gilpin, which appear to be in general trustworthy as regards the change of mean position of the needle from year to year, and of the diurnal range from winter to summer.

2nd. That these observations should, according to the mean law, show a maximum near 1797, and another should have occurred near 1807. I have found that they do indicate a maximum in the former year; and though another maximum appears in 1803, that there are grounds for believing the maximum may really have occurred after 1806, when Gilpin's series terminated.

It has to be stated, however, that the maximum shown by Gilpin's observations in 1797 is very small; that the whole interval between the preceding and following minimum is not six years; and that no such short period and small maximum have been observed during the last half century. Since, however, the shortness of the period and the smallness of the maximum are both confirmed by the observations known to us of the frequency of sun-spots and of the aurora borealis, I can only conclude, in conformity with the facts, that both these were real phenomena, which may yet be repeated and aid in the determination of the cause of the decennial period. The mean duration of the period at which I arrive is therefore almost exactly that which Dr. Lamont had previously obtained, or 10·45 years.

For this result the facts have been taken as they present themselves; since it would be difficult to conclude that the observers of all the three phenomena could have erred in the same way during nearly twenty years. In addition to this, after a careful study of Dr. Wolf's sun-spot numbers, I find it impossible to accept his period of 11$\frac{1}{9}$ years. How ill the facts satisfy this result may be shown by two comparisons in which the epochs accepted by the Zurich astronomer are employed.

Thus a maximum of the magnetic oscillation occurred in 1787 by the observations of Cassini and Gilpin; this epoch has been confirmed nearly by Dr. Wolf's sun-spot numbers, and by Prof. Loomis for the auroral frequency. We have then the last observed maximum 1870·9, about which there can be no doubt. In the interval between these two maxima there were, according to Dr. Wolf, only seven periods, consequently we have—

$$\frac{1870{\cdot}9 - 1787{\cdot}3}{7} = \frac{83{\cdot}6}{7} = 11{\cdot}94 \text{ years,}$$

a period which differs as much from his mean period as that does from Dr. Lamont's. If on the other hand we take one of Dr. Wolf's sun-spot epochs about eighty years before 1787, and employ the number of periods he has himself given for the interval, we find—

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[1] See "On the Decennial Period," &c., *Trans. Roy. Soc. Edin.*, xxvii., pp. 563-594.

$$\frac{1787{\cdot}3-1705{\cdot}5}{8} = \frac{81{\cdot}8}{8} = 10{\cdot}23 \text{ years.}$$

If, then, we commence with the epoch of 1787 and compare it with any epoch of maximum since, we shall always find for the mean duration at the least 11·9 years according to Dr. Wolf; and if we compare it with any of the epochs given by him upwards of eighty years before, we shall never find a greater mean than 10·75 years, and this result includes an interval of 172 years before 1787, with all the uncertainty of the earlier epochs. This great difference of more than one year in the mean duration, as derived from eighty-four years after 1787, and eighty-two to 172 years before, disappears to a great extent if we admit three periods between 1787 and 1818.

It has been already remarked that the duration of a period is not constant, but varies within certain limits. The question naturally presents itself—Does this variation follow any law, or is it accidental, increasing one year and diminishing the next? The number of periods for which we have the epochs of maxima and minima of the diurnal oscillation of the magnetic needle accurately determined, is not sufficient for any very sure reply. At the same time the results I have obtained indicate a period of nearly forty-two years for the repetition of the variations in question; and if this conclusion is confirmed by next maximum, that should occur in the year 1879. It may also be pointed out that according to the law of forty-two years a maximum should have occurred in 1818 − 42 = 1776. Now this year, according to Dr. Wolf, was a year of minimum. The variation of his sun-spot numbers for that period, it appears to me, is not sufficient to give his conclusion much weight; while, on the other hand, Van Swinden's result, which it is extremely probable was a consequence of the decennial law, gives 1776 for the year of maximum; and that it was so is further supported by the magnetic observations of Cotte, at Montmorency. The exceptional period about 1797 shows, however, that any definite conclusion from observations during the last sixty years may be impossible, since causes of variation exist with which we are insufficiently acquainted as yet.

When we compare the mean range of the diurnal oscillation of the needle for the year in which it is a maximum with that for the year of minimum at any station, we find that the ratio of the two is very nearly constant for places so widely separated as Toronto, Dublin, Trevandrum, and Hobarton. I have also found that the *law* of the diurnal movement is the same in the year for which the range is least, and in that for which it is greatest. This shows that it is the same cause which is acting, the variation being one of intensity only. Since few or no sun-spots are visible in the years of minimum range, we perceive that the sun-spots happen only when the intensity of the force producing the magnetic variations exceeds a given value. It also appears that considerable variations in the amount of magnetic disturbance may exist near the equator when there are few or no sun-spots; and, on the other hand, that the spotted surface of the sun may be a maximum, and no corresponding increase of the magnetic oscillations be visible. The latter are, however, exceptional cases, since increases of sun-spots and of magnetic movements occur frequently near the same time; the increase of the one, however, bears no constant proportion to that of the other.

It has been already stated that the ratio of the diurnal oscillation of the needle in the year of maximum to that in the year of minimum is very nearly constant for places very widely separated from each other; there are, however, slight variations in the ratio shown at some places; thus, although it is nearly the same at Toronto, Dublin, Trevandrum, and Hobarton (1·55), it is slightly greater for Munich and Lisbon (1·71). This is probably due to the action of disturbances which are known to obey local laws. I have also found for Trevandrum, nearly on the magnetic equator, that the disturbances, or the deviations of the magnetic needle from the mean position, do not show exactly the same epochs of maximum and minimum in the decennial period when different hours are considered. Thus, though the cause is cosmic, the actions appear to be influenced, though but slightly, by circumstances of locality.

When we seek for the cause of the decennial period, we are met at first by the three phenomena which obey this law: the magnetic variations, the sun-spots, and the aurora borealis. The connection between the first and third is so marked, that if a magnetic disturbance commences during the day in a high latitude, it is quite certain that the aurora will be seen as soon as the disappearance of sunlight permits. This is a fact I have verified during several years' observations in the south of Scotland. Both these phenomena are results of electrical motions. It did not seem improbable then that the solar spots might be connected with disturbances of electrical equilibrium, and that these might be due to the different electrical states of the sun and of the planets.

We do not know, however, of any planet with a period of ten and a half years, nor of any combination of planetary positions which would produce such a period. My own researches have failed in connecting the variations of the sun's spotted surface with the time of revolution of any planet by a law which holds for different decennial periods. This fact, however, does not disprove a planetary action. We are unacquainted with the nature of the medium through which the electrical actions producing the magnetic variations are conveyed. Physicists seek to reduce the phenomena of nature to the fewest possible factors: many then have been induced to believe that electrical and magnetical actions are conveyed by the same ethereal medium which we believe transmits heat and light. The facts do not appear to be easily explained by such a hypothesis; thus I have found that certain electrical actions of the sun producing marked diminutions of the earth's magnetic force happen exactly at successive intervals of twenty-six days; when one point or meridian of the sun returns to the same position relatively to the earth; this action, similar to that of a beam of light reflected from a revolving mirror, which illuminates a particular point only at the same part of its revolution, has no resemblance to that of light and heat, which are propagated equally in all directions.

If, then, we can suppose that the electrical medium is disposed unsymmetrically around the sun, that the disposition and extension varies, it is obvious that the supposed planetary actions would also vary, and might be quite different for different parts of their orbits, in different decennial periods. This suggestion may explain why I have not been able to find a law remaining the same in the different periods; and it is not opposed to the conclusions of Messrs. De la Rue, Stewart, and Loewy, who have found very remarkable relations between certain positions of the planets and the amount of the sun's spotted surface during a single decennial period

Any hypothesis which seeks to explain the mode of production of the sun-spots (by cyclones or otherwise) must also explain why the causes become insufficient for their production every ten and a half years. M. Faye, the distinguished French astronomer, considers that the prime cause of sun-spots is to be found in the excess of heat radiated; so that the spots are the symptoms of a dying sun; that we have in fact here a phenomenon like the flickering of an expiring lamp which may have a periodical character. Such a hypothesis will scarcely satisfy the demands of science, but we must evidently wait for more facts before any satisfactory theory can be proposed.

JOHN ALLAN BROUN

HOW TO DRAW A STRAIGHT LINE[1]

THE great geometrician Euclid, before demonstrating to us the various propositions contained in his "Elements of Geometry," requires that we should be able to effect certain processes. These *Postulates*, as the requirements are termed, may roughly be said to demand that we should be able to describe straight lines and circles. And so great is the veneration that is paid to this master-geometrician, that there are many who would refuse the designation of "geometrical" to a demonstration which requires any other construction than can be effected by straight lines and circles. Hence many problems—such as, for example, the trisection of an angle—which can readily be effected by employing other simple means, are said to have no geometrical solution, since they cannot be solved by straight lines and circles only.

It becomes then interesting to inquire how we can effect these preliminary requirements, how we can describe these circles and these straight lines, with as much accuracy as the physical circumstances of the problems will admit of.

As regards the circle we encounter no difficulty. Taking Euclid's definition, and assuming, as of course we must, that our surface on which we wish to describe the circle is a plane, we see that we have only to make our tracing-point preserve a distance from the given centre of the circle constant and equal to the required radius. This can readily be effected by taking a flat piece of any form, such as the piece of carboard I have here, and passing a pivot which is fixed to the given surface at the given centre through a hole in the piece, and a tracer or pencil through another hole in it whose distance from the first is equal to the given radius; we shall then, by moving the pencil, be able, even with this rude apparatus, to describe a circle with considerable accuracy and ease; and when we come to employ very small holes and pivots, or even larger ones turned with all that marvellous truth which the lathe affords, we shall get a result unequalled perhaps among mechanical apparatus for the smoothness and accuracy of its movement. The apparatus I have just described is of course nothing but a simple form of a pair of compasses, and it is usual to say that the third Postulate postulates the compasses.

But the straight line, how are we going to describe that? Euclid defines it as "lying evenly between its extreme points." This does not help us much. Our text-books say that the first and second Postulates postulate a ruler. But surely that is begging the question. If we are to draw a straight line with a ruler, the ruler must itself have a straight edge; and how are we going to make the edge straight? We come back to our starting-point.

Now I wish you clearly to understand the difference between the method I just now employed for describing a circle, and the ruler method of describing a straight line. If I applied the ruler method to the description of a circle I should take a circular lamina, such as a penny, and trace my circle by passing the pencil round the edge, and I should have the same difficulty that I had with the straight-edge, for I should first have to make the lamina itself circular. But the other method I employed involves no begging the question. I do not first assume that I have a circle and then use it to trace one, but simply require that the distance between two points shall be invariable. I am of course aware that we do employ circles in our simple compass, the pivot and the hole in the moving piece which it fits are such; but they are used not because they are the curves we want to describe (they are not so, but are of a different size), as is the case with the straight-edge, but because, through the impossibility of constructing pivots or holes of no finite dimensions, we are forced to adopt the best substitute we can for making one point in the moving piece remain at the same spot. If we employ a very small pivot and hole, though they be not truly circular, the error in the description of a circle of moderate dimensions will be practically infinitesimal, not perhaps varying beyond the width of the thinnest line which the tracer can be made to describe; and even when we employ large pivots and holes we shall get results as accurate, because those pivots and holes may be made by the employment of very small ones in the machine which makes them.

It appears, then, that although we have an easy and accurate method of describing a circle, we have at first sight no corresponding means of describing a straight line; and there would seem to be a substantial difficulty in producing what mathematicians call the simplest curve, so that the question how to get over that difficulty becomes one of a decided theoretical interest.

Nor is the interest theoretical only, for the question is one of direct importance to the practical mechanician. In a large number of machines and scientific apparatus it is requisite that some point or points should move accurately in a straight line with as little friction as possible. If the ruler principle is adopted, and the point is kept in its path by guides, we have, besides the initial difficulty of making the guides truly straight, the wear and tear produced by the friction of the sliding surfaces, and the deformation produced by changes of temperature and varying strains. It becomes therefore of real consequence to obtain, if possible, some method which shall not involve these objectionable features, but possess the accuracy and ease of movement which characterises our circle-producing apparatus.

Turning to that apparatus we notice that all that is requisite to draw with accuracy a circle of any given radius is to have the distance between the pivot and the tracer properly determined, and if I pivot a second "piece" to the fixed surface at a second point having a tracer as the first piece has, by properly determining the distance between the second tracer and pivot I can describe a second circle whose radius bears any proportion I please to that of the first circle. Now, removing the tracers, let me pivot a third piece to these two *radial* pieces, as I may call them, at the points where the tracers were, and let me fix a tracer at any point on this third or *traversing* piece. You will at once see that if the radial pieces were big enough the tracer would describe circles or portions of circles on *them*, though they are in motion, with the same ease and accuracy as in the case of the simple circle drawing apparatus; the tracer will not however describe a circle on the *fixed* surface but a complicated curve.

This curve will, however, be described with all the ease and accuracy of movement with which the circles were described, and if I wish to reproduce in a second apparatus the curves which I produce with this, I have only to get the distances between the pivots and tracers accurately the same in both cases, and the curves will also be accurately the same. I could of course go on adding fresh pieces *ad libitum*, and I should get points on the structure produced, describing in general very complicated curves, but with the same results as to accuracy and smoothness, *the reproduction of any particular curve depending solely on the correct determination of a certain definite number of distances.*

These systems, built up of pieces pointed or pivoted together, and turning about pivots attached to a fixed base, so that the various points on the pieces all describe definite curves, I shall term "link-motions," the pieces being termed "links." As, however, it sometimes facilitates the consideration of the properties of these structures to regard them apart from the base to which they are pivoted, the word "linkage" is employed to denote any combination of pieces pivoted together. When such a combination is pivoted in any way to a fixed base, the motion of points on it not being necessarily confined to fixed paths, the link structure is called a "linkwork:" a "linkwork" in which the motion of every point is in some definite path being, as before stated, termed a "link-motion." I shall only add to these expressions two more: the point of a link-motion which describes any curve is called a "graph," the curve being called a "gram."

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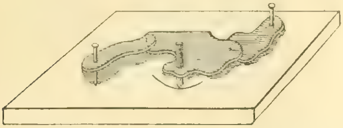
[1] Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A.

Fig. 1.



The consideration of the various properties of these "linkages" has occupied much attention of late years among mathematicians, and is a subject of much complexity and difficulty. With the purely mathematical side of the question I do not, however, propose to deal to-day, as we shall have quite enough to do if we confine our attention to the practical results which mathematicians have obtained, and which I believe only mathematicians could have obtained. That these results are valuable cannot, I think, be doubted, though it may well be that their great beauty has led some to attribute to them an importance which they do not really possess; and it may be that fifty years ago they would have had a value which, through the great improvements that modern mechanicians have effected in the production of true planes, rulers and other exact mechanical structures, cannot now be ascribed to them. But linkages have not at present, I think, been sufficiently put before the mechanician to enable us to say what value should really be set upon them.

The practical results obtained by the use of linkages are but few in number, and are closely connected with the problem of "straight-line motion," having in fact been discovered during the investigation of that problem, and I shall be naturally led to consider them if I make "straight-line motion" the backbone of my lecture. Before, however, plunging into the midst of these linkages it will be useful to know how we can practically construct such models as we require; and here is one of the great advantages of our subject—we can get our results visibly before us so very easily. Pins for fixed pivots, cards for links, string or cotton for the other pivots, and a dining-room table, or a drawing board if the former be thought objectionable, for a fixed base, are all we require. If something more artistic be preferred, the plan adopted in the models exhibited by me in the Loan Collection can be employed. The models were constructed by my brother, Mr. H. R. Kempe, in the following way. The bases are thin deal boards painted black; the links are neatly shaped out of thick cardboard (it is hard work making them, you have to sharpen your knife about every ten minutes, as the cardboard turns the edge very rapidly); the pivots are little rivets made of catgut, the heads being formed by pressing the face of a heated steel chisel on the ends of the gut after it is passed through the holes in the links; this gives a very firm and smoothly working joint. More durable links may be made of tin-plate; the pivot-holes must in this case be punched, and the eyelets used by bootmakers for laced boots employed as pivots; you can get the proper tools at a trifling expense at any large tool shop.

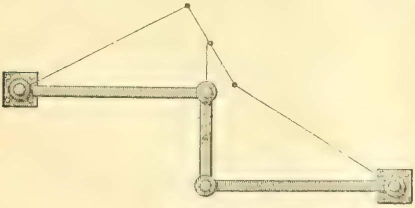


Fig. 2.

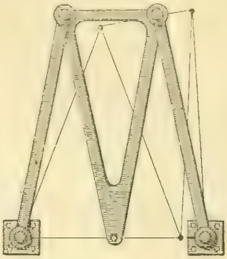


Fig. 3.

Now, as I have said, the curves described by the various points on these link-motions are in general very complex. But they are not necessarily so. By properly choosing the distances at our disposal we can make them very simple. But can we go to the fullest extent of simplicity and get a point on one of them moving accurately in a straight line? That is what we are going to investigate.

To solve the problem with our single link is clearly impossible: all the points on it describe circles. We must therefore go to the next simple case—our three-link motion. In this case you will see that we have at our disposal the distance between the fixed pivots, the distances between the pivots on the radial links, the distance between the pivots on the traversing link, and the distances of the tracer from those pivots; in all six different distances. Can we choose those distances so that our tracing-point shall move in a straight line?

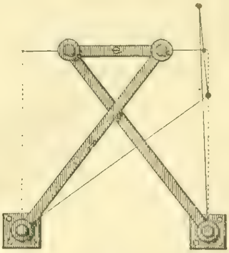


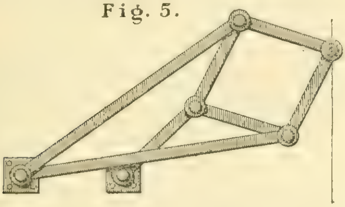
Fig. 4.

The first person who investigated this was that great man James Watt. "Watt's Parallel Motion," invented in 1784, is well known to every engineer, and is employed in nearly every beam-engine. The apparatus reduced to its simplest form is shown in Fig. 2.

The radial bars are of equal length—I employ the word "length" for brevity, to denote the distance between the pivots, the links, of course, may be of any length or shape,—and the distance between the pivots or the traversing link is such that when the radial bars are parallel the line joining those pivots is perpendicular to the radial bars. The tracing-point is situate half-way between the pivots on the traversing piece. The curve described by the tracer is, if the apparatus does not deviate much from its mean position, approximately a straight line. The reason of this is that the circles described by the extremities of the radial bars have their concavities turned in opposite directions, and the tracer being half-way between, describes a curve which is concave neither one way nor the other, and is therefore a straight line. The curve is not, however, accurately straight, for if I allow the tracer to describe the whole path it is capable of describing, it will, when it gets some distance from its mean position, deviate considerably from the straight line, and will be found to describe a figure 8, the portions at the crossing being nearly straight. We know that they are not quite straight, because it is impossible to have such a curve partly straight and partly curved.

For many purposes the straight line described by Watt's apparatus is sufficiently accurate, but if we require an exact one it will, of course, not do, and we must try again. Now it is capable of proof that it is impossible to solve the problem with three moving links; closer approximations to the truth than that given by Watt can be obtained, but still not actual truth.

Fig. 5.



I have here some examples of these closer approximations. The first of these, shown in Fig. 3, is due to Richard Roberts of Manchester.

The radial bars are of equal length, the distance between the fixed pivots is twice that of the pivots on the traversing piece, and the tracer is situate on the traversing piece, at a distance from the pivots on it equal to the lengths of the radial bars. The tracer in consequence coincides with the straight line joining the fixed pivots at those pivots and half-way between them. It does not, however, coincide at any other point, but deviates very slightly between the fixed pivots. The path described by the tracer when it passes the pivots, altogether deviates from the straight line.

The other apparatus was invented by Prof. Tchebicheff of St. Petersburg. It is shown in Fig. 4. The radial bars are equal in length, being each in my little model five inches long. The distance between the fixed pivots must then be four inches, and the distance between the pivots or the traversing bar two inches. The tracer is taken half-way between these last. If now we draw a straight line—I had forgotten that we cannot do that yet, well, if we draw a straight line, popularly so called—through the tracer in its mean position as shown in the figure, parallel to that forming the fixed pivots, it will be found that the tracer will coincide with that line at the points where verticals through the fixed pivots cut it as well as at the mean position, but, as in the case of Roberts's parallel motion, it coincides nowhere else, though its deviation is very small as long as it remains between the verticals.

We have failed then with three links, and we must go on to the next case, a five-link motion—for you will observe that we must have an odd number of links if we want an apparatus describing definite curves. Can we solve the problem with five? Well, we can, but this was not the first accurate parallel motion discovered, and we must give the first inventor his due (although he did not find the simplest way), and proceed in strict chronological order.

In 1864, eighty years after Watt's discovery, the problem was first solved by M. Peaucellier, an officer of Engineers in the French army. His discovery was not at first estimated at its true value, fell almost into oblivion, and was rediscovered by a Russian student named Lipkin, who got a substantial reward from the Russian Government for his supposed originality. However, M. Peaucellier's merit has at last been recognised, and he has been awarded the great mechanical prize of the Institute of France, the "Prix Montyon."

M. Peaucellier's apparatus is shown in Fig. 5. It has, as you see, seven pieces or links. There are first of all two long links of equal length. These are both pivoted at the same fixed point; their other extremities are pivoted to opposite angles of a rhombus composed of four equal shorter links. The portion of the apparatus I have thus far described, considered apart from the fixed base, is a linkage termed a "Peaucellier cell." We then take an *extra* link, and pivot it to a fixed point whose distance from the first fixed point, that to which the cell is pivoted, is the same as the length of the extra link; the other end of the extra link is then pivoted to one of the free angles of the rhombus; the other free angle of the rhombus has a pencil at its pivot. That pencil will accurately describe a straight line.

I must now indulge in a little simple geometry. It is absolutely necessary that I should do so in order that you may understand the principle of our apparatus.

(*To be continued.*)

FOSSIL FLORAS AND GLACIAL PERIODS

A RECENT notice in NATURE (vol. xiv. p. 336) of certain inferences of Prof. Heer in connection with the Arctic fossil plants obtained by the Swedish Expeditions of 1870 and 1872, suggests some thoughts on the relations of fossil plants to climate, which, though I have discussed them elsewhere, deserve to have attention again directed to them. In my Bakerian Lecture before the Royal Society in 1870, and in my "Report on the Pre carboniferous Flora of Canada," published by the Canadian Survey in 1871, I deduced from the generalisations of Prof. James Hall as to the growth of the American Continent from the north-east, in connection with the distribution of the fossil plants of the Upper Silurian, Erian, and Carboniferous systems, the conclusion that these assemblages of plants entered North America from the north-east, and propagated themselves southward and westward. Prof. Asa Gray had, as early as 1867, stated similar conclusions with reference to the modern floras of America and Eastern Asia, and has more recently extended them to the Tertiary floras on the evidence of Heer and Lesquereux.[1]

The further conclusion that all the old floras appeared suddenly and abruptly in the temperate regions, and with a great number of species, I have illustrated in the Report above referred to, as far as regards the Palæozoic plants, and have referred to the evidence of it in the case of the Cretaceous and Tertiary floras in my address to the American Association in 1875.

With regard to the succession of these floras, it is true that it has been the fashion with certain European palæontologists to regard our rich Devonian or Erian flora of America as of Carboniferous age, simply because it contains forms not found as yet in rocks so old in Europe. But this notion is at variance with stratigraphy and animal fossils, and quite as wide of the mark as the often-repeated dictum of some of the same authorities that the Cretaceous flora of Vancouver Island and the Eocene flora of the North-western plains are equivalents of the European Miocene. In point of fact, we have in America distinct floras of Erian and Carboniferous age with an intermediate sub-flora of Lower Carboniferous date,[1] and succeeding them the Triassic and Jurassic flora, that of the Cretaceous, that of the Eocene, and that of the Miocene; and there is good reason to believe that all of these invaded the Continent from the northward and lingered longest in the south. There may no doubt have been counter migrations from the south, but these seem to have left less trace in the geological record. The special Lower Carboniferous or "Culm" flora, and that of the Eocene in Europe, may be of this character.

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[1] Address to the American Association, 1872.

If we compare these facts with those known from other sources as to the alternation of cold and warm climates in the northern hemisphere, it would seem that they harmonise most closely on the hypothesis advocated by Sir Charles Lyell, that these changes of climate have depended mainly on the distribution of land and water and of the ocean currents.

Assuming a condition in which much tropical land existed, along with islands in the Arctic and sub-Arctic regions, surrounded with deep water over which warm currents were distributed, a rich flora might extend as far northward as the supply of light would permit. Further, if such condition of equatorial protuberance were coincident with a less obliquity of the ecliptic, there might be less difficulty with regard to a continuous supply of light than under present circumstances. Succeeding elevation of northern and temperate land and depression of that nearer the equator, would destroy the more southern flora and cause that of the north to advance over the newly-elevated continental plateaus. This would more especially be the case if, as we may infer from the possible connection of equatorial subsidence with the retardation of the earth's rotation, the depression of the northern land was very slow and gradual, and that of the equatorial land more sudden and paroxysmal.

Invasions of plants from the north would thus result from continental elevation in the temperate regions, and these invasions would go on contemporaneously with the introduction of less equable and cooler climatal conditions. These might not, however, advance so far as to cause extreme glacial phenomena, except in those, perhaps rare, circumstances, when Arctic land was elevated while the greater part of the tropical and temperate areas remained under shallow seas with little heating and radiating surface and invaded by much northern ice. Further, when we take into consideration the growth of the continents in later geological times, it is evident that such periods of exceptional cold would be more likely to occur in these later times, and that they might be less intense in earlier geological periods, and might attain their maximum in the last glacial period. They would also be irregular as to the intervals between them, and might through long periods be absent altogether. We have proof of the efficacy of such causes in the contrast between the climates of Labrador and England at the present day, and also in that sameness of the climate of those regions in the Post-pliocene period, of which I long ago pointed out the evidence in my "Acadian Geology." Such moderate views as to glacial periods may also serve to render more explicable the facts as to the absence of evidence of glacial action in Arctic Tertiary formations as observed by Nordenskjöld.

It will of course be understood that my conception of glacial periods is not that of continental ice-caps; but rather such conditions as would cover great breadths of shallow sea in the northern hemisphere with a permanent and continuous ice-pack, accompanied of course with "bordage" and "anchor ice" and with glaciers descending to the sea from high lands; the whole resembling that now occupying large areas of the Arctic seas, and occurring in winter in the Gulf and River St. Lawrence. To such agencies I have been accustomed for the last twenty years to refer our Canadian boulder clay and glaciated rocks. Further, to this extent we have evidence, locally at least, of ice-action in temperate latitudes (in non-fossiliferous conglomerates with boulders) as far back as the Huronian age, while the evidence of alternate submergence and emergence of the northern land extends down to that of the Post-pliocene, whose greatness geologists are only beginning to realise.

It is a corollary from these views that there can have been no change within geological time in the position of the earth's axis of rotation. The distribution of sediment by the polar currents, and the lines of plication and upheaval of the crust, as well as the distribution of successive floras, prove that the poles have remained since the Laurentian period where they now are. I need here merely refer to the fact, well known to all American geologists, that the earthy matter of the thick Appalachian sediments lies parallel to the line of the modern Arctic currents, which seem in all geological time to have been potent agents in carrying the *débris* of the disintegrated Arctic rocks to the south, and filling up the voids caused by equatorial subsidence. Further, the great organic limestones, which represent the contemporaneous food-bearing warm currents from the equator, lie on the plateaus and in the bays of the old Eozoic and Palæozoic land.

We need not, however, in consistency with such views, refuse to attach any importance which they may seem to require to astronomical cycles affecting the eccentricity of the earth's orbit and the precession of the equinoxes, or to the possible diminution or inequality of solar energy, or to the secular cooling and contraction of the earth or the retardation of its rotation. But geologists and palæontologists, in speculating on past conditions of the earth, should endeavour in the first instance to gauge the value of the causes indicated by their own sciences; and where climate is in question no evidence can be more important than that of continental elevation and depression, in connection with the appearance and diffusion of those assemblages of land plants which furnish so sure testimony as to climatal influences.

I should perhaps apologise for throwing out these suggestions with so little of illustration or proof. For much of this I may refer to my published memoirs;[1] and I have now before me a mass of additional evidence, collected in all the great regions from Newfoundland to British Columbia by several recent observers, which I have not at the moment time or opportunity to throw into a connected form. My present object is to invite the attention of the many young and active geologists now working at these subjects to lines of investigation from which they may be deterred by some of the theoretical views now current.

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A NEW STIMULANT—PITURY

BARON VON MUELLER writes to the *Australian Medical Journal* on the origin of the Pitury, a stimulant said to be of marvellous power, and known to be in use by the Aborigines of Central Australia. After years of efforts to get a specimen of the plant, he had obtained leaves, but neither flowers nor fruits. He can almost with certainty, after due microscopic examination, pronounce those of the Pitury as derived from his *Duboisia Hopwoodii*, described in 1861 (*Fragm. Phytogr. Austr.* II., 138). This bush extends from the Darling River and Barcoo to West Australia, through desert scrubs, but is of exceedingly sparse occurrence anywhere. In fixing the origin of the Pitury, a wide field for further inquiry is opened up, inasmuch as a second species of *Duboisia* (*D. myoporoides*, *R. Br.*) extends in forest land from near Sydney to near Cape York, and is traced also to New Caledonia, and lately by him also to New Guinea. In all probability this *D. myoporoides* shares the properties of *D. Hopwoodii*, as he finds that both have the same burning acrid taste. Baron Mueller adds: "Though the first known species is so near to us, we never suspected any such extraordinary properties in it as are now established for the later discovered species. Moreover, the numerous species of the allied genus *Anthocercis*, extending over the greater part of the Australian continent and to Tasmania, should now also be tasted, and further the many likewise cognate *Schwenkeas* of South America, should be drawn into the same cyclus of research, nothing whatever of the properties of any of these plants being known. The natives of Central Australia chew the leaves of *Duboisia Hopwoodii*, just as the Peruvians and Chilians masticate the leaves of the Coca (*Erythroxylon Coca*), to invigorate themselves during their long foot journeys through the deserts. I am not certain whether the Aborigines of all districts in which the Pitury grows are really aware of its stimulating power. Those living near the Barcoo travel many days' journeys to obtain this, to them, precious foliage, which is carried always about by them broken into small fragments and tied up in little bags. It is not improbable that a new and perhaps important medicinal plant is thus gained. The blacks use the *Duboisia* to excite their courage in warfare, a large dose infuriates them."

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[1] Tweedian of North of England, Culm of Germany.

[1] Especially the Report above referred to, and "Notes on the Post-pliocene Geology of Canada," *Canadian Naturalist*, New Series, vol. vi.

THE ANTIQUITY OF MAN

ON Tuesday evening last a conference was held at the rooms of the Anthropological Institute on the Present State of the Question of the Antiquity of Man. The chair was taken by the president, Mr. John Evans, F.R.S. There were also present Lord Talbot de Malahide, Prof. Huxley, Prof. Prestwich, Prof. Rolleston, Prof. Busk, Prof. Boyd Dawkins, Prof. McK. Hughes, Rev. Prof. Sayce, Mr. J. Heywood, Col. Lane Fox, Mr. A. W. Marks, Capt. Douglas Galton, Rev. E. W. Edgell, and many other gentlemen.

The President in opening the conference alluded to the altered position of the question since it was first brought before the British public in 1859, and pointed out the extreme caution which was necessary in dealing with the subject as it lay within the domain of the archæologist, the anthropologist, and the geologist, neither of whom alone was sufficient by himself to offer a very strong opinion on the subject. Great care was also necessary with regard to the facts of the discoveries themselves, as the objects discovered were liable to get mixed with other objects below them, and this was important in the case of cave deposits in which there might be interments of a later date than the human skeletons deposited in the caves. The question was now very much within the province of the geologist, whose business it was to determine the antiquity of the deposits in which the discoveries may have been made. After alluding to several recent discoveries in France, Spain, and Switzerland, the President remarked that each successive discovery or presumed discovery must be received in a cautious but candid spirit, and looking to the many sources of doubt and error which attached to isolated discoveries, their watchword must for the present be "caution, caution, caution."

The debate was opened by Prof. Boyd Dawkins by an inquiry into the value of the evidence offered by the bone-caves of Great Britain. The antiquity of man is not to be measured by the system of chronology used by the historian, but by the physical and biological changes familiar to the geologist. Beyond historical record time past cannot be estimated in terms of years, because of our ignorance of the length of the intervals, and of the time necessary to produce the changes which mark the hour on the geological dial. The caves of Cresswell Crags, recently brought before the Geological Society, were taken as types, showing the strange association of human implements and remains of animals. Bones and teeth of species now found only in the south, such as the spotted hyæna and lion, were lying side by side with those of northern habit, such as the reindeer, while some are extinct, such as the mammoth and woolly rhinoceros, and others, such as the stag, horse, and bison, still live in the temperate regions. This mixed fauna is universal in British bone-caves, and in those of France and Germany, and it cannot be accounted for by the supposition of Messrs. James Geikie and Croll that the southern animals inhabited Britain in a warm period inter-glacial, while the northern were here at another time after, with an interval between them of from 5,000 to 12,000 years; not only because they are closely associated together in the same strata, but because we have full proof that northern and southern species coexisted at the same time on the same place, in the fact that the reindeer formed an important portion of the prey of the hyæna. It may, however, be accounted for by the overlapping of faunas according to the ever-varying summer heat and winter cold over what was then a vast continent, extending from Northern Africa as far as the 100-fathom line off the coast of Scotland and Scandinavia. The palæolithic man of the caves belongs to the northern group of the pleistocene animals, and his remains are therefore of late pleistocene age. This northern group invaded Europe as the glacial cold came on, was pushed down as far south as the Mediterranean, the Alps, and Pyrenees, as the ice-sheet advanced southwards, and on its retreat passed again northwards. It therefore follows that they are both pre- and post-glacial in Britain. Some caves have been inhabited by man in post-glacial times, as, for example, that of Pont Newydd, near St. Asaph, but it does not follow that all palæolithic caves are post-glacial. The Victoria Cave offers no evidence as to the antiquity of man, because fibula found in association with the pleistocene mammalia, and supposed to be human, is most probably ursine. Further the relation of the deposit in which it was found to the glacial strata of the district is a matter of dispute.

The facts brought forward by Mr. James Geikie, that all palæolithic remains are of earlier date than post-glacial times may be interpreted otherwise. The "something like perpetual summer" which he considers necessary for the presence of the southern animals in the mixed fauna of the caves and of which there is no trace in post-glacial times, is inconsistent with the abundance of reindeer invariably associated with the palæolithic remains of the caves. The barren areas in Great Britain, in which no pleistocene species are found, may be reasonably accounted for by the fact that they were covered with ice, *while* the species were living in more glaciated regions in the south, than by the view that they were equally distributed over the whole area, and afterwards removed by ice for the glaciated regions. The glacial phenomena are no guide to age in non-glaciated districts. In fine, the evidence of the caves is decisive that these palæolithic inhabitants are of late pleistocene age, post-glacial, and possibly pre-glacial, and glacial.

Prof. Hughes, after a few remarks on some foreign cases in which man had been referred to periods more remote than was generally included under the term glacial, commenced by explaining that in using the word glacial he meant the period in which conditions prevailed in the area in question such as must have caused glacier ice, or in adjoining areas which, by supplying berg or coast ice or influencing the climate, must have affected the area in question. He then proceeded to criticise the cases adduced from the neighbourhood of Brandon and Thetford. By an appeal to sections he showed that the beds in which the flint implements had been found were remains of valley deposits resting on older deposits which he referred to the middle glacial.

He explained the various divisions of the middle glacial beds and correlated them with deposits of the same age in Hertfordshire, pointing out that there were several horizons at which loams occurred. He then showed that the beds in which the flint implements had been found rested upon various members of the middle glacial series and occurred in troughs and hollows scooped out of the middle glacial beds. In the case of the

Beeches Pit, opposite Culford, he said that the implements were found in a deposit which seemed to be the end of a terrace of valley gravel which, further down as it was followed towards Icklingham, became more clearly marked, consisting of gravel and brick-earth with pupa, pisidium, and mammalian remains.

The only deposits at all like boulder clay which either in the Beeches Pit or at Botany Bay overlaid these implement-bearing loams, he considered to be the wash either from boulder clay or directly from the chalk as the case might be.

Mr. R. H. Tiddeman then read "Some Observations on the Hyæna Bed at the Victoria Cave, and its Bearing on the Antiquity of Man." After some remarks on the disputed fibula formerly determined to be human, which had been found at a great depth in the cave deposits in the hyæna bed, the author went on to call attention to two bones, one certainly of goat, and another a rib of a small ruminant probably belonging to the same species, on both of which are cuts or hacks which appear to be the result of human workmanship. These were also found in the same bed at the depths respectively of 25 and 15 feet. In considering the age of these it was remarked that the condition of a bone is not necessarily a test of age, and in many instances might be a most fallacious guide. It was admitted that the goat has not been usually considered as introduced into Britain before Neolithic times, but it certainly had appeared in the Victoria Cave in association with the remains of hyæna, *Elephas antiquus*, *Rhinoceros leptorhinus*, and *Hippopotamus*. In the caverns in the neighbourhood of Dinant-sur-Meuse, in Belgium, M. E. Dupont records the goat as occurring in the lower beds in the same association. It seems, therefore, not improbable that it should have occurred in Britain at the same time.

If these are human workmanship, as appears probable, this cave holds only in common with other caves, the works of man so accompanied, and the actual finding of man or his works in the cave is a secondary question compared to the correlation of the beds with certain great and widespread physical changes.

The hyæna bed contains amongst others besides hyæna, the following—*Elephas antiquus*, *Rhinoceros leptorhinus*, and *Hippopotamus*. These were chosen as a well-marked fauna, about which no doubt was entertained that they were contemporary. They occur in non-gravels in France and Switzerland and in the south and east of England, and in each of these countries are associated with man's bones or handiwork. The geologists who have worked chiefly at the drifts of the south of England maintain, and rightly, that these remains are then post-glacial; but to infer that they are so in the north of England may lead to error. Their remains appear to have been removed from the open country there by glaciation, although from their existence in the Victoria Cave and another near Skipton they must at one time have been as abundant in the valleys as they are in the south. The author considered that this later glaciation was on the wane during its maximum at about the parallel of Derbyshire, and it appeared probable from authorities quoted that it had not extended over the southern end of the Pennine Chain. The glacial drifts further south and of earlier age than the animals referred to appeared to be the relics of an earlier glaciation than that of the north country, and extended further south. The author believed that the acceptance of two great and well-marked periods of glaciation differing in their extent would reconcile many of the differences which now exist amongst geologists as to the age of man and the drifts of this and other countries.

An interesting discussion followed, of which we hope to be able to give some account next week.

OUR ASTRONOMICAL COLUMN

THE REVOLVING DOUBLE STARS.—Dr. Doberck, of Col. Cooper's Observatory, Markree, has published elements of ξ Bootis, calculated from measures extending over ninety-five years, which interval appears to be about two-thirds of a complete revolution. In this second computation for the same star he has followed a suggestion made in this column (NATURE, vol. xiv. p. 475), with regard to the probable interpretation of Sir William Herschel's measures in 1792 and 1795, and his results prove the necessity for the alteration proposed.

We are now indebted to Dr. Doberck for orbits of thirteen of the revolving double stars, calculated in every case in the most complete manner possible from the available data, and which have been communicated from time to time to the Royal Irish Academy. They form collectively a very valuable contribution to this department of astronomy. Col. Cooper may be congratulated on such work emanating from his observatory, and Dr. Doberck likewise on the success which has attended his efforts. We subjoin the periods and eccentricities for Dr. Doberck's stars, omitting only ζ Aquarii, which from the great length of period is open to more uncertainty than the others :—

	Period. Years.	Eccentricity.
γ Coronæ Borealis	95·5	0·350
ξ Scorpii	95·9	0·077
ω Leonis	110·8	0·536
ξ Bootis	127·4	0·708
τ Ophiuchi	185·2	0·582
η Cassiopeæ	222·4	0·576
λ Ophiuchi	241·0	0·493
44 Bootis	261·1	0·710
μ^2 Bootis	290·1	0·617
36 Andromedæ	349·1	0·654
γ Leonis	402·6	0·739
σ Coronæ Borealis	843·2	0·750

The number of binary stars of which the orbits have been determined by various calculators with a greater or less degree of precision, now amounts to twenty-five. The shortest period of revolution hitherto detected belongs to 42 Comæ Berenices, which, according to M. Dubiago, of Pulkowa, in a communication from M. Otto Struve to the St. Petersburg Academy in May 1875, amounts to only 25·71 years. The star was single in 1845 and 1870–71; in 1829 and 1854–55 the distance of the components slightly exceeded six-tenths of a second, which is the greatest separation. The inclination of the orbit to the tangent-plane of the heavens is 90°, or so nearly so that the measures appear to be represented upon this assumption within their possible errors; thus the apparent orbit is a right line, with the direction 11° – 191°. Notwithstanding the difficulty of the case, M. Dubiago has been able to assign the other elements of the orbit with a fair degree of probability as follows :—peri-astron passage 1859·92, angle between the peri-astron and the node, 99° 11′, eccentricity 0·480, semi-axis major 0″·657. The distance of the components at the present time will therefore be 0″·50, with the smaller star on an angle of 11°.

From the elements of ξ Bootis by Dr. Doberck, to which reference is made above, the following appear to be the angles and distances, up to about the epoch of the approaching peri-astron passage, 1898·04 :—

1882·0	Pos. 271·7	Dist. 3″·81	1896·0	Pos. 191°·5	Dist. 1″·57
86·0	,, 259·7	,, 3·29	97·0	,, 174·0	,, 1·36
90·0	,, 242·8	,, 2·71	98·0	,, 150·8	,, 1·21
92·0	,, 231·1	,, 2·38	1899·0	,, 123·5	,, 1·17
94·0	,, 215·3	,, 2·00	1900·0	,, 98·2	,, 1·30

PHYSICAL OBSERVATIONS OF MARS.—Mr. Marth has communicated to the Royal Astronomical Society an elaborate paper intended to facilitate physical observations of the planet Mars during the favourable opposition of the present year, when it is much to be desired that observations tending to improve our knowledge of the planet may be undertaken by those who are provided with adequate instruments. Mr. Marth has calculated the areographical longitude and latitude of the centre of the disc for the times of about ninety sketches of Mars, by Dawes, von Franzenau, Harkness, Kaiser, Lassell, Lockyer, Rosse, and Secchi, and with the aid of a table applicable to the interval June 9—December 14, with very little trouble the observer will be enabled to refer to the particular drawing which applies the most nearly to the time of any proposed observation, and will thereby be assisted in fixing upon the details of the surface to which it may be desirable to direct his attention. The table contains the angle of position of the axis of Mars, no doubt from Bessel's elements, or rather those deduced by Oudemanns from the observations of the Königsberg astronomer, the areographical western longitude and the latitude of the centre of the disc, the apparent diameter, the amount and position of the greatest defect of illumination, and the areocentric angle between the earth and sun, all quantities for Greenwich alternate noon. Vol. xxxii. of the "Memoirs of the Royal Astronomical Society," contains the sketches of Lassell, Lockyer, and Rosse, and this volume alone would be of considerable assistance to the intending observer, as will appear from Mr. Marth's second table.

CHEMICAL NOTES

CRYSTALLISATION UNDER GALVANIC CURRENTS.—A recent number of the *Journal* of the Russian Chemical and Physical Society (vol. ix., fasc. 2) contains an interesting report, by M. Shidlovsky, on observations he has made as to the microscopical crystallisation of various metals under the influence of a galvanic current. Placing on the object-glass of the microscope two fine metallic plates, the edges of which are about a quarter millim. distant, immersing them in a drop of water and passing a current through, M. Shidlovsky watched the growth of small ramified threads of crystals of metal transported from the cathode to the anode plate. The growth of these tree-like agglomerations goes on very speedily; their branches spread out to the anode plate, vibrate on reaching it, and collapse, whilst another ramified tree grows from the cathode spreading out to the anode; this goes on until the space between the plates is filled with a spongy metallic mass. Each of the metals experimented on (lead, silver, zinc, tin, copper, and iron) gives its own characteristic ramifications, and if the two plates be of different metals the tree has ramifications characteristic of the metal of which the anode plate is made. Gold and platinum do not exhibit any appearance of crystalline trees, nor does the crystallisation appear when the anode is gold or platinum. Iron submitted to a continuous current does not show a transport of crystals, but the phenomenon appears immediately when the currents are originated by a Ruhmkorff's coil or by a Holtz's machine. Iron-powder suspended in water undergoes a rapid motion under the influence of a strong inductive current, forming threads which spread out from the cathode to the anode plate.

ISODIBUTYLENE.—The same volume contains the second part of the important paper by Prof. A. Butlérof on the polymerisation of hydrocarbures from the ethylene series:—On isodibutylene.

ON THE THERMIC FORMATION OF OZONE.—M. Berthelot has recently investigated this question by subjecting pure and dry oxygen to the influence of the silent discharge, whilst passing the gas into a flask containing 500 c.c. solution of titered arsenious acid. At the end of thirty minutes, six to nine litres of oxygen had passed through the flask, the temperature being raised one-third of a degree; then by passing the oxygen current without the action of the discharge for an equal time, the thermal data were rendered complete. The arsenious acid solution was then treated with potassium permanganate, and redetermined with a solution of oxalic acid. By this means the quantity of arsenious acid oxidised, and consequently ozone absorbed, was determined. The amount of oxygen absorbed was found to be 30·3 and 51·9 milligrams, corresponding to 90·9 and 155·7 m.m. ozone, the heat set free being 118·2 and 223 calories respectively. Hence for one molecule the heat is equal to + 68·8 calories. Subtracting from this the heat formed in the oxidation of a molecule of arsenious acid + 39·2 calories (Favre and Thomsen), we have + 29·6 calories for the heat set free in the condensation of one molecule ozone into oxygen, and consequently – 29·6 in the reverse process. Ozone therefore is a body in which heat is absorbed in its formation, its activity in combination being probably due to this heat being set free. This is worthy of note when it is remembered that it is condensed oxygen, condensation generally setting free heat.

CHLOROPHYLL IN CONIFERÆ.—Coniferæ are remarkable amongst other plants for developing their chlorophyll even in places which seem perfectly dark. In the Reports of the *Naturforschende Gesellschaft* of Leipzig, Herr R. Sachsse publishes the results of some investigations he made in order to ascertain whether the chlorophyll formed under these circumstances is quite identical with ordinary chlorophyll. He extracted the chlorophyll from young Coniferæ, which had germinated in the dark, by boiling them in alcohol. He obtained a solution which showed the ordinary chlorophyll spectrum; all bands were in the right position and showed the correct grades of intensity. When the solution was concentrated the absorption at the end of the spectrum was continuous, when more diluted the absorption was resolved into the well-known three bands. The only peculiarity in this spectrum, when compared with that of chlorophyll of ordinary origin, was the somewhat lesser intensity of band V. According to Kraus's idea this would prove a predominance of cyanophyll over xanthophyll. The solution of Coniferæ chlorophyll very readily turns to modified chlorophyll.

CHEMISTRY OF THE GRAPE.—In several treatises lately presented to the Royal Academy of Physical and Mathematical Sciences of Naples, Prof. G. Licopoli gives an account of some recent micro-chemical researches upon oranges, lemons, and grapes. The latter are of special interest, as Prof. Licopoli tried to determine the time and place at which, in the grape, the different chemical substances which are contained in it (such as tartaric acid, chlorophyll, albuminoid matter, sugar, colouring matter, &c.) first begin to form. The conclusions which the author draws from his labours are the following: Tartaric acid and chlorophyll first show themselves in the tissue of the pistils in course of formation. Oxalate of lime next shows its presence in the sub-epidermic tissue in the form of raphides, in the endocarpic epidermis in the shape of conglomerated crystals (dumb-bells?), and in the kernels in raphides. The albuminoid matter first appears spread over the whole of the fruit, but predominates in the mesocarp. Colouring matter results from the metamorphosis of chlorophyll, its appearance and diffusion showing the growth of the fruit, and the progress the chlorophyll has made at the time of its formation. The growth of this colouring matter begins in the peripherical tissues, and continues towards the central ones. Sugar is found in the pericarp wherever there is tartaric acid present. Resinous matter or wax appears first on the surface of the epicarpic epidermis. Tannic acid is principally formed in the seed, and particularly in the hard and friable part of the episperma; the fibro-vascular fascicles of the pericarp, however, also contain this acid.

NOTES

WE regret to hear that the state of health of M. Leverrier, the distinguished director of the Paris Observatory, is causing great anxiety to his friends. He has been entirely prostrated by his enormous labours, which have been almost unceasing for the last twenty years.

M. BELGRAND read, at the last sitting of the Council of the Paris Observatory, a report on the necessity of extending telegraph warnings to Algeria, and taking advantage of the documents collected by the Algerine Meteorological Service. The necessary steps will be taken by M. Leverrier, and observations extending from Marocco to Tunis, and from the Mediterranean coasts to Laghouat and Biskra will be sent to and from Paris to every Meteorological Office in connection with the meteorological system. At the same sitting M. Leverrier announced that he had taken steps to utilise the weather telegrams sent from America by the *New York Herald.*

THERE is no ground whatever, we are informed, for the rumour that M. Krantz will resign his office of Director of the International Exhibition, or that the Exhibition will be postponed. The works are progressing favourably, and will not be interrupted. The British Commission have secured a large plot of ground close to the Champ de Mars for their private use. This ground measures more than 5,000 square yards.

THE Emperor of Brazil, who is now in Paris, has been assiduously attending the meetings of various scientific societies. On Friday he was present at the meeting of the Zoological Society. Several communications were read on fishes, insects, and worms from Brazil. He was also present at the last sitting of the Geographical Society of Paris. A paper was read on the Pampas by an American gentleman, whose flattery of the Emperor was so high that his Majesty left the room to show his disapproval. The lecturer stated that the Argentine Republic was building a strong wall to protect the Pampas against incursions from uncontrollable Indians, and that in doing so not less than 20,000 square miles of excellent grazing ground will be reclaimed.

THE Congrès Scientifique of France is holding its present session at Versailles on the occasion of a floral meeting, as we intimated two months ago. The principal attraction is a series of excursions held in the vicinity of Versailles.

THE Russian Council of State has granted a yearly sum of 2,000 roubles to the West Siberian Branch of the Russian Geographical Society at Omsk.

RUSSIAN newspapers announce that Prof. Ahlquist had reached, on April 10, Kondinsk, 530 miles north of Tobolsk. His companion, M. Bergroth, remained at Tobolsk.

SCIENCE in Italy has suffered a heavy loss through the death of Prof. Dr. C. L. Rovida, formerly first physician at the Ospedale Maggiore of Milan, and for the last three years Professor of Special Pathology and Clinical Medicine at the University of Turin. Next to Prof. Moleschott in rank, if not in fame, he was one of the few Italians who follow a rigidly scientific method of investigation and instruction.

THE fourth number of the *Bulletin* of the Geographical Society of Egypt contains an interesting account of a journey to Harar with a plan of that place, published by two Arabian officials; one of whom, Fayous Effendi, accompanied the Italian expedition to Zeilah.

MOUNT VESUVIUS shows signs of fresh activity. The crater is continually sending forth clouds of smoke which at night assume a fiery aspect from the deep-lying glowing lava masses.

CAPT. R. GESSI, the Italian explorer of Albert Nyanza under Col. Gordon, is now preparing at Cairo to undertake, on his own account, a new expedition in company with a naturalist and a photographer. He intends to push forward to the Equatorial Lakes, studying, on his route, all the principal facts of meteorology, anthropology, and natural history, taking sketches and photographs of men, animals, plants, and interesting geological features.

A CORRESPONDENT writes to us that the medal "of the first class" of the Paris Acclimatisation Society was presented, not to Mr. Alfred Mosenthal, but to Messrs. Julius de Mosenthal and J. E. Harting, the joint authors of the work recently published by Messrs. Trübner and Co., on "Ostriches and Ostrich Farming," and reviewed by us in vol. xv. p. 176. This work was published some time *after* the experiments, to which we referred last week, were made at Algiers. These experiments will be found to be fully detailed in the work in question.

A LETTER received at Rome from the commander of the *Scilla* states that the Italian expedition for the exploration of Central Africa were occupied at Zeilah on April 27 with the final preparations for their then imminent departure to Shoa.

IN the last two numbers of the *Bolletino della Società Geografica Italiana* Prof. Gio. Beltrame publishes an interesting paper on the language of the Akkás—an African tribe of which two individuals were brought over to Italy some years ago. This first attempt at giving an idea of the Akká language and its grammar will prove a valuable contribution to the study of comparative African linguistics

AN immense quantity of locusts have shown themselves in the Algerian provinces, and are travelling from the south towards the Mediterranean. The number of these insects was so prodigious that the trains from Blidah to Algiers were almost stopped in the beginning of May.

MR. JOHN F. DOLLEY writes to the *Times* under date Uitenhage, Cape of Good Hope, South Africa, March 19:—"In this part of South Africa we have just witnessed a magnificent sight, such as a person can hardly expect to see more than once in a lifetime, if even then. It was on the beautiful clear starlight evening of the 16th of March, at about eight o'clock, when suddenly every one was startled with a bright lightning, like a flash, and on looking for the cause discovered a large meteor coming out of the eastern horizon, and which travelled slowly across the firmament, in an oblique direction to the westward, when it burst, sending forth streams of fire, as if from a hundred rockets, and then was heard a low rumbling noise as of thunder in the distance. The meteor appeared to be nearly, if not quite, as large as the full moon, but not round, more of an oblong shape, and while travelling through the air it very much resembled a large turpentine ball. It gave forth a bright bluish light which lit up the whole sky, and you could distinguish everything around you for miles as plainly as in the daytime. . . . A party of Hottentots who were coming in from 'Hankey,' a station belonging to the London Missionary Society, state that the driver of the waggon was struck down in the road, and that they all felt a glow of heat as the fireball passed them. The illumination lasted for nearly a minute, and the light was such that it dazzled the eyes of all who saw it."

A TELEGRAM from New York on the 16th states that forest fires are making great ravages in North-eastern New York, Long Island, Massachusetts, New Hampshire, Maine, Pennsylvania, Canada, and New Brunswick. A large part of the White Mountains is in flames. The summer hotels are in danger, and railways are interrupted. A great number of mills and dwellings have been destroyed, and hundreds of persons have been rendered destitute.

STEAM at ordinary pressure sent into saline solutions on which it has no chemical action, gives a rise of temperature that seems at first sight paradoxical, the temperature produced being always higher than that of the steam. M. Müller, of the Berlin Chemical Society, has been studying the phenomenon. Chloride of sodium is one of the best salts to use. A solution of it sufficiently concentrated to have a boiling point of 127° may be raised to 125° simply by sending steam into it at 100°. Here, then, the steam produces a rise of 25° above its own temperature. The more concentrated the solution the higher is the rise. M. Müller points out, in explanation, that saline solutions at 100° absorb the steam at the same temperature, and the result is a rise analogous to that produced when a gas, like ammonia, is dissolved in water. These experiments throw new light on the controverted question, what is the temperature of the steam which escapes from a concentrated and boiling solution? Is it 100° or a temperature near that of boiling of the solution? The new results seem to be against the latter, and common, view.

INTELLIGENCE has been received from Quebec stating that ships which have recently arrived at that port have encountered unusually large fields of ice and remarkably high icebergs in the Atlantic. The *Una*, from Leith, passed through eighty-five miles of heavy ice.

AT the meeting of the Royal Society of Edinburgh on Monday night, reports were read from four lighthouse keepers on the west coast of Scotland, detailing their experiences of earthquake shocks on March 11 and April 23. The keeper of the Pladda lighthouse says the tower by his dwelling-house shook very much; the Lismore keeper reported that everything in his lighthouse shook at an alarming rate and awoke all the inmates. Mr. Stevenson, C.E., said these observations were valuable because of their trustworthiness.

THE engineers of the French Northern Railway have been making experiments with the vacuum brake, which has been found to work satisfactorily owing to its simplicity of construction. MM. Sartiaux and Lartique have devised some ingenious arrangements for bringing it into action automatically if any mistake has been made respecting the crossings. Should distressed passengers want to call for help they can also put the continuous break into operation instead of ringing a bell as is customary.

THE Superintendent's Report on the Botanic Garden and Public Plantations for 1875-76 has recently been officially published in Jamaica. It deals almost entirely with plants of economic value, foremost of which is the coffee, the ordinary kind (*Coffea arabica*), apparently giving way to its formidable rival *Coffea liberica*, which was introduced to Jamaica in 1874, and is now thriving, especially in some districts. In one situation, at a height of about 1,000 feet above the sea, a plant that had only been planted out a little over a year has already fruited. This seems to indicate that in the course of a few years the new coffee may be widely cultivated in Jamaica from plants raised from seeds ripened in the island. Amongst other important plants treated of in the report which have received special attention, may be mentioned cocoa, sugar canes, pine apples, cinchonas, jalap, &c.; of this last we learn that nearly two acres are under cultivation, producing during the year under review a crop of 1,700, and it was estimated at the time the report was written that an additional 3,000 would be obtainable in the course of a few months, all of which would find its way to England.

DR. LAUDER LINDSAY of Perth has for years been forming a collection of lichens, which, although frequently broken up and distributed, still forms the nucleus of a good type collection. The collection consists of—1. Herbarium, the main object of which is to illustrate the Variations of the Commoner Species: and so to encourage (1) the establishment of Typical, Comprehensive, or Aggregate Species; (2) the abolition of Named Trivial Variations; and the (3) consequent Reduction of Names, and Simplification of Synonymy and Classification. 2. Museum of Illustrations of the Economic Properties and Applications of Lichens. 3. Library of Lichenological Works, Foreign and British. 4. Drawings (original) of Microscopical Structure, several thousands unpublished. 5. Correspondence with Lichenologists. 6. Unpublished Manuscripts—of (1) Outlines of Lichenology:" (2) "Lichenographia Britannica:" and of other works or papers illustrative of Lichens in various aspects. The collection has been offered—with all the cabinets and fittings in which it is contained—as a donation to the Royal Botanic Gardens of Edinburgh, on the simple conditions—(1) That a small room is provided for its accommodation in connection either with the Herbarium, or Museum of Economic Botany; and (2) That it is kept in proper order either by the Curators of said Herbarium or Museum, or by any of the numerous students of the Edinburgh School of Botany. But it is understood that no such donations can be accepted by the said school, by reason of the very inadequate accommodation provided by Government for the more essential requirements equally of students and teachers. As has been repeatedly pointed out—officially and otherwise—there is at present urgent need—(1) of a new commodious class or lecture room; (2) of a new commodious museum; (3) of botanical laboratories; and (4) of extra special rooms for such herbarian or other purposes as the reception and maintenance of such donations as that now referred to.

A NEW burner for obtaining high temperatures in laboratories has recently been described by M. Godefroy. It consists of four metallic cylinders one within another; the first and the third are pierced with lateral holes at their base. The intervals between the cylinders communicate, one set with two vertical pipes uniting in a horizontal pipe below, the other set with another similar system. A piece of metallic net at the lower part regulates the entrance of air.

AT Tabor, in Bohemia, 423 metres above the sea, in a house out from the town, M. Farsky made observations of the amount of carbonic acid in the air from October 10, 1874, to the end of August, 1875. The average obtained was 3·43 volumes in 10,000 volumes of air; a number smaller than that of Saussure and Boussingault (4·15), and higher than those got by Schulze in Rostock and Fittbogen in Dahme. The most numerous variations are in November, December, February, March, and April, the least in October. M. Farsky says that the more variable the weather, and the sharper the transition from one weather to another, the greater are the variations in proportion of carbonic acid in the air. The strong north-west and south-west winds reduce the amount of carbonic acid, while the cold north and north-east winds, which are always thought the heralds of clear weather, cause an increase in the carbonic acid. Further, the carbonic acid is increased descending mist, and continuous dust-rain. No other connection with atmospheric precipitates was perceptible. These results agree, in the main, with those formerly obtained by Angus Smith and Roscoe. Smith found that the air in the suburbs of Manchester contained on the average 3·69 volumes in 10,000 volumes of air. The amount appeared to diminish slightly during long-continued westerly winds; on the other hand it increased when easterly winds prevailed. From a large number of analyses of air collected from the hill-districts of Scotland, Smith obtained, as an average, 3·36 volumes in 10,000 volumes; the extremes recorded are 3·00 and 3·60. These comparatively low numbers are probably due to the proximity of the sea, the air over which has been shown by Thorpe, from a large number of analyses made over the Atlantic Ocean and Irish Sea, to contain about 3·00 volumes of carbonic acid in 10,000 volumes of air. This amount was constant, or nearly so, in different latitudes, and, contrary to the statements of Lewy, exhibited no perceptible diurnal or seasonal variations.

THE additions to the Zoological Society's Gardens during the past week include two tigers (*Felis tigris*) from Jahore, presented by Rear-Admiral Rowley Lambert, C.B.; two Javan Chevrotains (*Tragulus javanicus*) from Java, presented by Messrs. Hill and Isaac, Lieuts. R.N.; a Malayan Bear (*Ursus malayanus*) from Sumatra, presented by Dr. F. Wicksteed; a Phatagin Manis (*Manis tricuspis*) from West Africa, a Humboldt's Saki (*Pithecia humboldti*) from the Amazons, a Red and Yellow Maccaw (*Ara chloroptera*) from South America, a Sooty Mangabey (*Cercocebus fuliginosus*) from West Africa, deposited; a Concave Casqued Hornbill (*Buceros bicornis*), three Yellow-billed Blue Magpies (*Urocissa flavirostris*) from India, a Pin-tailed Whydah Bird (*Vidua principalis*) from West Africa, received in exchange; two Impeyan Pheasants (*Lophophorus impeyanus*) from the Himalayas, a Mantchurian Crossoptilon (*Crossoptilon mantchuricum*) from North China, purchased.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At a Congregation on May 17, the Vice-Chancellor, the Master of Emmanuel, Professors Cayley, Adams, Clerk-Maxwell, Stuart; Messrs. P. Frost, St. John's; J. Todhunter, St. John's; H. W. Besant, St. John's; N. M. Ferrers, Caius; E. J. Routh, St. Peters; A. Freeman, St. John's; H. H. Taylor, Trinity; W. D. Niven, Trinity; R. T. Wright, Christ's; C. H. Prior, Pembroke; W. Garnett, St. John's; and Lord Raleigh, Trinity, were appointed a Syndicate to consider the higher Mathematical Studies and Examinations of the University.

The Chancellor's gold medal, given annually to a resident undergraduate who shall compose the best English poem, has been adjudged to Edmund Whytehead Howson, Scholar of King's College. The subject of the poem is "The Heroism of Arctic Exploration."

The Moderators and Examiners for the Mathematical Tripos have presented a report of the results of the examination to the Board of Mathematical Studies, from which it appears that 110 candidates were examined. Of that number 36 were classed as Wranglers, 29 as Senior Optimes, 30 as Junior Optimes, one obtained an Ægrotat degree, and 14 were found to be not worthy of mathematical honours. The full marks were 18,643; the average obtained by the first ten Wranglers was 5,748; of the last ten Wranglers, 1,794; of the first ten Senior Optimes, 1,506; and of the first ten Junior Optimes, 721. The Additional Examiner (Mr. H. W. Watson) reports that the work done by the best men in the higher physics was very satisfactory, and proved the interest and success with which those subjects are now being studied in the University. At the same time there were indications of the tendency of the reading in this part of the course to become too diffuse and unmathematical. This tendency may be kept in check, in the first place, by framing the papers in such a manner—whether by the adoption of alternative questions or otherwise—as shall make too diffuse reading in the higher subjects unprofitable or even impossible; and, in the second place, by providing that every question set in these subjects shall be accompanied by a rider of strictly definite mathematical character, and of a difficulty proportioned to the weight assigned to the bookwork.

The Board for admitting and superintending non-collegiate students give notice that an exhibition of 50*l.* a-year, tenable for three years, granted by the Worshipful Company of Clothworkers for the encouragement of proficiency in physical science, will be awarded by means of the Certificate Examination, to be held next December, under the authority of the Oxford and Cambridge Schools Examination Board. Candidates must be either non-collegiate students in their first term of residence, or persons who have not commenced residence in the University. Full information may be obtained from the Censor of non-collegiate students, the Rev. R. B. Somerset, Cambridge.

GILCHRIST TRUST PRIZES.—The first (in London) presentation of prizes in physiology under the direction of the Gilchrist Trustees was made on Tuesday, last week, at the large room of the Society of Arts. The chair was occupied by the Rev. J. Rodgers, M.A., Vice-Chairman of the London School Board, supported by Dr. Carpenter, C.B., F.R.S., secretary to the Gilchrist Trust Fund, and others. The prizes were awarded to students who, as elementary school teachers, had attended the course of lectures on physiology recently delivered by Dr. B. W. Richardson, at St. Thomas's School, Charterhouse, and who had submitted to the examination with which the course was brought to a close. A large number of students entered into competition, and in the end prizes and certificates were awarded in the following order to four competitors:—John Pilley, George Price, W. R. Cory, and Maria J. Menzies; and certificates to Mary C. Menzies and Messrs. C. E. Marks, C. W. Shreeve, H. Steadman, J. F. Adcock, and G. Garland. In the course of the proceedings Dr. Carpenter gave a very interesting account of the origin of the Gilchrist Trust and of its founder, and the chairman delivered a very earnest and admirable address on the progress of education and on the value of the lectures such as had been delivered, and which he had himself attended.

WORKING MEN AND SCIENCE.—On Saturday afternoon the members of the Working Men's Clubs, under the auspices of the Working Men's Club Union, paid a visit, by permission of the Royal College of Surgeons, to the magnificent museum founded by John Hunter, and attached to the building of the College in Lincoln's Inn Fields. Prof. Flower, the Curator, received the visitors, who were conducted into the first great hall, where Prof. Flower gave a general description of the Museum. The visitors manifested an unmistakably genuine interest in the collection, and in Prof. Flower's descriptions, and at the conclusion of the visit one of the party, on behalf of his comrades, gave hearty thanks to the Professor and to the Council of the College, for the treat which had been afforded to them, and said it was altogether a mistake to suppose that the working men took no interest in science. The Professor said it afforded him much pleasure to show the museum, and especially so when he found his labours thus appreciated.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 3.—"On the Temperature-correction and Induction-coefficients of Magnets," by G. M. Whipple, B.Sc., Superintendent of Kew Observatory. Communicated by Robert H. Scott, F.R.S.

"Distribution of the Radicals of Electrolytes upon an Insulated Metallic Conductor," by Alfred Tribe, Lecturer on Chemistry in Dulwich College. Communicated by Dr. Gladstone, F.R.S.

May 17.—"On Hyperjacobian Surfaces and Curves," by William Spottiswoode, M.A., Treas.R.S.

Royal Astronomical Society, May 11.—Dr. Huggins, F.R.S., president, in the chair.—A gift of 500*l.* was announced from Mr. C. J. Lambert, being part of the sum bequeathed by his late father to scientific societies. The special thanks of the meeting were voted to Mr. Lambert.—The Astronomer-Royal pointed out an inaccuracy in a description of meteors by a certain "J. W. M."—Mr. Penrose read a paper (and explained a diagram) on the correction for the spheroidal figure of the earth.—Lord Lindsay spoke upon the two comets B and C of 1877. Winnecke's showed three bright lines on a weak continuous spectrum which he described. The president made some remarks thereon: there were two distinct spectra shown by comets; one was limited to two particular comets; the carbon spectrum was common to all the rest.—Lord Lindsay described the 4-inch heliometer which he had placed at the disposal of Mr. Gill for his expedition to Ascension to measure the parallax of Mars. The object glass was made by Mertz, and cut and mounted as a heliometer by Repsold; the pillar and equatorial mounting being that provided by Messrs. Cooke, of York, for an 8-inch telescope. The halves of the object-glass were moved in circular grooves by means of a handle near the eye-piece, so that they could be separated without putting the object out of focus. Several other details were pointed out, one of them being a movable wire screen adapted to equalise the light of two objects of different brightness under measurement. Mr. De la Rue admired the stability and rigidity of the instrument. The Astronomer-Royal would have had the declination axis and the polar axis twice as large. Mr. Gill vindicated the steadiness of Lord Lindsay's heliometer, and described an accident by which it narrowly escaped being smashed; which accident occurred through the instrument having been represented as an "universal equatorial," whereas it was nothing of the kind; the elevating screw having run out whilst being set to the latitude of ascension, the polar axis was shot out of its collar on to the floor. Mr. Gill then spoke upon the positions of the planets Ariadne, Melpomone, and Iris, and their special merit of having no sensible disc.—Dr. De la Rue again referred to the axes of the heliometer, which he said were eight times as strong as Repsold thought sufficient. The Astronomer-Royal said it was perfectly adapted to the purpose intended.—A note was read from Padre Secchi on an alleged fall of a meteorite in Italy which turned out to be untrue.—Mr. Lecky related how he had made a good artificial horizon by filling a blackened trough with glycerine.—A catalogue of double stars was presented by Messrs. Wilson and Seabroke, of the Temple Observatory, measured with a parallel wire micrometer and a power of 400. Mr. Dunkin said the only fault was the omission of the R.A. and N.P.D., which necessitated the use of two catalogues.—A note was read from Mr. Proctor referring to his chart of 3,976½ stars.—Mr. Green laid before the Society some paper impressed with blank discs to aid observers in drawing the features of Mars, so prepared that lights could be taken out with ease and precision.—Mr. Christie described Prof. Zenger's solar eye-piece, which consisted of a prism with one lenticular surface, cemented to another prism of different density; the quantity of light reflected at the junction being in proportion to the difference of the density of the two glasses.

Linnean Society, May 3.—Prof. Allman, F.R.S., president, in the chair.—Three foreign members were elected, and Mr. James Paton, of the Kelvin Grove Museum, Glasgow, was likewise elected an ordinary fellow.—Specimens of abnormal primroses were exhibited and commented on by Mr. Alf. W. Bennett.—A paper on the perfoliate penny-cress (*Thlaspi perfoliatum*) was read by Mr. G. S. Boulger. This little plant has a very limited British area, viz., the neighbouring districts of Oxfordshire and Gloucestershire, and according to our author Wilts; thus equally belonging to the Thames and Severn Valley provinces. The altitude attained is from 360 to 500 feet above the sea-level.—A conjoint memoir by Prof. St. G. Mivart and the Rev. R. Clarke on the sacral plexus and sacral vertebræ of lizards was brought forward and discussed. They stated that it has of late been recognised that in any attempt to reply to the question, which vertebra of any lower animal answers to the first sacral one of man, the nervous quite as much as the bone relations require consideration. Our authors pass in review the researches of Gegenbaur and Hoffmann, and then proceed to describe their own dissections of the parts in question in the chameleon, green lizard, iguana, monitor, and others. Instituting a comparison of the parts in the Batrachia, and of the sacral region in birds, they, in a somewhat technical summary, announce that although often puzzling and complicated from occasional variations in species and otherwise, the true sacral vertebræ may be defined in all vertebrates above fishes, where hind limbs are well developed.—The Secretary read a paper on the genus *Alveolites* and some allied palæozoic corals, by Prof. Nicholson and Mr. R. Etheridge, jun. It seems from their researches that the name Alveolites covers many forms whose affinities, to say the least, are obscure. Discussing the characters and essential attributes of the genus in a historical *résumé*, they proceed by comparisons, microscopic and otherwise, to define certain groups coming under previous definitions of *Alveolites*. These are several species of the above and others of genera such as *Cœnites*, *Brachypora*, *Chætites*, &c. But moreover they state that in several instance there appears much in common between certain groups of *Alveolites* and *Favosites*, so that future investigation may further necessitate the breaking down of what at present may be regarded as but meagre lines of demarcation.

Chemical Society, May 17.—W. Crookes, F.R.S., vice-president in the chair.—The chairman announced that an extraordinary general meeting would be held on May 31 at 8 P.M. The following papers were read:—On a slight modification of Hofmann's vapour density apparatus, by M. M. P. Muir and S. Suguira. The authors propose to omit the india-rubber plate of the original apparatus, and mark off the height of the mercury by a cathetometer and a slip of gummed paper.—Note on the fluid contained in a cavity in fluorspar, by J. W. Mallet. The cavity was 6 mm. by 2·5 mm. by 1 mm.; it contained water and a bubble. On heating, the bubble became less mobile and the crystal showed signs of incipient splitting.—Examination of substances by the time method, by J. B. Hannay. The author has determined the loss sustained by various hydrates in equal and successive intervals of time, when submitted, in a Liebig's drying tube, to a current of air at various temperatures, and thus obtains evidence of the existence of hitherto unknown hydrates. Magnesium sulphate, when treated as above, loses 8 per cent. of water in five minutes at 100° C.; the loss is then much slower and regular up to 29 per cent., when the rate of loss decreases somewhat suddenly from the formation of a lower hydrate, which loses water much more slowly.—On the dehydration of hydrates by the time method, by W. Ramsay. The author examined the hydrates of alumina, iron, copper, and lead.—On the transformation of aurin into rosaniline, by R. S. Dale and C. Schorlemmer; by heating sulphuric acid and pure phenol, and gradually adding oxalic acid, pure aurin is formed; by the action of ammonia on aurin, red aurin is produced, which, by the action of alcoholic ammonia at 150° for several days is converted into rosaniline. The authors consider aurin to be identical with rosolic acid.—On certain bismuth compounds, Part VI., by M. M. P. Muir. The author describes the preparation, &c., of hypobismuthous oxide, bismuthous oxychloride and oxybromide, and sulphbismuthyl chloride.—On the theory of the luminous and non-luminous flame by J. Philippson. The author states what he considers to be the causes of the luminosity and non-luminosity of flames.

Zoological Society, May 15.—Prof. Mivart, F.R.S., vice-president, in the chair.—Mr. Sclater made some remarks on the progress and condition of the Zoological Gardens of Rotterdam, Amsterdam, Antwerp, Brussels, and Ghent, which he had just visited.—A communication was read from Mr. G. S. Brady, C.M.Z.S., containing a monograph of the fossil Ostracoda of the Antwerp Crag.—A communication was read from Dr. F. Day containing a notice of the capture of a specimen of *Coregonous oxyrhynchus*, on the coast of Lincolnshire.—A communication was read from the Marquis of Tweeddale, F.R.S., containing a memoir on the birds of the genus *Batrachostomus*. The author came to the conclusion that there were seven recognisable species of this difficult group inhabiting the Indian region, one of which yet undescribed, was from the Philippines. The rule appeared to be that the females were rufous from the nest, while the males are brown and somewhat spotted.—Mr. Edward R. Alston read the description of a shrew from Guatemala, which had been indicated without being characterised by the late Dr. Gray, and for which the name of *Sorex veræ-pacis* was now proposed.—Mr. A. H. Garrod, F.R.S., read the second portion of a series of papers on the anatomy of passerine birds.—A communication was read from Mr. T. E. Buckley containing remarks on the past and present geographical distribution of the larger mammals of South Africa.

Entomological Society, May 2.—J. W. Dunning, F.L.S., vice-president, in the chair.—Messrs. H. J. Adams, Charlestrom, Adams, and J. W. Slater were elected members of the Society.—Mr. Jenner Weir exhibited a large silken cocoon from the Cape of Good Hope, supposed to be a spider's nest. On being opened it was found to contain, among other *débris*, the skins of a number of small spiders and the elytra of beetles of the genus *Moluris*. Mr. Weir also exhibited a spider's nest from Montserrat.—Mr. F. Grut exhibited a large species of *Chelifer* from North Spain.—Sir Sydney Saunders exhibited a spider (*Atypus sulzeri*) taken on Hampstead Heath, where it is found inhabiting tubes concealed under bushes. The tubes are about fourteen inches in length and extend about ten inches beneath the surface of the ground, the remainder projecting above the surface. The same or an allied species had been observed by Mr. Jenner Weir on the South Downs.—Mr. Champion exhibited a series of *Alaus pareyssi* from Thaso Island.—Mr. C. O. Waterhouse exhibited specimens of the following insects from Tasmania:—*Dohrnia miranda* (Heteromerous beetle), *Creophilus erythrocephalus* (*Staphylinidæ*), and *Forficula erythrocephala*. The two last species bore some mimetic resemblance to each other.—A paper was read from Sir Sydney Saunders on the adult larvæ of the *Stylopidæ* and their puparia, the author exhibiting specimens in illustration.—Mr. H. W. Bates communicated a paper on *Ceratorhina quadrimaculata*, Fab., and description of two new allied species. Specimens of the new species and also of *C. morgani*, Westw., were exhibited.

Physical Society, May 12.—Prof. G. C. Foster, president, in the chair.—The following candidates were elected members of the society:—Capt. R. Y. Armstrong, R.E., Mr. W. H. M. Christie, Lieut. N. Darwin, R.E., Prof. E. Frankland, D.C.L., F.R.S., Mr. H. F. Morley, Capt. R. G. Scott, R.E., and Mr. Angus Weiss. Mr. S. P. Thompson read a paper on the chromatic observation of the eye in relation to the perception of distance. He discussed the various means of estimating distances by the eye, showing that when data for forming a judgment by the associations of visible form or visible magnitude fail, the judgment is founded on "aërial perspective," or else upon the muscular sensation of adjustment to focus. As the eye is, however, not achromatic, it cannot be in focus at the same time for red rays and blue rays proceeding from one object, but may be in focus if the blue rays come from a more remote object. This gives a definite basis to the axiom of painters that blue is a retiring and red an advancing colour. Experiments were described demonstrating the truth of this fact, and illustration was afforded of the chromatic aberration of the eye by casting beams of light through a solution of permanganate of potash upon a silvered ball, the illuminated point appearing red with a blue surrounding halo to an eye adjusted to short focus, but blue with a red halo to long focus.—Prof. Guthrie referred to the theory by which the apparent size of an object depends on the amount of nervous excitement which it occasions, whether this be due to the extent of the illuminated area or the intensity of its illumination, and he pointed out that an object always appears larger when looked at with two eyes than with one eye.—Mr. Roberts drew attention to the fact that the system ordinarily adopted in mechanical drawing of assuming the light to fall from the left hand top-corner gives an appearance of solidity, whereas if this be reversed, and the light falls from the right-hand bottom corner the object appears hollow.—The president referred to the well-known fact that if two stereoscopic pictures are taken, representing the same object in complementary colours, most people have a great difficulty in combining them so as to see a single picture of a neutral tint.—Mr. S. P. Thompson then described a curious observation of change of pitch occurring when a tuning-fork is caused to rotate rapidly round its axis; the nodal interferences at each quarter rotation ceasing to be separately heard when recurring more than about thirty times in a second. He has attempted various ways of estimating the amount of this change of pitch, including a method founded on the binaural estimation of interference beats.

Institution of Civil Engineers, May 8 and 15.—Mr. George Robert Stephenson, president, in the chair.—A paper by Sir G. W. Armstrong, C.B., F.R.S., V.P. Inst. C.E., on the history of the modern development of water-pressure machinery was read.

CAMBRIDGE

Philosophical Society, May 7.—Prof. Clerk Maxwell, president, in the chair.—Mr. J. W. L. Glaisher read a paper on expressions for the theta functions as definite integrals.—Mr. Warren's fourth "Exercise in Curvilinear and Normal Coordinates" was presented to the Society by Prof. Cayley, and will appear in the next issue of the Society's *Transactions*.

PARIS

Academy of Sciences, May 14.—M. Peligot in the chair.—The following papers were read:—Isoperimetric triangles having one side of constant size and the summit at a fixed point, by M. Chasles.—Rotatory action of quartz on the plane of polarisation of obscure calorific rays, by M. Desains. He has examined the action of six groups of dark rays of decreasing refrangibility. The law of thicknesses still applies to the least refrangible rays. In the symmetrical dark region of nearly extreme violet the polarised heat is so little sensible to the action of quartz that transmission through a plate 0·015 m. thick, gives hardly a rotation of 5 degrees, or $\frac{1}{3}$ degree per millimetre. This is 132 times less than for the violet of M. Biot's table. M. Desains describes his apparatus fully.—Analysis of an ancient wine, preserved in a glass vessel sealed by fusion, by M. Berthelot. This was found on the site of an old Roman cemetery near Arles, and probably dates back some sixteen hundred years.—Analysis showed in a litre, 45 c.c. alcohol, 3·6 gr. fixed acids, 0·6 bitartrate of potash, 1·2 acetic acid. There were also tartrate of lime and traces of acetic ether. It is a weakly alcoholic wine, which must have entered on acetification before being put in the tube.—M. Serret presented tome vii. of the "Œuvres de Lagrange," completing the series.—On the new navigation, by M. Villarceau.—On the origin and nature of the fever called typhoid, by M. Guerin. The object of this third memoir is to show that the toxical principle produced by stercoral fermentation causes what are looked on as the anatomical characters of the fever. M. Guerin has proved that the liquids specially poisonous are those which accumulate near the end of the ileum, and are permanently retained by the ileo-cœcal valve. The poison passes into the mesentery and to the ganglions contained in it.—New exposition and generalisation of the method of Gauss for calculating approximately a definite integral, by M. Pujet.—New meteorological maps of the South Atlantic, giving at once the direction and the intensity of the winds, by M. Brault. The general movement of the summer winds there is that of an immense cyclone with its centre about 30° or 35° south latitude, and 10° or 20° west longitude. It turns in opposite directions to the hands of a watch, and gives off the south-east trades towards Africa, &c. There is not a zone of tropical calms, nor a zone of weak and arbitrary breezes.—On a new type of simple monstrosity, omphalocephaly or umbilical hernia of the head, by M. Dareste. The head seems to come from the aperture of the umbilicus. The form has been observed in the hen, but not in man or mammalia.—Experiments made at the vitreolar station of Cognac with the view of finding an efficacious remedy against phylloxera, by M. Mouillefert. This is in favour, specially, of sulpho-carbonate of potassium.—On a modification of the pneumatic mercury machine, by M. Serret. This consists in substituting a simple valve for the glass stopcocks. In another form, even the valve is suppressed, and vacuum obtained by free circulation of mercury in simple tubes.—On solar spots, by M. Tacchini. He thinks the sun's surface at present in a true state of repose relatively to the great phenomena observed at a time of maximum sun-spots. He shows the contrast in metallic eruptions and spots between 1871 and 1876 by figures. In the first four months of 1877, he adds, there has been a diminution in the visibility of magnesium, so that the line 1474 κ has had a marked superiority.—On the otheoscope, (a new arrangement of the radiometer), by Mr. Crookes.—Direct transformation of mechanical work into electricity, by M. Guignet. In an electromagnetic machine, having six electromagnets and a drum with six bars of soft iron, the wires are connected with a galvanometer and the drum is rotated by the hand. A continuous current is thus produced, and it is reversed on reversing the rotation. This experiment also shows the induction taking place under the influence of the earth; the soft iron is magnetised, and the magnetisation increases with the speed of rotation.—Note on work in chemistry at the Polytechnical School of Rio de Janeiro, by M. Guignet.—On work of the School of Mines of Ouro Preto, by M. Gorceix.—On some monochlorised acids of the amylic series, by M. Demarçay.—On the salts of sesquioxide of chromium, by M. Etard.—Researches on pseudopurpurine; continuance of researches on the colouring matters of madder, by M. Rosenstiehl. The remarkable instability of pseudopurpurine (he says) is a happy circumstance; as neither it nor alizarine could give a plant the extraordinary importance of madder.—On an application of the microscope to ceramic art, by MM. Fouqué and De Cessac. This refers to vases found at Santorin.—On a new larval form of Cestoides, by M. Villot.—On granular conjunctivitis in Egypt; *résumé* of observations on ophthalmias of North Africa, by M. Gayat. Eye diseases are endemic in North Africa, and have their common origin in granular conjunctivitis, which is brought on by atmospheric and terrestrial conditions.—Treatment of hypopyon, by M. Fano.—On M. Maumené's gas hydrometer, by M. Dumas.—A new arrangement of the electromagnetic induction apparatus with automatic interruption, by M. Becquerel.—On the glaciers of Greenland, by M. Malland.

ROME

R. Accademia dei Lincei, April 15.—The Age of Bronze in the piles of Peschiera in the Lago di Gardo, by M. Ferri.—On the use of the reversed siphon in the ancient conduits of water, by M. Lanciani.

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THURSDAY, MAY 31, 1877

THE UNIVERSITIES BILL AND UNIVERSITY MOVEMENTS

THE monotonous progress of the Oxford and Cambridge Bill through the House of Commons has never been interrupted. The Government having drawn its measure, consents to modify it in a few trivial points, but wherever it does not consent, the House accepts it and passes it as an elementary matter of party discipline. Very few of the amendments are of serious importance. Lord Edmond Fitzmaurice has added perhaps the most valuable. He enables the Universities to give a definite status and payment for any special educational work done out of the University under its control. Thus the funds of Oxford and Cambridge may be freely used in support of their missionary efforts in the large and neglected towns of England. There is no reason why permanent educational centres should not be established under this clause in any part of England. Any College with too much money can assign a sufficient portion of it to the advancement of learning in the neglected provinces. Leeds, Bristol, Birmingham, Halifax, Sheffield have had university men lecturing in their midst under the University Extension Scheme, and Lord Edmond Fitzmaur ice's amendment invites the colleges to support and endow this movement. Under it the "idle fellows" who now run off to the bar will be drawn more and more into teaching, for teaching will open to them more and more of a career. No doubt there is risk that the possibility of such grants may paralyse local effort. But as yet it is only a possibility, and the colleges may be trusted to distribute their money only in places where adequate local effort cannot be expected. Another amendment by Mr. Trevelyan enables the Commissioners to review the conditions under which university emoluments can be given, especially those relating to age. There is no doubt that the age of undergraduates has increased, is increasing, and ought to be diminished. The honour man keeps back at his school as long as possible in order to make sure of getting his entrance scholarship. Even if the age at which entrance scholarships can be taken were restricted, honour men might in many cases delay so as to have a better chance after they come up of the scholarships and fellowships which are to be gained while in residence. It is reasonable to say that the work which a man is to do under the pressure of a prize examination *ought* to be over by twenty-one. What comes after is another matter. The competitions of the University imply a discipline which is admirable for youth "under tutors and governors," but which is merely fatiguing to men of mature intellect with serious objects in life.

A clause is to be brought up by Mr. Goschen on the report which provides that the Commissioners shall first publish the main outlines of their plans before they are allowed to apply them in detail. Certainly it seems odd that Parliament should consent to throw the whole of the questions affecting the Universities into the hands of any body of Commissioners without asking for some statement from them by which they may discover the general drift of their ideas and the character they propose to give their work. It is of great importance that before any one of them is dealt with the Colleges should know on what principles it is proposed to deal with all of them, and what are the objects to which, in the opinion of the Commissioners, the money taken should be applied. In an amusing letter to the *Pall Mall Gazette* a few days ago, Mr. Dodgson, of Christchurch, represents science at Oxford as first modestly asking endowment to enable her to teach, next claiming endowment for boys to be sent to her to be taught, and lastly demanding an endowment to enable her to "think." It is an amusing travestie of the claims of the more advanced and extreme "researchers," and it is natural to ask what is the view of the Commissioners on the subject.

Since Parliament separated for the Whitsuntide holidays two things have happened in Oxford, which indicate the drift of that University opinion which in the end controls the actions even of Commissioners. The first was the Oxford scheme for new professorships. It was an echo —certainly an exaggerated one—of a similar scheme proposed some time ago by some similar board of studies in Cambridge, and they both showed that the leading members of the two Universities are not indisposed to consider very large and radical schemes of reform. Both of them will give fresh strength to the party, more powerful perhaps in sympathy than in numbers, which is suspicious not merely of proposed researchers with nothing to do but "to think," but of possible professors with very few lectures to deliver, and still fewer students to hear them. It is the commonest and the most vulgar argument against such proposals that they are really drawn in the pecuniary interest of the class of actual and possible professors who are numerous in the Universities. The Hebdomadal Board shows us at any rate one distinct way in which it is possible to utilize the endowments of Oxford and Cambridge, and they put fairly before us the question whether it is not a better way than the prize fellowship system.

The second event of the past week was the discussion in congregation of the new resolutions on natural science degrees. It is proposed that instead of there being one kind of degree and only one—that in Arts—open to ordinary undergraduates, a new degree shall be created in Natural Science. The Bachelor of Arts has to pass three examinations, at which he has to show proficiency in Latin and Greek. It is proposed that the new Bachelor of Science should be let off with Latin or Greek, the missing classical language being replaced by German or French. The Bachelor of Science was meant to know more mathematics than the Bachelor of Arts, but the mathematical men have pressed the question whether mathematics itself is not a science, and whether mathematical honour men ought not to be let off as well as experimental men with one dead language. So far as things have yet shown, Oxford is in favour of the change, and eager to consider the case of the mathematicians. These are, in fact, the proposals of the Duke of Devonshire's Commission. We desire to speak with the greatest possible respect of those who maintain the *status quo* of the two classical languages, but it seems to us impossible to ignore the fact that the Greek learned by the pass man is about as much of a "possession for ever" as the Hebrew of most clergymen. It is begun late; it is not carried far enough to give the student any real pleasure in reading a Greek book for himself; it is thrown aside as soon as its use, which is solely a "pass" use, is over, and done with. Mr. Sidgwick says, with perfect fairness, that "the study of Greek is one thing, the knowledge of the *Alcestis* and the study of Mr. Bohn's translation of it another." That the University should have voted in this sense by sixty-three votes to forty, and expressed its desire to treat the mathematicians as entitled to a similar relief by twenty-seven to fourteen is a conclusive proof that the world moves even at Oxford. Many of those who are best acquainted with that University indeed declare that it is there only that it does move—at all events, that it is only there that it moves by "leaps and bounds," as British commerce used to do in the happy days of Mr. Gladstone's ministry! Certainly the votes of Oxford are often more liberal than those of London, and we cannot doubt that whenever the new Commission sets to work it will find as much impulse as obstruction from that great University. Of Cambridge itself it has ceased to be true to say that she maintains her usual attitude of magnificent repose. The universities are anxious to reform themselves if they only know how—the Commissioners will be happy to assist them if they only get power enough—and we may perhaps hope that a few "thinkers" may get something out of the reconstruction better than the very plain living with which their "high thinking" has hitherto been so commonly coupled.

THE NEED OF MUSEUM REFORM

FEW of the many subjects now pressing themselves on the attention of the public are more important than that of museums, of the work which they are doing now in general education, and what they may reasonably be expected to do in the future. It is one which has occupied my mind for many years, and on which I venture to offer the following remarks.

The collecting instinct, the desire to accumulate what strikes the fancy, is so universal in all minds lifted above the satisfaction of the mere animal needs, that its absence is to be viewed as an infirmity or misfortune analogous to colour blindness or deafness. It is present in some form or another in most savages, and even in some birds, such as the bower-bird. It is based ultimately on the principle of curiosity combined with that of selfishness. Poor and much to be pitied is the man who has it not. The collections which result from it bear the stamp of the individual who makes them, and are as various as his tastes. They may be conveniently termed museum units, which, like molecules, have a tendency to coalesce into bodies of greater or less size, and thus constitute museums. These are of high or low organisation according as the units keep or lose the stamp of the individual, and have been moulded into one living whole or are dissociated. They are highly organised and valuable if the parts are duly subordinated to each other and brought into a living relationship; they are lowly organised and comparatively worthless if they remain as mere assemblages of units placed side by side without organic connection and without a common life.

Unfortunately in this country the provincial museums mostly belong to this latter class. It is that which takes shelter for the most part in the top rooms of Mechanics Institutes and in the holes and corners of Free Libraries and Museums. In one instance which occurs to me, you see a huge plaster cast of a heathen divinity surrounded by fossils, stuffed crocodiles, minerals, and models of various articles such as Chinese junks. In another, a museum unit takes the form of a glass case containing a fragment of human skull and a piece of oatcake labelled "fragment of human skull very much like a piece of oatcake." In a third, wax models are exhibited of a pound weight of veal, pork, and mutton chops, cod fish, turnips, potatoes, carrots, and parsnips, which must have cost the value of the originals many times over, with labels explaining their chemical constitution, and how much flesh and fat they will make—just as if the public were unacquainted with those articles of food, and required any information as to what these names really cover. Strangely enough this museum unit appears modern. In very many museums art is not separated from natural history, nor from ethnology, and the eye of the beholder takes in at a glance the picture of a local worthy, a big fossil, a few cups and saucers, a piece of cloth from the South Seas, a war club or two, and very possibly a mummy. The result of such an association as this, of articles which have no sort of relationship with the rest, is to convert the whole into rubbish, using the word in the Palmerstonian sense of being "matter in the wrong place." I do not mean to say that museums of this low order are useless. In default of better they are useful, just in proportion as they encourage the collecting instinct in the beholders. They may ultimately arrive at the higher stage of development. It is, however, a reproach to this country that museums of this low type should be found at this time, not merely in the smaller towns, but in some of the more important centres of population. They constitute a serious blot on our educational system, which we are striving to make as perfect as possible, since they are worse than useless for purposes of teaching. Instead of the natural harmony of things, they put before the mind a fortuitous concourse of atoms which is a very chaos.

While this state of things prevails largely in this country, there is no room for astonishment that museums of natural history hold the position which they do hold in the public mind. They are looked upon as haunts of the mere specialist, and as altogether outside any scheme for the advancement of the higher studies. If they are sufficiently attractive to be visited, they are treated as places of amusement, in which "a happy day" may be spent, instead of places of instruction. They are sometimes avowedly arranged for that end. It rarely enters into any one's head that collections are as absolutely necessary for the advancement of natural history studies, as books to the literary student, though it is allowed on all hands that natural history is of great importance in general education. Until this anomaly be removed by the re-arrangement of the museums which require it, and the establishment of new ones, it is hopeless to expect the natural sciences to flourish as they should flourish, or for them to assume the importance which they deserve in the studies of this country. To the obvious remark that the fruits of English natural science are not worse than those of our neighbours, it may be answered that what has been done is the result of personal effort overcoming obstacles, and succeeding in spite of disadvantages. The fact that some men can swim does not render life-belts unnecessary for those who cannot.

Well-arranged museums of every kind are now an educational necessity in every highly civilised state, and everywhere excepting in our own they are put on exactly the same footing as libraries. They are to be seen in nearly every town of any pretension on the Continent, and in cities, such as Turin, Bologna, Lyons, Brussels, and Hamburg, they exist on a scale which is only rivalled by those of London. In the United States, also, and in Australia, their value to society at large is fully recognised. They are liberally supported and largely endowed. In no museum out of Britain have I seen the chaos from which our own are now painfully and slowly emerging.

There are many highly organised museums in Britain which perform their true function as repositories of knowledge, such as those at London and the Universities, those of Leeds, Liverpool, Bristol, Taunton, Exeter, Salisbury, and others. Their numbers must be largely increased if we are to hold our own in the race for knowledge with our neighbours on the Continent and our kinsmen in Australia and America.

W. BOYD DAWKINS

FOSTER'S "TEXT-BOOK OF PHYSIOLOGY"

A Text-Book of Physiology. By M. Foster, M.A., M.D., F.R.S., Prælector of Physiology, and Fellow of Trinity College, Cambridge. (Macmillan, 1877.)

PHYSIOLOGY, like most other sciences, has been making rapid strides within the last half century, and although scarcely yet to be classed among the exact sciences, the number of well-established facts which have been accumulated is not inconsiderable, and is in several cases sufficient to serve as a substantial foundation for the building up of more or less stable theories, and the enunciation even of tolerably fixed laws. Consequently it is to-day no longer necessary to urge the importance of the cultivation of physiological science as the basis of rational medicine. Not only is this fully recognised by the medical profession but there are distinct indications that the public in general is beginning to appreciate the importance of a correct knowledge of the normal processes which are going on within the body, preparatory to the recognition and cure of such deviations from the natural processes as constitute disease. And no wonder the science should be popular the object of which is to teach us how "we live and move and have our being!"

Students of physiology in this country have long needed an advanced text-book containing the leading facts and inferences of the science set forth at length and intelligibly, the statements and deductions which are less important or less clearly established being relegated to the background of small print, or even omitted altogether. The place of such a book was supplied in Germany by Hermann's "Grundriss der Physiologie," in every respect a model text-book, and one of which it is impossible to speak too highly; and it was hoped that the production last year of a translation of Hermann's work would meet the requirements of the English student. But whether simply from the fact of its being a translation or from other causes, certain it is that the book has not fulfilled the expectations which were entertained with regard to it. It is, therefore, a matter for sincere congratulation to physiologists that one in every respect so well qualified for the task as Dr. Michael Foster should have undertaken to provide what was so much needed in the way of a text-book, and also, it may fairly be added, to himself, that he should have brought the undertaking to so successful an issue.

Dr. Foster's aim in writing the book is best given in his own words:—

> "I have striven to explain, in as clear and straightforward a manner as I could, the main facts and fundamental principles of physiological science. The student before whom things both new and old are tumbled out of the physiological treasury, without adequate critical appreciation of their respective values, is simply bewildered instead of being taught. . . . . And it is the duty of the teacher to bring his pupil to that which is fixed and sure, without too much display or too much neglect of that which is uncertain and loose. . . . . A desire to contribute, as far as my powers will allow, to the development of physiology in the medical profession has been my guiding principle in writing the book."

The style of treatment and the mode of thought pursued throughout are characteristic of the author, and serve to indicate the originality of the book, always one of the chief merits of a work of this sort. It is often thought that a scientific text-book need be little more than a museum of facts and opinions, carefully arranged and neatly labelled, to enable them the more readily to be "got up" by the student, in order that he may be able to satisfy an examiner with a narration of whatever has been stated or conjectured about any particular subject. Were this the case, the making of a text-book would be a mere matter of scissors and paste-brush, and the task could be performed by any one who was capable of reading the language of the science. That the idea is a wrong one is so self-evident that it would be waste of words to delay in refuting it. Even an elementary text-book is never so well done as when it is taken in hand by one who is a master in the science. There is a well-known instance in this particular science of physiology, and, in fact, it is to Huxley's "Lessons" that Dr. Foster wishes his book to be regarded as the sequel.

An indication of the original character of the book before us is to be found in the fact that it is not throughout equal. All the subjects are well done, but some are better treated than others, a result to be expected from the very extended nature of the science. No one—not even a Helmholtz—can pretend to an intimate personal acquaintance with all the branches of so ramified a science, and it is casting no slur upon the rest of the work to single out a section here and there, characterised by the especial clearness with which the known facts are stated and the phenomena are discussed and explained. The articles on the coagulation of the blood and on gastric and pancreatic digestion, and the chapter on the spinal cord, may be especially mentioned as illustrations of this.

Another and a more prominent indication of originality is occasionally met with in the descriptions of observable phenomena, facts being here and there noticed which are obviously the result of personal observation, and which have not hitherto so far as we are aware, been noted down. Thus in describing the phenomena of the heart's beat in mammals the contraction of the auricles is stated to be preceded by a peristaltic contraction of all the great veins which open into the heart. And there is no doubt that Dr. Foster is right in describing this as a distinct factor in the cardiac cycle.

The manner in which the nervous system is introduced to the student calls for special comment. In treating of the various phenomena of the circulation, respiration, secretion, &c,, frequent mention must necessarily be made of the relation of the nervous system to those functions. This pre-supposes a knowledge of the mode of origination and transmission of nervous impulses, and of the changes they may undergo in transmission, and hence of the fundamental properties of the nervous tissues. It might well, therefore, be deemed proper to commence a work on physiology with the account of a system which, in the higher animals at least, dominates and directs all the other functions. But on the other hand, from its greater intricacy, it is found in the teaching of physiology by far the most convenient plan to defer the account of the nervous system until the simpler, and more obviously physical phenomena of the living body have been dealt with.

Dr. Foster has got out of the dilemma in an ingenious and effectual manner. For whilst the account of the central nervous system and its principal instruments—the sense-organs—are reserved for one of the concluding chapters of the book, a short exposition of the fundamental properties of the nervous tissues, and also, but in very general terms, of the special functions of the chief nervous centres, is introduced at quite an early stage—a mode of dealing with the subject which enables such important questions as the influence of the nervous system upon the heart, respiration, secretion, &c., to be discussed at length with their respective subjects in place of being deferred until the end of the book. We are not sure that this introduction to the nervous system might not with advantage have come in even earlier than it does; as a sequel, in fact, to the chapter with which the work is introduced.

We quote a few passages from the prefatory article on the fundamental properties of the nervous system as yielding an illustration of the gradual, easy way in which Dr. Foster leads up to a difficult subject :—

> "In its simplest and probably earliest form a nerve is nothing more than a thin strand of irritable protoplasm, forming the means of vital communication between a sensitive ectodermic cell exposed to extrinsic accidents, and a muscular, highly contractile cell (or a muscular process of the same cell) buried at some distance from the surface of the body, and thus less susceptible to external influences. If in hydra we imagine the junction of the ectodermic process with the body of its cell to be drawn out into a thin thread (as appears to be the case in some other hydrozoa), we should have just such a primary nerve. Since there would be no need for such a means of communication to be contractile and capable of itself changing in form, but on the other hand an advantage in its remaining immobile, and in its dimensions being reduced as much as possible consistent with the maintenance of irritability, the primary nerve would in the process of development lose the property of contractility in proportion as it became more irritable, *i.e.*, more apt in the propagation of the waves of disturbance arising within the ectodermic cell.
>
> "We have already seen (introductory chapter) that automatism, *i.e.*, the power of initiating disturbances or vital impulses, independent of any immediate disturbing event or stimulus from without, is one of the fundamental properties of protoplasm. In simpler but less exact language, such a mass of protoplasm as an amœba, though susceptible in the highest degree to influences from without, 'has a will of its own.' . . . . A hydra has also a will of its own; and seeing that all the constituent cells are alike, we have no reason for thinking that the will resides in one cell more than in another. . . . In both hydra and amœba the processes concerned in automatic or spontaneous impulses, though in origin independent of, are subject to and largely modified by, influences proceeding from without . . . .
>
> "The next step of development beyond hydra is evidently to differentiate the single (ectodermic) cell into two cells, of which one, by division of labour, confines itself chiefly to the simple development of impulses as the result of stimulation, leaving to the other the task of automatic action, and the more complex transformation of the impulses generated in itself. The latter, which we may call the eminently automatic cell, will naturally be withdrawn from the surface of the body, while the other, which we may call the eminently sensitive cell, will still retain its superficial position, so that it may most readily be affected by all changes in the world without. And just as a primary *motor* nerve arises as a retained thread of communication between a sensitive cell and its own muscular process, so a primary *sensory* nerve may be conceived of as arising as a thread of communication between an eminently sensitive cell, and its twin the eminently automatic cell. . . . . Naturally the muscular process or muscular fibre would, on the splitting of the original single cell, remain in connection with the most eminently automatic. We thus arrive at that triple fundamental arrangement of a nervous system in its simplest form, viz., a sensitive cell on the surface of the body connected by means of a sensory nerve with the internal automatic central nervous cell, which in turn is connected by means of a motor nerve with the muscular fibre-cell. . . . ."

In the introductory chapter of the book the physiological processes which occur in the amœba are described, and these are taken as the basis upon which the whole superstructure of the science is to be built up. This is a wise course to pursue, for in a work on physiology the amœba cannot have too prominent a place assigned to it. It is over the amœba that the battle of physiology must eventually be fought out.

The chief organic compounds met with in the animal body are considered together in an appendix, which has evidently been carefully drawn up, and will be found not the least useful portion of the book. It is, no doubt, an advantage in many respects to have a series of similar facts thus collected and put on one side for reference. At the same time it may be questioned whether in the case of some of the bodies which have been relegated to this situation—the constituents of the blood and urine, for example—it would not have been better rather to have awarded them somewhat greater prominence in the parts of the book which treat of the special fluids or secretions in which they occur. A frequent reference to the appendix in such places might answer the purpose.

It is pleasant in a book of this sort to be able to find so little that is open to adverse criticism. It is true the latter part of the work bears traces of haste, and one or two important subjects are somewhat lightly dealt with. Moreover here and there statements which are anything but "fixed and sure" are to be found clothed in large type, while others, which are based upon a large number of exact experiments (the observations of Ludwig and

Woroschiloff upon the paths of conduction in the spinal cord, for example), are confined to small print. Exception might also be taken to the somewhat dogmatic decision of an undecided question, such as that of the cause of the pulse-dicrotism. And it may be doubted whether the introduction, if not of pictures, at least of a greater number of diagrams, would not render some of the subjects easier to the comprehension of the student. But looked at as a whole, the book must be pronounced thoroughly well done, admirably adapted for its purpose, and creditable alike to its author and to the science which it is intended to promote. E. A. SCHÄFER

WEISBACH'S "MECHANICS OF ENGINEERING"

A Manual of the Mechanics of Engineering, and of the Construction of Machines, with an Introduction to the Calculus, by Julius Weisbach, Ph.D. Vol. I.—Theoretical Mechanics. Translated from the Fourth Augmented and Improved German Edition, by Eckley B. Coxe, A.M. (London: Trübner and Co., 1877.)

THERE is, perhaps, no book on mechanics so well suited to the wants of civil and mechanical engineers as the late Dr. Weisbach's "Lehrbuch der Ingenieur und Maschinen-Mechanik." In his preface to the first edition of his work the author thus stated his design (we quote from the translation before us):—

> "My principal effort has been to obtain the greatest simplicity in enunciation and demonstration, and to treat all the important laws in their practical applications without the aid of the higher mathematics. If we consider how many subjects a technical man must master in order to accomplish anything very important in his profession, we must make it our business as teachers and authors for technical men to facilitate the thorough study of science by simplicity of diction, by removing whatever may be necessary, and by employing the best known and most practicable methods. For this reason I have entirely avoided the use of the calculus in this work. Although at the present time the opportunities for acquiring a knowledge of it are no longer rare, yet it is an undeniable fact that unless we are constantly making use of it, we soon lose that facility of calculation which is indispensable; for this reason so many able engineers can no longer employ the calculus which they learned in their youth. As I do not agree with these authors who in popular treatises enunciate without proof the more difficult laws, I have preferred to deduce or demonstrate them in an elementary, although somewhat in a roundabout manner."

Weisbach was severely censured by some people for attempting to treat his subject without the higher mathematics, but he kept to his own way, saying that he intended his work not as a university text-book, but only for "practical men." In the later editions of his work, however, he gave additional demonstrations of some of the laws by the differential and integral calculus, on which he also added an introductory chapter, which surpasses in clearness anything we have seen on the subject. In judging of Weisbach's method we must not forget that few men had so much experience in teaching practical engineers, and that no one had a better knowledge of what such men really require; and we must at least acknowledge that, although in some few cases he may have carried his system too far, and have sacrificed scientific exactness of expression to mathematical simplicity, if he erred, he did so on the right side. Most English authors of books on mechanics and kindred subjects seem to forget how small is the amount of mathematical knowledge possessed by the average engineer. In the rising generation of engineers this is no doubt changing for the better, but there are still many in this country—men of ability and men who have executed works which do credit to the nation—whose ideas of the differential and integral calculus are vague in the extreme, and it is satisfactory that there is at last a standard work for their use.

The work appeared originally in two parts. The first "Theoretische Mechanik," and the second, "Statik der Bauwerke und Mechanik der Umtriebsmaschinen." To these was afterwards added a third, "Mechanik der Zwischen und Arbeits-Maschinen." The volume before us is a reprint of an American translation of the fourth German edition of the "Theoretische Mechanik." This edition was published in 1863. In 1875, after the author's death, a fifth edition was issued, which differs to a small extent from the fourth. It contains a chapter on springs, taken principally from Reuleaux's "Construction und Berechnung der für den Maschinenbau wichtigsten Federarten;" another on the general principles of dynamics, also, in the Appendix, "The Elements of Graphical Statics," none of which are in the fourth edition. We regret that the chapter on graphical statics was not added to the translation. The graphical method seems likely to come more generally into use, and a short introduction to it, giving the general rules for its application to statics, and showing the manner of using it, would have added to the value of the book.

Mr. Coxe has done the work of translation carefully, and, on the whole, well. He has avoided the blunders made by the author of the former translation, and he has given a faithful rendering of the German. He uses, however, many terms which are not in use in this country, and we are sorry that they are far from being improvements. He talks, for example, of the centre of gravity of lines and geometrical figures, of "living forces" (surely *vis viva* was quite bad enough), &c. There is room for difference of opinion as to whether Mr. Coxe has done wisely in retaining the various tables in the book on the metric system.

This translation is wonderfully free from misprints, and most of those which do exist are quite apparent. On p. 479, for example, we find the following:—"REMARK.—Under the supposition that the proof strength increases and decreases with the ultimate strength, the English engineers increase the size of that portion of cast-iron girders which is subject to *compression*," &c. (the italics are ours). On p. 121, § 21, $v = \frac{\sigma}{\tau}\left(\frac{ds}{dt}\right)$ should be: $v = \frac{\sigma}{\tau} = \frac{ds}{dt}$, and "The element of time $\tau\,(dt)$" should be: The element of time $\tau = dt$. On p. 291, § 157, we have: "From this we obtain $QR = \overline{OR}.\tan.\phi = O\overline{R}$." Then on the line below "$\frac{y}{b} . \frac{V}{H}$, which is the difference," &c. This should be $QR = OR.\tan.\phi = OR.\frac{y}{b}.\frac{V}{H}$.

Weisbach's name is known in this country principally in connection with hydraulics. In this branch of mechanics he was a most laborious experimentalist, and he obtained many valuable results, many of which are incorporated in the work before us. In this first volume of his work the discharge of water is very fully treated. Unfortunately, however, his so-called theoretical formulas belong to the same category as those which Prof. James Thomson of Glasgow showed in his paper, read before the last meeting of the British Association, to be founded on assumptions which are not in accordance with known hydrodynamic principles.

On page 851 of this translation there is a formula to which we would draw attention. It is taken from "The Lowell Hydraulic Experiments," by J. B. Francis, and is for the discharge of water over a weir. This formula is "$Q = 3·33\ (l - 0·1\ nh)h^{\frac{3}{2}}$ English cubic feet, in which h denotes the head of water above the sill of the weir, l its length, and n either 0 or 1 or 2, according as the contraction of the vein is prevented upon both, one, or none of the sides." Prof. Thomson, in the above-mentioned paper, referred to this formula as identical, in its general form $a\,(l - \beta\, n\, h)h^{\frac{3}{2}}$, with the one which he had deduced from known principles as the true theoretical formula. Mr. Francis put it forward merely as an empirical formula which agreed with the results of his experiments, and it is curious that he should have made a guess which turned out to be more in accordance with the true theory, than all the previous so-called theoretical formulas, which had been advanced and sanctioned by the best authorities.

PATRICK EDWARD DOVE

OUR BOOK SHELF

A General Dictionary of Geography, Descriptive, Physical, Statistical, Historical; forming a Complete Gazetteer of the World. By A. Keith Johnston, F.R.S.E. New edition, thoroughly revised. (London: Longmans and Co., 1877.)

THE title of this work is somewhat misleading. The "physical" and "historical" elements are so meagre that they are scarcely worth mentioning as features of the work. To call this a "*complete* gazetteer of the world" is a misuse of the term "complete;" *incomplete* would have been more accurate. Even on the scale of the present work it would take a gazetteer at least three times its size to contain anything like a register of all the places one would naturally expect to find in a "complete" gazetteer. The work includes a selection of the more important places in the world, very few towns, for example, out of the United Kingdom being given, whose population is under 1,000. We find no fault with the publication of a selective gazetteer, but it should not pretend to be more than it is. When compared with Ritter's well-known work, *e.g.*, the proportion of places found in the latter as compared with "Johnston" is something like five to one. We believe a service would be done to the public by the issue of a gazetteer containing simply all the names omitted in "Johnston." It is not for well-known places we turn up a gazetteer, but for names that one seldom hears. During these Eastern troubles, how many names of places not to be found in "Johnston" have become of great importance, and during the war just begun how many more are likely to come prominently into notice? On the other hand, much valuable space is occupied with catalogues of streets and public buildings in the articles devoted to well-known places like London, Edinburgh, Paris, Vienna, &c. All that can be said about public buildings and similar features of a town in a gazetteer of this scale is practically useless; the space would be used to much better purpose by an enlargement of the list of names. In Russia, for example, nearly all "towns" and "villages" seem to be omitted, many of them with thousands of inhabitants, only "district towns," as a rule, being given. Poland and Finland are also very unsatisfactory; in fact these countries have never been properly "gazetteered" even in Russia. In several instances the "latest" information has evidently not been obtained. To get it, indeed, would involve a vast amount of research among official publications and travellers' narratives, but in a standard work such research is demanded. In Switzerland, we are informed by a Swiss friend, much of the information is half a century behind date. Under Chaux-de-Fonds, *e.g.*, the statement with regard to the manufacture of chains for the movements of watches has not been true for at least thirty years; and there is no lace now made at St. Imier. To arrange the wealth of information published by the United States Survey alone would involve much time and labour; we fear that for the new edition this has not been thoroughly done. Nearly two years ago Mr. W. H. Dall, of the United States Coast Survey, published a Report on the mountains in the Alaska territory. Yet no use has been made of this Report though it is quite accessible. For Mount St. Elias the height in the English Admiralty Chart, 14,970 feet is given, instead of upwards of 19,000 feet, obtained by the careful measurement of the United States Survey in 1874. The height of Mount Fairweather is set down as 14,708 (1855) instead of 15,500 (1874); Mount Crillon 13,500 instead of 15,900; Mount Cook 16,000, Mount La Perouse 11,300, and Mount Vancouver 13,100 feet, are not given. Such imperfections make one doubt if this new edition has been "thoroughly revised." It is easy to give information contained in census tables and in other gazetteers and guide-books, but even a work on the limited scale of the present cannot be made throughout trustworthy without very considerable trouble being taken.

Zoological Classification. By F. C. Pascoe, F.L.S. (John Van Voorst, 1877.)

THIS small work will be found particularly serviceable to many working naturalists. It is a concise compilation of the sub-kingdoms, classes, and orders of the animal kingdom, with lists of the families and most important genera. Specialists will be able to find fault with some of the details in many cases, nevertheless we know no volume which, in the space, contains so much reliable information. The larger groups are all succinctly defined, with many of the most modern views incorporated; and these definitions extend to the orders. Taking the mammalia for criticism, we regret to find the Sirenia included with the Cetacea, the Musk Deer with the Chevrotains, the Peccaries with the true Swine, and the Camels between the Giraffe and the other typical ruminating animals. The caecum is not "enormous" in Hyrax. "Whatever gaps there may be at the present day" between the Perrissodactyla and Artiodactyla "are *not* nearly all filled in by numerous extinct forms." Such errors may be found in many places; they do not, however, much detract from the general value of the work, which will be found more valuable as a basis for annotation, than a book of reference. There is a very complete index we are glad to say.

Tracts relating to the Modern Higher Mathematics. Tract No. 2, Trilinear Coordinates. By Rev. W. Wright, Ph.D. 77 pp. (London: Messrs. C. F. Hodgson and Son, 1877.)

DR. WRIGHT is, or was until quite recently, Professor of Mathematics at Wilson College, Pennsylvania. His object is to make his countrymen acquainted with certain branches of modern mathematics, and we learn that his first venture (Tract No. 1, *Determinants*) has met with considerable acceptance in the American universities. M. Hermite, too, has expressed himself well pleased with the author's standpoint, "Une grande transformation s'est déjà faite et continue encore de se faire dans le domaine de l'analyse; des voies nouvelles plus fécondes, et je crois aussi plus faciles, ont été ouvertes, et c'est l'œuvre de ceux qui veulent servir la science et leur pays de discerner ce que les éléments peuvent recevoir de l'immense élaboration qui s'est accomplie depuis Gauss jusqu'à Riemann."

There is in the present tract a clear exposition of the elementary applications of Trilinear and Triangular Coordinates, and just a passing glance at Polar reciprocals.

In such a work we do not look for anything original, but for clearness and correctness. These ends, we think, have been attained, and we wish Dr. Wright health and leisure to enable him to carry out his design.

Grundriss der chemischen Technologie. Von Dr. Jul. Post. (Berlin: Robert Oppenheim, 1877.)

DR. POST, who is known to chemists as an able Privat Docent at the University of Göttingen, has, like many other teachers, felt the great necessity of a manual of chemical technology, suited to the requirements of students who desire a general training in that branch of applied chemistry. A considerable number of excellent treatises, as, for example, those of Knapp, Wagner, Bolley, Kerl, and Stohmann, already exist in German, and some of them have been translated into English, but no one of them is exactly adapted to the class-room. Their excellence consists in their completeness as works of reference; indeed as such they may be said to be invaluable to the chemical manufacturer; but the mode of their arrangement renders them of comparatively little value as aids to systematic study. Dr. Post has succeeded in producing a work which, within the compass of some of our smaller chemical manuals presents a complete outline of the present position of chemical technology. His book thus serves as a fitting introduction to the larger and more special treatises above mentioned.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Colour-Sense in Birds

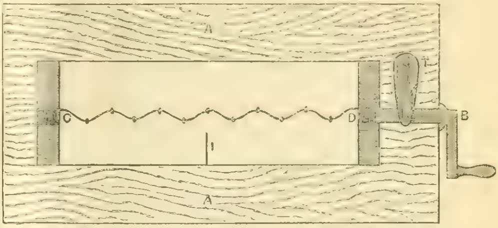
I HAVE been lately watching, with great delight, two goldfinches building their nest. They placed it nearly at the end of an outside branch of a young sycamore tree, so that there was nothing but sky above it, and the gravel path below. The window from which I observed them, being never opened, and well covered with flowers in pots and a blind, seems to have caused them no alarm, although not more than two yards distant from them; and their object appears to have been to make their nest invisible from below. To this end they chose their building materials with such skill and such colour-matching power that if one had not seen the nest built it would be quite impossible to discover it; to match the tree they took its long flexible blossoms, and to match the sky the equally long and flexible stalks and flowers of the garden forget-me-not, of which a bed was close at hand in full bloom. I watched them carefully, and, as far as I could see, they used no other materials than these flowers, though I saw one of them *attempting* to get the dirty-white cotton tie off a budded rose-tree. At all events the nest was mainly built of them. The blue of the forget-me-not has of course faded, but the general effect from below is that of a scarcely visible grey-green thickening of one of the bunches of sycamore leaves. They seemed to enjoy flinging their flower-wreaths about. And that leads to the question whether birds—who are in many ways like children—do not often out of mere playfulness and love of colour, pull to pieces yellow crocuses and other bright flowers. While my pen is in my hand I may mention, with reference to Dr. Muirhead's communication on the subject of noise causing a sensation of colour, that I have frequently observed whilst tuning a harp, that the sudden breaking of a string will cause a curious *taste* and sensation in the mouth, like that produced by a piece of silver and one of zinc placed above and beneath the tongue, when they are made to meet.

J.

A Simple Wave-Motion Apparatus

IT has been suggested to me that I should publish a description of a simple and portable wave-motion apparatus, devised by me a year or two ago, which has given satisfactory results to others as well as to myself. I therefore send the description.

In the figure A A represents an ordinary wooden lantern slide, with a rectangular aperture, which may vary in size according to the size of the lantern condensers, the sketch being half size for 4-inch condensers. A small winch, B, is fitted into the slide at one end of the aperture, and held in its place by the tongue, T. The spindle, B D, is milled or otherwise roughened near the end, D. A brass stud similar in shape to the milled end of the spindle, but *smooth* and slightly smaller, is fixed in the opposite side of the aperture at C. A helix of 25-gauge hard brass wire is wound on a spit of the same size as the smooth stud, taking care to wind the coils close together; about fifteen turns of the helix are cut off, and the middle five turns drawn out till they form a perfect wave similar to the figure when held up to the light. The length of the helix should then be the same as C D. One end of the helix is pushed tight on the milled end, D, and the other end is slipped loosely over the stud, C, so as to work on it like a swivel, to keep the end of the helix true when the winch is turned. A little bead of wax is melted on each crest and hollow of the wave to represent particles, and the essential parts of the apparatus are complete. On placing it in front of the lantern, and focussing, a distinct and striking image of a moving wave with its vibrating particles is produced by turning the winch.



If the helix is not perfectly straight the image of the wave will rise up and down more or less as a whole; the helix should then be straightened or "set" with the fingers till true. When once set thin glass plates may be placed on each side to protect it from injury. An index, I, of wire, may be fixed so as to give a means of proving that the particles *only* move up and down.

A modification I have tried by using a dark wire with bright silver beads, on a velvet back-ground in the aphengescope, is more difficult to make and use. I therefore prefer the apparatus as sketched above.

Of course the amount of finish depends on the taste of the user, &c. A pasteboard frame instead of mahogany, a wire bent twice at right angles instead of a finished brass winch, and tied to the frame by two bits of wire instead of let in, &c., may be used, thus reducing the cost to a few halfpence.

In use it will recommend itself. W. JESSE LOVETT

Birmingham

Atmospheric Currents

A CONTROVERSY was recently waged in your columns as to the course which is pursued by the hot water-laden air of the equatorial regions in its journey to the poles. Both combatants seem to adopt what I may call the sheet-theory, which regards the winds as moving in sheets or strata, and gliding over and under each other at the polar and equatorial sides of the calms of Cancer and Capricorn, a process which would inevitably result either in both opposing winds being torn to tatters, or in their commixture and neutralisation. Surely the truth is that like all other moving fluids, the air will seek equilibrium in the direction of least resistance, and will carve out for itself wide channels in accordance with local conditions from the poles to the equator, and from the equator to the poles—channels which will not intersect or interfere with one another, except when affected by disturbing causes.

One possible cause of change in this direction of least resistance or normal channel in the case of the south-west wind of these latitudes may possibly be a shifting of the thermal pole. Suppose, for instance, we have any reason to surmise that the centre of greatest cold is now on the American side of the true North Pole and at another time on the Asiatic side, we have at once a satisfactory explanation of observed variations in the prevalent direction of the main channels of the water-laden winds of the northern hemisphere.

I will now as briefly as possible state my reasons for suspecting that such is actually the case.

Since 1873 the south-west winds have prevailed very considerably over the average in Europe, and as a natural consequence we have had continued floods all over the west of this continent. In Asia, during the same period the water-laden winds have been fearfully under the average, the rainfall during the last three years having been about nine inches below the average of the previous half-dozen. Famines, of course, have been the result.

If my theory is correct we should expect to find that the thermal pole has been situated during the last three or four years on our own side of the North Pole.

Now in 1872 Capt. Hall, of the *Polaris*, saw unmistakable signs of an open polar sea where Capt. Nares, in 1875, saw nothing but a vast wilderness of ancient ice. In a former letter of mine which appeared in NATURE (vol. xv. p. 116) I attempted to reconcile these apparently conflicting observations on the supposition that this palæocrystic wilderness is in reality a vast floating island of ice some hundreds of miles in diameter. I called to mind Sir E. Parry's disappointing experiences in 1827 in the *Hecla*, when, after a toilsome journey northwards on what he believed to be the main pack, he found he was after all drifting southwards; whereupon he concluded his supposed main pack must be a loose floe of immense extent.

Is it not equally probable, to say the least of it, that he was on the main pack—on the palæocrystic island—and that he caught it on the move towards our shores of the Arctic Sea?

Be this as it may (and it is merely a suggestion) it is certain that five years later occurred the terrible famine of 1832 in India, and five years is just the time required, according to Dr. Hunter, for the effects of the proximate cause of drought (whatever that may be) to attain its maximum, according to the law of the "multiplication of effects."

Although I have examined the records of the winds at the Meteorological Office, I will add nothing more, as I fear I have already exceeded my proper limits.

WORDSWORTH DONISTHORPE

Yellow Crocuses

A LETTER in NATURE (vol. xvi. p. 43) calls attention to the destruction of the flowers of the yellow crocus by the sparrow. I have for many years been a cultivator of the crocus, both yellow, white, and purple; this spring they flowed abundantly, the white and purple blooming undisturbed, the yellow picked and torn. My gardener and I talked the matter over but could find no solution of the problem. As this has been my experience in former years, and the fact is now corroborated by general experience, can no naturalist discover the reason, or must it still be left a secret in the bosom of pert little *Fringilla domestica?*

A. H.

Complementary Colours

IN connection with this subject, which was referred to in Mr. Terrill's letter in NATURE for May 17, perhaps the following homely way of illustrating the fact that the combination of two complementary colours produces white may interest your readers. If a tumbler of beer be held in front of the green glass shade of an ordinary reading lamp, it will be found on looking through the beer at the shade that the tumbler appears to be filled with an almost colourless liquid.

J. ROMILLY ALLEN

Chromatic Aberration of the Eye

THERE is a slight inaccuracy in your report of my communication of May 12th to the Physical Society, wherein I am made to affirm that a blue object and a red object cannot both be in focus at once unless the blue object be the more distant. The next sentence of your report, and indeed the whole tenor of my communication imply the reverse condition, that the blue rays should come from the *less* distant source. The dispersion of the eye takes place in the same sense as its refraction; hence the adjustment of the eye to focus may be the same for blue rays proceeding from a body near the eye as for red rays proceeding from an indefinitely distant luminous source; as, indeed, Fraunhofer proved half a century ago.

S. P. THOMPSON

University College, Bristol, May 25

A Correction

PERMIT me to explain that the subject of my note, read at the last meeting of the Astronomical Society, was not my chart of 324,912 stars, though I had occasion in the course of it to mention that chart. My note referred in reality to a paper read at the preceding meeting, and relating to the general subject of the distribution of stars in space.

RICHARD A. PROCTOR

DR. PHILIP P. CARPENTER

WE regret to announce the death at Montreal, in his fifty-eighth year, of Dr. Philip P. Carpenter, formerly of Warrington, one of the most scientific conchologists of our time. Taking up this pursuit, in the first instance, merely as a recreative occupation, he was led by his friend, Dr. J. E. Gray, who saw his remarkable aptitude for it, to make it one of the principal objects of his life; and he brought to it a mind trained in those scientific habits which prevented him from ever becoming the mere species-monger, whilst specially delighting in that study of minute detail which is required for the true determination of specific types and their geographical distribution. It was well observed by Dr. Hooker, in his introductory essay to the "Flora of New Zealand," that "a wider range of knowledge and a greater depth of study are required to prove those dissimilar forms to be identical, which any superficial observer can separate by words and a name;" and this wide range of knowledge and thoroughness of research were the essential characteristics of all Dr. P. P. Carpenter's conchological work. The opportunity having occurred to him more than twenty-five years ago, while residing at Warrington, of studying a large collection of shells formed at Mazatlan, in California—after Mr. Cuming had selected from it what he considered the new specific types, which he caused to be described by Mr. C. B. Adams—Dr. P. P. Carpenter was impressed with the fact that Mr. Cuming had left behind him those *intermediate* forms, the study of which would prove that many of his supposed species are mere varieties; and having brought the importance of such study before the Zoological Section of the British Association, he was requested to prepare a report on the present state of our knowledge with regard to the mollusca of the west coast of North America, which was published in the *Transactions* of the Association for 1856, and at once took rank as a most able and conscientious work. A Supplementary Report on this subject, marked by the same "wide range of knowledge and depth of study," was published in 1863. Besides these, several monographs, prepared by Dr. P. P. Carpenter on particular groups of shells in the Cumingian Museum, were published in the Zoological *Proceedings*. So high was the reputation which his Reports acquired for him among American naturalists that he was invited by Prof. Henry of the Smithsonian Institution at Washington to assist him in the arrangement of its national collection of shells; and having been led in 1865 to take up his residence in Montreal, he was subsequently engaged in similar work for other museums in the Northern States. He soon acquired in the city of his adoption the character he had left behind him in Warrington, of being ever ready for any kind of philanthropic labour; and especially distinguished himself by his untiring advocacy, through evil as well as good report, of the sanitary reforms which he saw to be greatly needed. There is reason to believe that the typhoid fever which brought his useful life to a close was engendered in the foul air of the building in which he was accustomed to carry on his scientific work.

KOENIG'S TUNING-FORKS AND THE FRENCH "DIAPASON NORMAL"

HAVING had occasion to measure a series of Koenig's tuning-forks, kindly lent me for that purpose by Professors Tyndall and Guthrie, by means of Appunn's tonometer, now in the South Kensington Museum, I was much struck, and for a time puzzled, by finding that though the forks were perfectly consistent with each other, they did not answer to their names, that is, the numbers of single vibrations marked on them, did not answer at all to the double vibrations measured by Appunn's tonometer. The workmanship of Koenig's forks is so good and the intervals between them so exact, that one might be at first inclined to suspect the accuracy of the absolute numbers of the reeds on Appunn's instrument. But there can be no doubt of the accuracy of the differences of the number of vibrations between any reed and any other, for these admit of ready control by counting, and I have counted them all. Hence such a thing as this is quite certain. The difference of the vibrations of C 256 and G 384 is 128 vibrations, as on Appunn's instrument; but the difference in the vibrations of Koenig's corresponding forks is 129·2 vibrations. Now, I have no doubt about the perfection of Koenig's fifth C to G. If then his C make x vibrations, his G makes $x + 129{\cdot}2$ vibrations. Putting this $= \frac{3}{2}x$, we obtain $x = 258{\cdot}4$ as the number of the vibrations of his C, and this is the precise number furnished by Appunn's tonometer, according to a very careful measurement made by Mr. A. J. Hipkins, of Messrs. Broadwoods, who has had great experience in counting beats, and myself. The discrepancy, therefore, becomes an excellent proof of the perfection of Appunn's instrument. But how could Koenig have hit on this strange number 258·4, in lieu of 256? It was some littte time before the solution presented itself to my mind, but I believe that this, which is decidedly sufficient, will prove substantially correct.

The French normal A was settled at 435, and since Lissajous superintended the publication of the fork of the French Commission in 1859, the whole world has accepted that fork as having exactly 435 vibrations. Now the French Commission gave to Messrs Broadwoods, in return for their courtesy in sending them their forks, an authorised copy of this fork, stamped with their stamp (a lyre between D and N, at the end of each prong) and made by Secretan. This fork I assume to be an authentic representative of the French diapason normal, made at the time. I have examined many others made by Secretan, and also officially stamped, and one by Koenig, and they mostly agree within two- or three-tenths of a vibration in a second. Two of Secretan's, however—one bought in Paris by the Society of Arts, and one sent to that society officially in 1869 through the Foreign Office, as representing the French pitch used in the Grand Duchy of Baden, differ as much as six-tenths of a vibration, the extreme difference observed in authorised forks. Other copies differ as much as two vibrations. But I take as my standard the copy given to Messrs. Broadwoods (which through the kindness of Mr. Hipkins I have carefully measured), and the one made by Koenig (which Dr. W. H. Stone was so obliging as to allow me to measure). These differ only by *one*-tenth of a vibration, and that tenth may be my own fault in counting. All these forks show that the real French diapason normal is A 439, that is, four vibrations sharper than was supposed. This is really a result of prime importance as brought out by Appunn's instrument, and it fully accounts for Koenig's differences as follows:—

Koenig having to make a C 256, observed (I suppose) that a major sixth above it would be A $426\frac{2}{3} = \frac{5}{3} \times 256$, and that this would beat $8\frac{1}{3}$ times in a second with A 435, which he assumed to be given by his diapason normal. Constructing such a fork by beats, which is easy enough, he necessarily obtained one exactly four vibrations too sharp, that is, A $430\frac{2}{3}$. From this, by the Lissajous figures most probably (certainly *not* by interposing new forks and counting the beats, for that would have shown him his error), he obtained first the correct major sixth below it, C $258{\cdot}4 = \frac{3}{5} \times 430\frac{2}{3}$, and then got his other forks by true intervals obtained also by Lissajous' figures. This makes all Koenig's forks harmonics of C 64·6, instead of C 64, as he intended and as he marks his forks.

Since Koenig's forks are extensively used by physicists, and also for the purpose of obtaining other pitches from them either for musical or for counting purposes, I think it will be convenient to add a little table of the harmonics of 64·6, with the marks on the forks observed (all of which had Koenig's monogram) and the pitches as actually measured by Appunn's tonometer. I am quite willing to allow the small differences to be set down to my bad counting and not to defective workmanship of either Koenig or Appunn.

No. of Harmonic.	Double Vibrations of Harmonic.	Marks on Koenig's Forks, V.S.	Meaning in English Notation.	Measured by Appunn's Tonometer.
1	64·6	—	—	—
2	129·2	—	—	—
3	193·8	—	—	—
4	258·4	Ut_3 512	C 256	258·4
5	323·0	Mi_3 640	E 320	323·1
6	387·6	Sol_3 768	G 384	387·6
7	452·2	—	—	—
8	516·8	Ut_4 1024	C 512	516·7
9	581·4	—	—	—
10	646·0	Mi_4 1280	E 640	646·0
11	710·6	—	—	—
12	775·2	—	—	—
13	839·8	—	—	—
14	904·4	7. 1792	B *flat* 896	905·0
15	969·0	—	—	—
16	1033·6	Ut_5 2048	C 1024	1033·6
17	1098·2	—	—	—
18	1162·8	Re_5 2304	D 1152	1163·3
19	1227·4	—	—	—
20	1292·0	Mi_5 2560	E 1280	1292·0

The last four forks were difficult to measure. Those left blank were not found either at the Royal Institution or the School of Mines. The 11th, 13th, 17th, and 19th harmonics would not be easy to tune by Lissajous' figures. Appunn's instrument gives them with perfect ease. I may observe here with regard to the question of forks and reeds, that though forks may be the most permanent and portable records of pitch, reeds have a great advantage in the number of their upper partial tones, which allow of an extraordinary variety of verifications without any assistance beyond the instrument itself. I have found this reed tonometer easily checked and invaluable in measurements. But the above table would allow any tuning-fork maker to tune exactly to C 256, or from C 254 to C 265, by means of beats from Koenig's forks.

In trying forks to-day at King's College, Prof. Adams drew my attention to the fact that Koenig's organ-pipes are much flatter than his forks. On account of the difficulty of getting a steady blast on the organ pipes, it was not possible to measure them satisfactorily by Appunn's tonometer (a copy of which is in the physical laboratory there, and should be in all physical laboratories, as it is the best instrument for illustrating the nature of sound, partials, beats, and chords that I have yet seen), but they seemed to give very nearly C 250, about eight vibrations flatter than the forks. I cannot account for this, as this would be about Koenig's 248. ALEXANDER J. ELLIS

Kensington, May 18

HOW TO DRAW A STRAIGHT LINE[1]

II.

IN Fig. 6, Q C is the extra link pivoted to the fixed point Q, the other pivot on it C, describing the circle O C R. The straight lines P M and P′ M′ are supposed to be perpendicular to M R Q O M′.

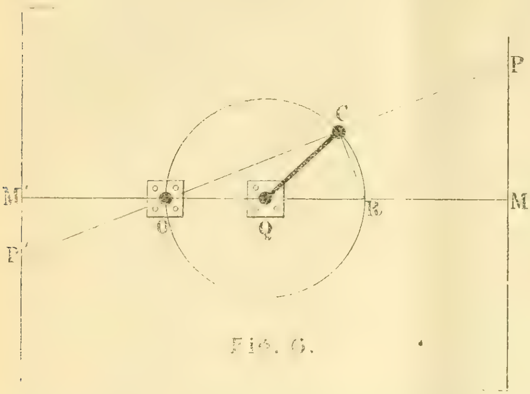


Fig. 6.

Now the angle O C R, being the angle in a semicircle, is a right angle. Therefore the triangles O C R, O M P are similar. Therefore,

$$OC : OR :: OM : OP.$$

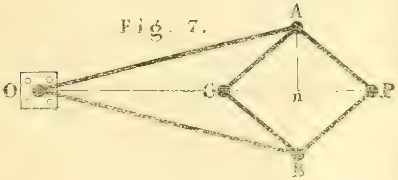
Therefore,

$$OC \cdot OP = OM \cdot OR.$$

wherever C may be on the circle. That is, since O M and O R are both constant, if while C moves in a circle P moves so that O, C, P are always in the same straight line, and so that O C · O P is always constant; then P will describe the straight line P M perpendicular to the line O Q.

It is also clear that if we take the point P′ on the other side of O, and if O C · O P′ is constant P′ will describe the straight line P′ M′. This will be seen presently to be important.

Fig. 7.



Now, turning to Fig. 7, which is a skeleton drawing of the Peaucellier cell, we see that from the symmetry of the construction of the cell, O, C, P, all lie in the same straight line, and if the straight line A n be drawn perpendicular to C P—it must still be an imaginary one, as we have not proved yet that our apparatus does draw a straight line—C n is equal to n P.

Now,

$$OA^2 = On^2 + An^2$$
$$AP^2 = Pn^2 + An^2$$

therefore,

$$OA^2 - AP^2 = On^2 - Pn^2$$
$$= [On - Pn]\cdot[On + Pn]$$
$$= OC \cdot OP.$$

Thus since O A and A P are both constant O C · O P is always constant, however far or near C and P may be to O. If then the pivot O be fixed to the point O in Fig. 6, and the pivot C be made to describe the circle in the figure by being pivoted to the end of the extra link, the pivot P will satisfy all the conditions necessary to make it move in a straight line, and if a pencil be fixed at P it will draw a straight line. The distance of the line from the fixed pivots will of course depend on the magnitude of the quantity $OA^2 - OP^2$ which may be varied at pleasure.

I hope you clearly understand the two elements composing the apparatus, the extra link and the cell, and the part each plays, as I now wish to describe to you some modifications of the cell. The extra link will remain the same as before, and it is only the cell which will undergo alteration.

If I take the two linkages in Fig. 8, which are known as the "kite" and the "spear-head," and place one on the other so that the long links of the one coincide with those of the other, and then amalgamate the coincident long links together, we shall get the original cell of Figs. 5 and 7. If then we keep the angles between the long links, or that between the short links, the same in the "kite" and "spear-head," we see that the height of the "kite" multiplied by that of the "spear-head" is constant.

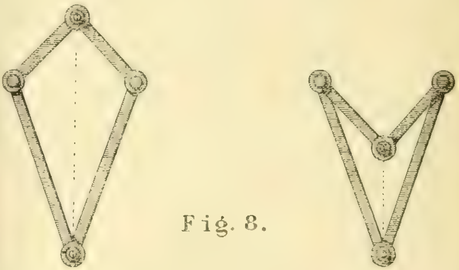


Fig. 8.

Let us now, instead of amalgamating the long links of the two linkages, amalgamate the short ones. We then get the linkage of Fig. 9; and if the pivot where the short links meet is fixed, and one of the other free pivots be made to move in the circle of Fig. 6 by the extra link, the other will describe, not the straight line P M, but the straight line P′ M′. In this form, which is a very compact one, the motion has been applied in a beautiful manner

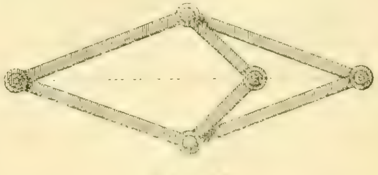


Fig. 9.

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[1] Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A. Continued from p. 67

to the air engines which are employed to ventilate the Houses of Parliament. The ease of working and absence of friction and noise is very remarkable. The engines were constructed and the Peaucellier apparatus adapted to them by Mr. Prim, the engineer to the Houses, by whose courtesy I have been enabled to see them, and I can assure you that they well worth a visit.

Another modification of the cell is shown in Fig. 10. If instead of employing a "kite" and "spear-head" of the same dimensions I take the same "kite" as before but use a "spear head" of half the size of the former one, the angles being, however, kept the same, the product of the heights of the two figures will be half what it was before, but still constant. Now instead of superimposing the links of one figure on the other, it will be seen that in Fig. 10 I fasten the shorter links of each figure together end to end. Then as in the former cases, if I fix the pivot at the point where the links are fixed together, I get a cell which may be used by the employment of an extra link, to describe a straight line. A model employing this form of cell is exhibited in the Loan Collection by the Conservatoire des Arts et Métiers of Paris, and is of exquisite workmanship; the pencil seems to swim along the straight line.

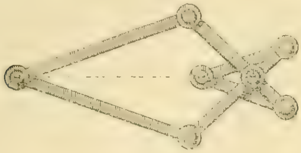


Fig. 10.

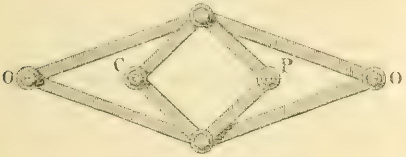


Fig. 11.

M. Peaucellier's discovery was introduced into England by Prof. Sylvester in a lecture he delivered at the Royal Institution in January, 1874, which excited very great interest and was the commencement of the consideration of the subject of linkages in this country.

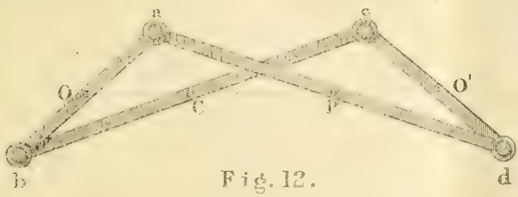


Fig. 12.

In August of the same year Mr. Hart, of Woolwich Academy, read a paper at the British Association meeting, in which he showed that M. Peaucellier's cell could be replaced by an apparatus containing only four links instead of six. The new linkage is arrived at thus.

If to the ordinary Peaucellier cell I add two fresh links of the same length as the long ones I get the double, or rather quadruple cell, for it may be used in four different ways, shown in Fig. 11. Now Mr. Hart found that if he took an ordinary parallelogramatic linkwork in which the adjacent sides are unequal, and crossed the links so as to form what is called a contra-parallelogram, Fig. 12, and then took four points on the four links dividing the distances between the pivots in the same proportion, those four points had exactly the same properties as the four points of the double cell. That the four points always lie in a straight line is seen thus: considering the triangle abd, since $aO : Ob :: aP : Pd$ therefore OP is parallel to bd and the perpendicular distance between the parallels is to the height of the triangle abd as Ob is to ab; the same reasoning applies to the straight line CO', and since $ab : Ob :: cd : O'd$ and the heights of the triangles abd, cbd, are clearly the same, therefore the distances of OP and $O'C$ from bd are the same, and $OCPO'$ lie in the same straight line.

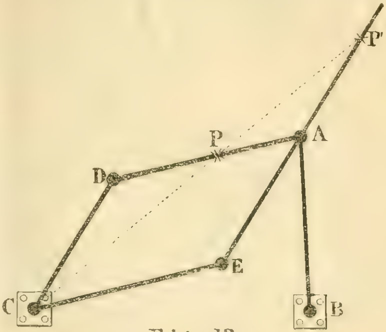


Fig. 13.

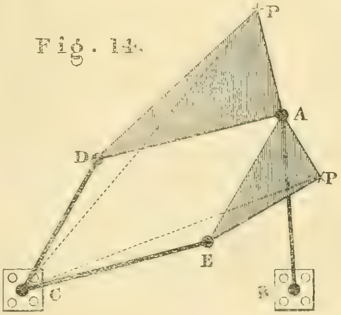


Fig. 14.

That the product $OC \cdot OP$ is constant appears at once when it is seen that ObC is half a "spear head" and OaP half a "kite;" similarly it may be shown that $O'P \cdot O'C$ is constant, as also $OC \cdot CO'$ and $OP \cdot PO'$. Employing then the Hart's cell as we employed Peaucellier's, we get a five-link straight line motion. A model of this is exhibited in the Loan Collection by M. Breguet.

I now wish to call your attention to an extension of Mr. Hart's apparatus, which was discovered simultaneously by Prof. Sylvester and myself. In Mr. Hart's apparatus we were only concerned with bars and points on those bars, but in the apparatus I wish to bring before you we have pieces instead of bars. I think it will be more interesting if I lead up to this apparatus by detailing to you its history, especially as I shall thereby be enabled to bring before you another very elegant and very important linkage—the discovery of Prof. Sylvester.

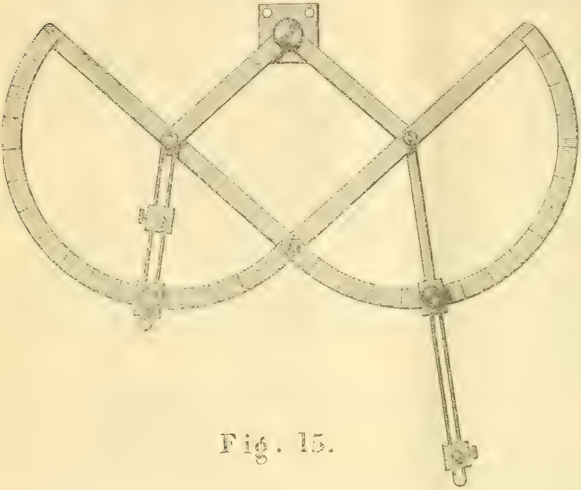


Fig. 15.

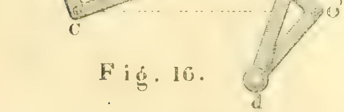


Fig. 16.

When considering the problem presented by the ordinary three-*bar* motion consisting of two radial bars and a traversing bar, it occurred to me—I do not know how or why, it is often very difficult to go back and find whence one's ideas originate—to consider the relation between the curves described by the points on the traversing bar in any given three-bar motion, and those described by the points on a similar three-bar motion, but in which the traversing bar and one of the radial bars had been made to change places. The proposition was no sooner stated than the solution became obvious; the curves were precisely similar. In Fig. 13 let C D and B A be the two radial bars turning about the fixed centres C and B, and let D A be the traversing bar, and let P be any point on it describing a curve depending on the lengths of A B, B C, C D, and D A. Now add to the three-bar motion the bars C E and E A P′, C E being equal to D A, and E A equal to C D. C D A E is then a parallelogram, and if an imaginary line C P P′ be drawn, cutting E A produced in P′ it will at once be seen that P′ is a fixed point on E A produced, and C P′ bears always a fixed proportion to C P, viz., C D : C E. Thus the curve described by P′ is precisely the same as that described by P, only it is larger in the proportion C E : C D. Thus if we take away the bars C D and D A, we shall get a three-bar linkwork, describing precisely the same curves, only of different magnitude, as our first three-bar motion described, and this new three-bar linkwork is the same as the old with the radial link C D and the traversing link D A interchanged.

On my communicating this result to Prof. Sylvester, he at once saw that the property was one not confined to the particular case of points lying on the traversing bar, in fact to three-*bar* motion, but was possessed by three-*piece* motion. In Fig. 14 C D A B is a three-bar motion, as in Fig. 13, but the tracing point or "graph" does not lie on the line joining the joints A D, but is anywhere else on a "piece" on which the joints A D lie. Now, as before, add the bar C E, C E being equal to A D, and the piece A E P′, making A E equal to C D, and the triangle A E P′ similar to the triangle P D A; so that the angles A E P′, A D P are equal, and

P′E : E A : : A D : D P.

It follows easily from this—you can work it out for yourselves without difficulty—that the ratio P′ C : P C is constant and the angle P C P′ is constant; thus the paths of P and P′, or the "grams" described by the "graphs," P and P′ are similar, only they are of different sizes, and one is turned through an angle with respect to the other.

Now you will observe that the two proofs I have given are quite independent of the bar A B, which only affects the particular curve described by P and P′. If we get rid of A B, in both cases we shall get in the first figure the ordinary pantagraph, and in the second a beautiful extension of it called by Prof. Sylvester, its inventor, the *Plagiograph* or *Skew Pantagraph*. Like the pantagraph, it will enlarge or reduce figures, but it will do more, it will turn them through any required angle, for by properly choosing the position of P and P′, the ratio of C P to C P′ can be made what we please, and also the angle P C P′ can be made to have any required value. If the angle P C P′ is made equal to 0 or 180°, we get the two forms of the pantagraph now in common use; if it be made to assume successively any value which is a submultiple of 360°, we can, by passing the point P each time over the same pattern, make the point P′ reproduce it round the fixed centre C after the fashion of a kaleidoscope. I think you will see from this that the instrument, which has, as far as I know, never been practically constructed, deserves to be put into the hands of the designer. I give here a picture of a little model of a possible form for the instrument furnished by me to the Loan Collection by request of Prof. Sylvester.

After this discovery of Prof. Sylvester it occurred to him and to me simultaneously—our letters announcing our discovery to each other crossing in the post—that the principle of the plagiograph might be extended to Mr. Hart's contra-parallelogram; and this discovery I shall now proceed to explain to you. I shall, however, be more easily able to do so by approaching it in a different manner to that in which I did when I discovered it.

If we take the contra-parallelogram of Mr. Hart and bend the links at the four points which lie on the same straight line, or *foci*, as they are sometimes termed, through the same angle, the four points, instead of lying in the same straight line, will lie at the four angular points of a parallelogram of constant angles—two the angle that the bars are bent through, and the other two their supplements—and of constant area, so that the product of two adjacent sides is constant.

In Fig. 16 the lettering is preserved as in Fig. 12, so that the way in which the apparatus is formed may be at once seen. The holes are taken in the middle of the links and the bending is through a right angle. The four holes O P O′ C lie at the four corners of a right-angled parallelogram, and the product of any two adjacent sides, as for example O C · O P, is constant. It follows that if O be pivoted to the fixed point O in Fig. 16, and C be pivoted to the extremity of the extra link, P will describe a straight line, not P M, but one inclined to P M at an angle the same as the bars, are bent through, *i.e.*, a right angle. Thus the straight line will be parallel to the line joining the fixed pivots O and Q. This apparatus, which for simplicity I have described as formed of four straight links which are afterwards bent, is of course strictly speaking, formed of four plane links, such as those employed in Fig. 1, on which the various points are taken. This explains the name given to it by Prof. Sylvester, the "Quadruplane." Its properties are not difficult to investigate, and when I point out to you that in Fig. 16 as in Fig. 12, O *b*, *b* C form half a "spear-head," and O *a*, *a* P half a "kite," you will very soon get to the bottom of it.

I cannot leave this apparatus in which my name is associated with that of Prof. Sylvester without expressing my deep gratitude for the kind interest which he took in my researches, and my regret that his departure for America to undertake the post of Professor in the new Johns Hopkins University has deprived me of one whose valuable suggestions and encouragement helped me much in my investigations.

(*To be continued.*)

METEOROLOGICAL NOTES

NOTES OF THE WEATHER IN SCOTLAND, FARÖ, AND ICELAND.—It appears from the meteorological returns for the eight principal towns in Scotland, that the weather of last April has been distinguished by low temperature, 3°·7 under the average, great fluctuations in the barometric pressure, large rainfall distributed among the towns with unusual uniformity, much wind and that more persistently from the eastward than has been before chronicled for any month of any year since the Scottish Meteorological Society was founded. At the monthly meeting of the Edinburgh Botanical Society, held on Thursday, May 10, Mr. McNab stated that the present spring is later than any other during the last twenty-eight years, when systematic observations on the flowering of plants began to be made in the Royal Botanic Garden of Edinburgh. In Farö the winter and early spring have been among the worst ever known, high easterly and northerly winds and snowstorms being very prevalent. In March and April snow fell on no fewer than thirty-one days. The mean barometric pressure at 32° and sea-level at Thorshavn during April, was 29,945 inches. On the other hand the winter, until Easter, was one of the finest ever known in Iceland, particularly in the north of the island. There was little snow, any frost that occurred was of short continuance, and fine calm weather prevailed. But about Easter-day a series of northerly snowstorms began, accompanied by severe frosts, which lasted with little intermission for about a week, causing the loss of many ships, and snowing up the pastures. Since these storms sunshine prevailed up to May 6. When the steamer left Reykjavik the Greenland and Spitzbergen ice had appeared off the northern coasts about the middle of March, but only stray icebergs neared the land, the ice becoming "land-fast" nowhere in any quantity. The season has also been singularly mild in Canada. Spring set in there fully three weeks earlier than usual, and as very little rain had fallen up to the close of April and the thaw was very gentle, the rivers were unusually low for the time of the year.

METEOROLOGY OF HOLLAND.—A highly-important work on the annual march of the thermometer and barometer in Holland, deduced from observations made from 1843 to 1875, has been published by Dr. Buys Ballot, the distinguished director of the meteorological system of that country. The monthly means for each meteorological lustrum of five years, as well as for the whole period during which the observations have been made, are given for each of the hours of observations. From these general averages, the normal values for each of the ten stations have been determined by the process of differentiation, so that the normals are substantially the averages which would have been obtained if the observations at each of the stations had been made during precisely the same terms of years. The normals are calculated for very extended periods by a comparison of the results arrived at for the Dutch stations, with the long averages for Copenhagen, Paris, and Greenwich. Thermometric and barometric normals have also been determined for each day of the year for all the stations, which cannot but prove to be of considerable value in framing forecasts of the weather and in some other practical matters. We hope, however, that Dr. Buys Ballot may be enabled soon to calculate the arithmetic means of the temperature of each day of the year at all his stations, and thus complete what must be regarded as an able and exhaustive discussion of the two most important elements of the meteorology of Holland. Tables are also added, showing the mean temperature of each month during the whole period of observations, and an exceedingly valuable table of the monthly mean barometric measure at Maestricht for sixty-nine years, beginning with 1807 and ending with 1875.

TYCHO BRAHE'S METEOROLOGICAL JOURNAL.—The Royal Academy of Sciences and Letters of Copenhagen has laid scientific men generally under a debt of gratitude in publishing *in extenso*, the Meteorological Journal, kept at Uraniborg in the Isle of Hveen by Tycho Brahe from 1582 to 1597. To the journal is appended a clear and interesting *résumé* of the observations by M. Paul la Cour. The results of the observations made on clouds, rain, snow, hail, fog, winds, frost, thunder, halos, and auroras, by the celebrated astronomer nearly 300 years ago, are compared with similar observations made at Copenhagen and other stations in Denmark in recent years. The results of the different sets of observations are fairly accordant when the different positions and times of observing are taken into consideration. The most noteworthy difference is in the monthly curve of thunder, the maximum at Hveen being strongly pronounced in June, whereas the recent observations at fourteen stations in different parts of Denmark have the maximum extending equally over June, July, and August —a difference perhaps due to a different seasonal distribution of thunder in different parts of Denmark. Of the seventy-eight auroras which were noted by Tycho Brahe, seventy-six occurred during the ten years from 1582-1591, and only two during the six years immediately following. From the detailed descriptions given of certain auroras and auroral arches, M. Paul la Cour concludes that the magnetic inclination at this observatory during 1584 was somewhere between 72° 25′, and 73° 25′.

"ATLAS MÉTÉOROLOGIQUE" OF THE OBSERVATOY OF PARIS, 1875.—A rapid glance through the *Atlas Météoro-*

logique for 1875 is enough to show that it more than sustains the high character of the publications of previous years. The thunderstorms and hailstorms of France for 1875 are elaborately and ably discussed, and to these discussions are appended no fewer than twenty-six memoirs on different meteorological subjects, by such well-known meteorologists as the two Becquerels, R. P. Denza, Brault, Crova, Moritz, Belgrande, Lemoine, Raulin, Coumbary, Brito-Capello, and Fron. Several of the more important of these memoirs not yet noticed by us, we shall bring before our readers on an early occasion, particularly those dealing with the climatology of Asia Minor and of Portugal, and with the rainfall of Algiers.

WEATHER MAPS IN AUSTRALIA.—Mr. Russell, of the Sydney Observatory, began a few months ago to issue daily weather maps for Australia. The plan of preparing the maps, which possesses some novel features, is briefly this :—There is a block of type metal with an electro of the coast-line and mountains fixed on it, and at the position of each station there is a slot in the metal block for the placing of the wind and weather symbols and figures which show the force of the wind, height of the barometer, and the temperature. The sea symbols, arrows, curves, words descriptive of the state of the weather, and in short everything which may be required on the map as varying from day to day, are glued on to the face of the metal block and held so fast that printing from an ordinary letter-press may be begun at once. The whole map is prepared in about two hours, and after a few copies are printed off for the use of the observatory, the block is sent to the *Herald* newspaper and by them stereotyped with their other matter. Copies of several weeks' weather maps, thus prepared and printed, have been forwarded to us. A note in manuscript on the map is sent to the *Herald* every day giving remarks on the weather of that day, and forecasts of coming weather. A system of exchange has been already effected with Melbourne, will soon be completed with Adelaide, and it is expected that all the other colonies will join in the effort to make the system as complete as possible so as to secure for this region of the globe effective warnings of coming storms. The colonial governments will no doubt see that this system of weather telegraphy, so admirable in itself and calculated to be highly beneficial to large public interests, will be furnished with the funds necessary for its efficient maintenance and further development.

STONEYHURST METEOROLOGICAL AND MAGNETICAL OBSERVATIONS, 1876.—This publication maintains the high character of its predecessors for the care and exhaustiveness with which the results are worked out and detailed in each of the monthly reports, and its value is further enhanced by the notes and tables of agricultural and horticultural results which have now been introduced. We are glad to see that observations of cirrus clouds are sent monthly to Dr. Hildebrandsson, of the Upsal Observatory; and we hope that, from the great importance of these observations in questions affecting atmospheric circulation, Father Perry will be enabled to add them to his future monthly reports. It was pointed out by us last year that in discussing the hours of occurrence of the minimum temperatures, the double inflexion in the curve which was obtained was solely due to the adoption in the discussion, of the civil day, beginning with 1 A.M., and that while the civil day must be employed in discussing the maximum temperatures, the astronomical day must be employed for the minimum temperatures. The minimum temperatures have now been discussed afresh, the astronomical day being adopted, with the result that there is only one inflexion in the time curve of the minimum temperature, the hour of lowest daily temperature falling in the annual curve between 4 and 5 A.M.

CLIMATE AND INFANT MORTALITY IN TASMANIA.—A carefully prepared paper on this subject, by Mr. E. C. Nowell, Government Statistician, has been published in the *Report of the Royal Society of Tasmania for* 1875, in which the statistics for Tasmania are compared with those for South Australia, Victoria, Queensland, and New South Wales for the five years 1869-73. Among the interesting results arrived at, the most important are these two, viz., first, the average number of deaths of infants under one year to 100 births for each colony was—South Australia 14·24, Victoria 11·86, Queensland 11·07, New South Wales 9·57, and Tasmania 9·45 ; and secondly, the proportions which the deaths of children under five years of age bore to 100 deaths at all ages were—South Australia 54·17, Queensland 46·33, Victoria 45·50, New South Wales 42·14, and Tasmania 28·08. These interesting and instructive results, showing the advantages possessed by Tasmania in regard to the low rate of mortality among infants and children, Mr. Nowell considers to be chiefly due to the remarkable salubrity of its climate. It is highly probable that it is to the climate that this low infant mortality must be ascribed, seeing that the summer heat of Tasmania is not nearly so great as that of the other colonies, and consequently the mortality from bowel complaints may be expected to be much less, whilst in all these colonies the temperature does not fall so low in the winter months as to prove so seriously fatal to the very young, as is the case in such climates as that of Great Britain. Mr. Nowell would do a very valuable piece of work if he extended the inquiry he has so well begun, in the directions we have indicated, so as to ascertain the particular diseases, the mortality from which is unusually low in Tasmania, and the seasonal distribution of the deaths from different diseases.

ON THE PROPER LENGTH OF THE GYMNASIUM SWING

MANY of the evolutions performed upon the gymnasium swing can be made equally well upon swings of any length; with others it is different. When the evolution is such that the swing in one direction marks a period of exertion, while the return is comparatively a period of rest, then the evolution cannot be equally well performed with swings of all length.

One of the most useful exercises is made as follows :—Reaching up and grasping the rings let the swing be started, and at the beginning of a forward swing the feet are thrown above the head, the legs being flexed. As the forward swing closes the legs are extended and the arms flexed, the body being thus thrown upward and outward. Here, also, by some practice, one learns to accomplish the swing with a minimum of exertion, which a good gymnast always does; nevertheless, the number of swings before exhaustion takes place varies with the length of the ropes, as is shown in the following series of experiments made upon myself :—

Nipher.

l	t	n
12	4·5	10·0
11	4·3	12·2
10	4·1	12·8
9	4·0	15·4
8	3·9	15·2
7	3·8	13·0
6	3·7	10·6

l = distance from point of suspension to centre of hands ; t = time of one complete oscillation (forward and back) ; n = No of oscillations before exhaustion.

It will be observed that n reaches a maximum where l = about 8·5 feet, or where the time of a full swing is between 3·9 and 4·0 seconds.

Another series of experiments was made upon Mr. Cunningham, a young man about 5 feet 2 inches in height, and of light build. The maximum value of n is here reached when the length of the rope was about ten feet, and here the time of a full swing was about 4·1 seconds.

My own height being about five feet eleven inches, it will be observed that these two cases are sufficiently representative.

Cunningham.

l	t	n
12	4·3	14·0
11	4·3	16·3
10	4·1	17·0
9	3·8	14·6
8	3·7	12·6

The swing was shortened by drawing the rings up from the ground, and in the latter table the values of n for short ropes are a little too small, as he seemed fearful of falling. Hence we may affirm that in order that this and similar evolutions may be elegantly performed, the time for the full swing should be four seconds.

The cause of rapid fatigue with long ropes is that the body must be held in a constrained position for too long a time. With very short swings the muscles are forced to work with too great a velocity.

The muscular action is here too complex to allow of any mechanical discussion, but the general results are exactly what the discussions of Prof. Haughton might have enabled us to predict. FRANCIS E. NIPHER

Washington University

NOTES

THE demise of such veterans in biology as Von Baer, Ehrenberg, &c., during the past year has left gaps in the lists of honorary fellows of our scientific societies which come to be filled up with men almost of a different generation, yet worthy successors of the great masters departed. We understand the three subjoined *savans* have recently been elected foreign members of the Linnean Society—viz., Pierre Du Charte of Paris, highly distinguished for his researches in teratological, physiological, and other branches of botany; Prof. Carl Gegenbaur of Heidelberg, whose labours in zoology and the comparative anatomy of the vertebrates and invertebrates are acknowledged as of the highest standard; Prof. Rudolph Leuckardt of Leipzig, by whose philosophical investigations into the morphology and physiology of the lower forms of animals and establishment of the group coelenterata, zoologists of all countries are highly indebted.

WE are glad to hear that Dr. Dohrn's Zoological Station continues to make satisfactory progress. The number of naturalists who have availed themselves of the institution, we are informed, has reached eighty, from almost all parts of Europe. The summer dredging with the small steamer will now shortly commence, and we may hope that besides the important physiological work which is there done, that a complete knowledge of the rich fauna of this bay will be a further result furnished by this station. The institution is carried on under the direction of Dr. Dohrn, the detail management being in the hands of Dr. H. Eisig, who is backed up by two assistants. The aquarium belonging to the station has in their hands for some time been the most successful in Europe, and naturalists we do not doubt will find the experience gained by some years working enables their wants to be more readily provided for. A statement has appeared in an English paper which might lead one to think some change had been made, but we are in a position to state that no alteration has been made, and that with increased opportunity of collecting material the institution will become each year more useful.

ALTHOUGH M. Leverrier's health is so unsatisfactory he continues to attend to his professional duties as persistently as ever. The number of stations oraganised by him in connection with the international service now exceeds 1,200. He is preparing instructions to be sent to each correspondent on the method of better utilising warnings from the Observatory. He confesses that the agricultural service is in a period of uncertainty, and that some time must elapse before it can render much service to the commonwealth. He urges strong reasons why the service—not conducted by military men, as in America—should be conducted by men accustomed to military discipline.

THE primary clock of the Paris Observatory is now regulating the motion of the clocks of the Conservatoire, St. Sulpice, and the Luxembourg. M. Leverrier proposes to adapt the same system to a number of other public clocks, and even to those which are used in the cab stations. But the application of the system is delayed for want of funds.

PROF. RUPERT JONES, F.R.S., is preparing a new edition of Dixon's valuable "Geology of Sussex" for Mr. W. J. Smith, of Brighton. The work will be brought up to the present state of knowledge. The descriptions and lists of Sussex fossils will be carefully revised in this new edition, and a full account will be given of the Sub-wealden boring and its results, of the Warren-Farm Well, and of the archæological discoveries at Cissbury and elsewhere in Sussex. It is also arranged that a selection of the original quarto plates of Mantell's "Fossils of the South Downs; or, Illustrations of the Geology of Sussex" (1822), with descriptions according to our latest knowledge of the subjects, shall form part of the new volume.

A NEW application of the principle of the magic-lantern has been lately introduced into London for drawing attention after dark to the names of restaurants and shops. At present it is only used where the establishment has a lamp overhanging the pavement. The lenses are fitted into the bottom of the lamp, the words to be read are painted on the "slide," which has an opaque ground, and thus the advertisement is thrown in letters of light on to the pavement. Ordinary gas lamps are used, and when the apparatus is once fixed the announcement appears every time the lamp is lighted without any further trouble.

THE anniversary meeting of the Geographical Society was held on Monday, and as usual, a large increase of numbers was reported, as well as the prosperity of the Society generally. The president, Sir Rutherford Alcock, reviewed the progress of geographical science during the year, a year remarkable by the return of three important expeditions to England—the *Challenger*, the Arctic, and that under Lieut. Cameron. The medals, the award of which we have already announced, were presented to Sir George Nares, the Pundit Nain Singh, and Capt. Markham. The president, in concluding his address, announced that the Society's African Exploration Fund Committee were about to appeal to the Society and the public for support and co-operation in the prosecution of continuous and systematic African exploration. In view of the interests concerned in this work, the Council felt confident that their appeal would meet with a ready response, not only in the United Kingdom, but in all our colonies.

ON the evening of June 5 the first trial, in this country, of the Jablochkoff electrical light will be made at the West India Docks. The object of this trial is to test the applicability of this new light to purposes of lighting up of docks, warehouses, &c., in order that work may be continued during the night.

"SUMMER Schools" are becoming a regular institution in America, and no more pleasant way could be devised of combining the *dulce* and the *utile* than that of a proposed aquatic summer school of natural history, which, under the direction of Prof. Theodore B. Comstock, of Cornell University, expects to charter a large steamer, and spend the summer around the shores of Lake Superior in the study of the geology and natural history of that region. The steamer will accommodate from seventy-five to one hundred passengers, to be made up of students and professors. Regular instruction will be given in the form of lectures during the voyage, and every facility afforded by the examination of mining localities and the like. The vessel will probably start from Cleveland or Detroit on July 7, and proceed thence to Lake Superior, making its full circumnavigation. The coast of Georgian Bay, on Lake Huron, will be investigated on the return voyage. The expense will probably amount to about 125 dollars for each person. In addition to the aquatic summer school mentioned above, Harvard University announces two special summer courses of instruction, one in zoology and the other in geology.

THE progress of industry in France (denoting by industry the working of raw material) has been very rapid, much more rapid, proportionally, than that of agriculture. We learn from *La Nature*, that in half a century, the employment of cast iron, so necessary to industry, has been multiplied tenfold, and that of coal twenty-fold. The total power of steam-engines has increased in still greater proportion; it is at least thirty times greater than it was in 1840. Going further back, the proportion would be less interesting, as steam was used in but few establishments. In 1820 there were only sixty-five steam-engines in the whole of France. As an acquisition of material force these engines represent in value at least 25 million workmen, added to the 10 million who labour in workshops, and to the motor forces furnished by nature gratuitously, air and water. There are in France nearly 40,000 weirs, the falls from which work more than 80,000 mills of every kind, and this number might be doubled. In some parts, lastly, they are beginning to utilise the force of the tide.

AT a recent meeting of the French Physical Society M. Gouy gave an account of experiments he had made on flames produced by a mixture of air and coal-gas, holding in suspension pulverised metallic salts. The salts, dissolved in water, were introduced by a pulveriser, acting with air compressed to half an atmosphere. In these flames the blue surface of the interior cone, which gives the spectrum of carbon, gives also the lines proper to the salt which the flame contains. These lines are not visible beyond this part, and they coincide with the principal lines of the metal in the electric spark. The metals sodium, strontium, magnesium, lithium, manganese, iron, cobalt, bismuth, cadmium, zinc, and osmium give this phenomenon distinctly. Platinum gives a special spectrum formed of regular bands. These experiments seem to prove that there is at the base of the flame a very fine layer which has a temperature much higher than the flame properly so-called.

THE exploration of the Angara, proposed by the Russian Geographical Society, has taken a yet larger extension. M. Sibiryakoff, who has presented a gift of 2,000 roubles, proposes to undertake also the exploration of the water-parting between the Obi and Jenissei, to solve the question of the practicability of a canal between the two rivers. Owing to the great commercial importance of such a canal, the Geographical Society has agreed to the proposal of M. Sibiryakoff, and will send an expedition for that purpose.

NEAR Lake Ourmia (N.W. Persia), a hill near Digala is irregularly excavated by a number of galleries for its nitrous earth, strongly impregnated with saltpetre. This loose, friable soil, of brownish colour, in irregular horizontal beds, includes layers of a mostly amber-brown earth, with layers of bone-ash, intermixed with large and small fragments of human bones, charred remains of straw, and thin seams of carbonised seeds of cereals. Fragments of burnt earthenware are scattered through this bed, and through the whole of the hill. Nearly in the middle of the hill is a conical hollow, cylindrical above, and becoming narrower upwards, like the inside of a high furnace. The inner wall shows four or five ranges of repositories, several feet distant from each other, and made of slabs of eocene sandstone, about 1½ ft. broad. These slabs of a rust-brown colour, bear evident traces of having been exposed to a fierce fire; and the whole chamber may thus be inferred to have served for a furnace to burn dead bodies. This view is confirmed by the traditions still extant among the surrounding people. Fragments of large pot-like vases, and of coffins made of slabs of sandstone, both inclosing an earthy residuum, mixed with fragments of skulls and bones show that in the same place, burials have been effected without cremation. The abundance of saltpetre in the soil of the hill has probably been derived from the nitrogen set free by the decomposition of organic remains.

THE *Phormium tenax* or New Zealand Flax, is, it is said, being largely planted in St. Helena, on behalf of a fibre company, who propose so to plant all the Government waste lands in the island.

WE have received from Dr. Petermann a very useful map issued in connection with the Russo-Turkish war. Its purpose is to show at a glance the relative position of the boundaries of Russia, Turkey, Persia, and British India. It extends from Bosnia to Central Asia, and from the north of the Black and Caspian Seas to the Indian Ocean, and includes enlarged special maps of the Nile Delta and of Crete.

THE first field-day of the Liverpool Geological Society was held on Saturday last. The members and their friends proceeded to Crosby by rail and from thence by 'bus through Little Crosby, Hightown, Altcar, Downholland, Has Rayne, to Hallsall, and back again through Lydiate and Maghull to Sefton, where they had tea and examined the church. The object of the visit was to examine the great post-glacial deposits of the West Coast of Lancashire. The party was guided by Mr. T. Mellard Reade, C.E., F.G.S., who described the succession of the beds and their superficial extension. He explained that what they had seen was only a part of a very extensive series of deposits surrounding our coasts and found at most estuaries. The society last May examined a portion of the same series disclosed by the North Dock excavations, and the present excursion would enable them to better understand this most interesting part of the geology of Lancashire. At the Alt Mouth was found a peat and forest bed between high and low water-mark washed daily by the tide. The moss land between the inland edge of the 25-feet plain was only an extension of this submarine forest which passes under the sand hills and joins the moss. Under the moss lies the main silts with here and there some freshwater deposits on the surface.

SOME interesting disclosures were made last week at the Marlborough Street Police Court as to the method on which certain war maps are constructed. A certain publisher, whose name is probably unfamiliar to most of our readers, has published one of those exaggerated pictorial maps of the seat of war so attractive to the indiscriminating public. We have seen the map, and a very misleading and rude specimen of cartography it is. Its natural defects are bad enough, but it came out during the proceedings that intentional errors—names of non-existing places and wrong positions of existing places, were introduced for the purpose of detecting imitations. The magistrate, Mr. Newton, was therefore to a considerable extent justified in stating that the designer of the map seemed to have constructed it out of his own brain, and he virtually dismissed the summons.

FROM the report read at the annual meeting of the Nottingham Literary and Philosophical Society, we see that it now has 352 members of various classes. The Society has had several scientific lectures during the session, and we are specially glad to see that the Natural Science Section is in a flourishing condition, having had twenty-nine meetings and excursions during the session, at which papers were read on various subjects of scientific interest.

THE Société Française de Navigation Aërienne, an institution which has been approved by the Minister for Public Instruction, is to organise a collective exhibition at the Champ de Mars. It will include any means of propelling and governing in the air. Any instrument which has proved efficacious in some degree will be admitted if its dimensions are not too large, or by model, if otherwise. Any apparatus for making aërial observations, or helping aëronauts in any way will be admitted. The collection will also include maps, books, manuscripts, and newspapers relating to aërial navigation.

WE have received from Dr. Warren de la Rue a small pamphlet containing two sets of tables which must prove of great use to most scientific workers. There are tables for the reduction to 0° centigrade of a mercury column observed with a glass scale divided into millimetres, and tables for the reduction of millimetres (mercurial pressure) to thousandths and millionths of an atmosphere, and *vice versâ*. These tables are printed for private circulation.

AN important Russian work has just been published by Prof. Inostrantsef—"Geological Sketch of the Povyenetz District, Government Olonetz, and of its Mines." This large volume (750 pp.), being the result of seven years' explorations, contains detailed reports on the travels of the author, an orographical description of the district (the surface of which exceeds that of Switzerland), an interesting chapter on the metamorphism of the green slates, and a sketch of the glacial formations. It is accompanied by maps, engravings, and chromolithographed plates representing microscopical cuttings of rocks.

THE additions to the Zoological Society's Gardens during the past week include an African Turkey Buzzard (*Buteo tachardus*) from Africa, presented by Mr. A. Anderson, F.Z.S.; two Rendall's Guinea Fowls (*Numida rendalli*) from Bogos Country, Abyssinia, presented by Capt. Burke, s.s. *Arcot;* three Carpet Snakes (*Morelia variegata*) from Australia, presented by Mr. J. Moseley; a Guianan Crested Eagle (*Morphnus guianensis*) from the Upper Amazons, a Green-necked Pea-fowl (*Pavo spicifer*) from Java, two Barred-tailed Pheasants (*Phasianus reevesi*) from North China, a One-wattled Cassowary (*Casuarius uniappendiculatus*) from New Guinea, a Great-headed Maleo (*Megacephalon maleo*) from the Celebes, purchased; an Inconvenient Curassow (*Crax incommoda*) from South America, deposited; a Derbian Wallaby (*Halmaturus derbianus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN

COMETS OBSERVED BY HEVELIUS.—Of the eight comets observed by Hevel, better known as Hevelius, at Dantzic, with such degree of precision as could be attained with his instruments, the observations of two only have been reduced with the aid of modern places for comparison stars, &c., and in these two cases only have we other orbits than those calculated by Halley, which appear in his *Synopsis Astronomiæ Cometicæ*. The observations of the comet of 1664 have been discussed by Herr Lindelof, with the view of ascertaining whether any support were afforded by them, to a conjecture of identity of the comet, with the first comet of 1853; and those of the comet of 1683, were similarly reduced by Mr. W. E. Plummer, in his examination of the elliptical tendency of the orbit indicated by the computations of Prof. Clausen, who had previously recalculated a few of the observations. There remain the comets of 1652, 1661, 1665, 1672, 1677, and 1682; Mechain's reduction of the observations of the second of these bodies nearly a century since, will hardly be considered final.

The observations of the comets of 1672 and 1677 were published in the second volume of the *Machina Cœlestis*, and in small special treatises. This second volume of the great work of Hevelius, as is well known, is extremely scarce, the whole of the impression, with the exception of such copies as had been already presented to astronomers having been lost in the fire which destroyed the observatory, library and papers of Hevelius on September 26, 1679. The copies thus saved were so few in number, that as Lalande remarks "On peut regarder cet ouvrage comme un manuscrit;" and the special treatises to which we have alluded are perhaps of equal difficulty of access. The observations of the comets of 1652 and 1661 were printed in the *Cometographia*, not a work of very great rarity, as well as in the scarce volume of the *Machina Cœlestis;* those of the comet of 1665 also appear in this volume, in a special treatise, and in the *Prodromus Cometicus*, while those of the comet of 1682 (Halley's comet) are found in *Annus Climactericus*, and have been fully utilised.

A new reduction and discussion of the observations of the comets of 1652, 1661, 1665, 1672, and 1677, is certainly a desideratum, and important assistance in this direction might be afforded by a republication of the original observations of Hevelius from some quarter where the scarce volume is accessible. Mädler remarks upon a certain degree of resemblance between the elements of the comet of 1672 and those of the comet of 1812, which is shortly expected to return to perihelion; and it has been pointed out in this column that Halley has given the descending in place of the ascending node, for a comet observed in 1686—an oversight which has found its way into all catalogues of cometary orbits hitherto published, so that a re-computation of the orbits of the five comets we have mentioned, which now rest upon the figures of the *Synopsis Astronomiæ Cometicæ* is wanted, if only for verification.

"THE OBSERVATORY."—The second number of this new periodical is before us. Mr. David Gill continues his paper on "The Determination of the Solar Parallax;" we have the first part of an article giving the substance of a lecture recently delivered at Gresham College, by the Rev. E. Ledger, on "The Scintillation or Twinkling of the Stars," which has long been an obscure subject; Mr. Marth continues his Ephemerides for aiding physical observations of the Moon, Mars, and Jupiter; and there is also a report of the proceedings at the last meeting of the Royal Astronomical Society, including the discussion on the papers read, which, as was mentioned in a previous notice, it is intended should form a feature of the publication. We think every one who is competent to judge of the actual state of the case will agree in the opinion expressed at p. 55, while remarking on Mr. Todd's extension of Damoiseau's Tables of Jupiter's Satellites to the end of the present century, that "the time has hardly yet come for the formation of entirely new Tables." So far as regards the necessary observations, it must be admitted that they are being followed up with vigour at several observatories. The first binary star orbit on p. 58, refers to ξ Scorpii (ξ Libræ of Flamsteed), not to ζ Libræ; the error, however, is made in the *Astron. Nachrichten*, whence the orbit is taken.

L'ÉTÉ DE LA SAINT-MARTIN ET LES ÉTOILES FILANTES.—In No. 493 of the *Bulletin Hebdomadaire* of the French Scientific

Association, the Abbé Lamey, under the above heading, endeavours to explain by a new theory, certain abnormal temperatures which in one case, at least, has formed the subject of popular tradition. "The Summer of St. Martin," as the common phrase runs, presented itself, according to the Abbé, in a very definite manner in the last year; the month of November commenced colder than usual, but on the 12th it suddenly became warmer than from the sun's altitude could have been expected. Long-continued notice of a similar rise in temperature about the feast of St. Martin the Abbé considers is a proof that our ancestors were excellent observers, while the existence of a tradition upon the point shows clearly that the phenomenon has not been confined within restricted limits; it has been exhibited, he says, simultaneously in Europe and in the United States, and this without being materially affected by the climacteric conditions of the places of observation. One circumstance only he thinks appears to influence it, viz., the latitude; it vanishes as the equator is approached, and is not yet known to be recognised in the southern hemisphere.

The anomalous thermometric effect is not, however, perceptible only about St. Martin's Day. There is an analogous phenomenon according to the Abbé, in August: "une chaleur torride qui règne subitement après quelques jours de refraîchissement de l'air," and a similar effect, though in an opposite direction, has been noticed at the end of April or at the beginning of May, when vernal frosts so disastrous at this season occur, and have often been preceded by "une douce chaleur précoce," as the Abbé terms it, which has hastened forward the vegetation.

After remarking that the cause of such abnormal changes of temperature is not to be sought in any influence residing either in the sun or in the earth's atmosphere, it is suggested by the Abbé that it may be more probably found in what he calls cosmical meteorology, or as we are more accustomed to term this branch of science, meteoric astronomy. In November, August, and April meteors are more numerous than usual, and two of the greatest periodical showers yet observed, fall in November and August. His theory is that when a large number of meteors are passing between the earth and the sun, the solar rays are intercepted to a sufficient extent, to cause a diminution of temperature on the earth's surface, while, when a similar large number of meteors are so placed that they might reflect the heat derived from these rays, and so produce an effect of the opposite nature, that, to use his own words, those calorific rays "qui viendront frapper l'essaim météorique encore voisin de la terre seront *réverbérés* sur notre planète, de façon à recevoir alors un surcroît de chaleur." The Abbé lays some stress also upon another point of apparent coincidence: the intensity of the periodical meteoric showers of November varies from year to year, and "the summer of St. Martin" does not present itself under the same circumstances in every year.

In thus noticing the Abbé Lamey's attempt to explain a phenomenon which has been so long remarked as to have become a popular belief, at least in France, it will be understood that we are by no means advocating the probability of such a theory.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In a Convocation held May 23 a decree was carried, without opposition, to the effect that the Vice-Chancellor and Proctors be authorised to nominate a delegate to represent the University at the 400th anniversary celebration of the University of Upsala, in September next.

In a Convocation to be held on June 5 it will be submitted to the House that the Curators of the University Chest be authorised to expend a sum not exceeding 7,000*l*. on the construction and fittings of new chemical laboratories at the University Museum. Also that a sum not exceeding 2,400*l*. be expended on certain additions to the University observatory.

An examination will be held in common at Magdalen, University, and New Colleges, on Tuesday, June 26, for election to the following mathematical scholarships:—One demyship at Magdalen College, of the annual value of 95*l*., inclusive of allowances; one scholarship at University College, of the annual value of 80*l*., tenable for five years during residence; one scholarship at New College, of the annual value of 100*l*., tenable for five years during residence. Testimonials of conduct, &c., to the President of Magdalen College, Mr. C. J. Faulkner, of University College, or the Sub-Warden of New College, between 4 and 6 or 8 and 9 P.M., June 25.

The commemoration fixed for June 13 will, it is understood, be held in the Sheldonian Theatre, although no official notice to that effect has appeared. There will be the usual round of festivities attendant on the event, though it has not transpired if the University will confer any honorary degrees on distinguished persons at the commemoration.

CAMBRIDGE.—The Museums and Lecture-rooms Syndicate, in their Eleventh Annual Report, just issued, state that the buildings are in an efficient state of repair, and the collections, to which many valuable additions have been made during the past year, are in good order. They draw attention to the munificence of the Chancellor of the University, the Duke of Devonshire, who has now completely furnished the Cavendish Laboratory with the instruments required by the present state of science. Profs. Liveing, Dewar, and Stuart complain of want of suitable accommodation for the work of their several departments, and the Syndicate concur in the reasonableness of their complaints.

An important report has been issued by the Musical Examinations Syndicate, which states that under existing regulations for obtaining a degree in music no provision is made for testing the literary and scientific qualifications of the candidates. They therefore recommend that no candidate be admitted to the examination for the degree of Mus. Bac. unless he have passed Parts I. and II. of the Previous Examinations, or one or other of their equivalent examinations. As to the examination for the degree of Mus. Bac., it is proposed to divide it into three parts—1, a preliminary examination, consisting of acoustics, harmony, counterpoint; 2, the exercise; 3, a more advanced examination in musical science; and that no person be accepted as a candidate for the second or third parts until he has qualified in the previous part or parts. In order to encourage the study of music, that it be recognised as the subject of an additional special examination for the ordinary B.A. degree, and that a student who has passed the Previous and the General Examinations, and is in his ninth term of residence at least, having previously kept eight terms, shall, on passing the preliminary examinations in acoustics, harmony, and counterpoint, be entitled, when he has kept nine terms, to receive the degree of Bachelor of Arts.

The "Rede" lecture was delivered on May 25 in the Senate House by Sir C. Wyville Thomson, who gave a brief sketch of the main results obtained by the *Challenger* expedition.

LONDON.—A new and additional Chair of Clinical Surgery has been created at King's College, which is to be filled by Prof. Lister of Edinburgh. The Chair of Systematic Surgery is thus still vacant.

EDINBURGH.—The students at the University have, during the past session, taken a step which it seems surprising they have not taken long ago. There is, in the Scotch universities, no college life as in England, the students appearing at their classes at the proper hours, and then dispersing to their respective lodgings in various parts of the town. While this system has undoubtedly its advantages, it is attended with not a few social, moral, and physical drawbacks, so that we are glad to learn that the Edinburgh students have started a Students' Club which has been thoroughly successful, and calculated we believe, if prudently conducted, to be productive of considerable benefit to the raw and lonely Scotch youth "when first he leaves his father's fields," to get what training and equipment for the future fight Edinburgh can give him.

By the transference of Prof. Lister to London, the Chair of Clinical Surgery in the University becomes vacant.

SIR JAMES KAY-SHUTTLEWORTH.—The death took place on Saturday last of Sir James Kay-Shuttleworth, a name well known in connection with educational and social reform. The deceased baronet, who was born at Rochdale on July 20, 1804, was for some time secretary to the Committee of Council on Education, and whilst fulfilling the duties of this post he was mainly instrumental in establishing a system of school inspection by officers appointed by the Government. On his resignation he was succeeded by Mr. Lingen, now permanent secretary of the Treasury, who was succeeded in his turn by Sir Francis R. Sandford. Under Sir James's scheme teachers were divided into nine grades, and received money grants, not according to the number of their scholars or of their passes, but largely, according to the grade they had obtained by examination or service. He was hostile to the Revised Code, which was introduced, about twelve years after his resignation, by Mr. Lowe and his successor. It is undoubtedly to Sir James that we owe the training colleges and the pupil teacher system, without which it would have been impracticable for us to advance educationally even as we have done. At the close of the year 1849 he received a baronetcy at the recommendation of Lord Russell, then Prime Minister. In 1870 he received the honorary degree of D.C.L. from the University of Oxford.

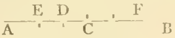
SPELLING REFORM.—An influential Conference on English Spelling Reform was held on Tuesday at the Society of Arts, under the presidency of the Rev. A. H. Sayce and Sir Charles Reed. Many weighty reasons were urged against the present system, and a deputation consisting of Prof. Max Müller, the Rev. A. H. Sayce, Dr. Morris, Mr. Ellis, Mr. Sweet, Dr. Murray and others, was appointed to wait upon the Education Department in reference to the subject. A proposal having the support of such names as we have mentioned deserves at least serious consideration.

A SIBERIAN UNIVERSITY.—It has been finally decided that the New Siberian University, to which we referred some time since, is to be established at Omsk. So long ago as 1803 a wealthy Uralian landowner named Demidoff gave 100,000 roubles to the Treasury, to be expended in the establishment of a University. This sum has now swollen to 150,000 roubles, to which a Siberian merchant has added 100,000 roubles more. Orders have been issued to begin the construction of the university buildings at once, so as to have them ready for occupation by July, 1880. The estimated cost of the future professional staff, together with other incidental expenses connected with the university, is 307,000 roubles yearly.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, May 10.—Lord Rayleigh, F.R.S., president, in the chair.—Mr. Tucker communicated a short account of a paper by Dr. Hirst on the correlation of two planes. In a former paper on the subject (*Proceedings*, vol. v., p. 40), the nature and properties were described first, of an ordinary correlation satisfying any eight given conditions; secondly, of an exceptional correlation of the first order, possessing either a singular point or a singular line in each plane, and satisfying seven conditions; and thirdly, of an exceptional correlation of the second order, having in each plane not only a singular point but also a singular line passing through that point, and satisfying six conditions. Moreover, the two following numerical relations were established between the (π, λ) exceptional correlations of the first order, with singular points and singular lines respectively, which satisfy any seven conditions, and the (μ, ν) ordinary correlations, which, besides satisfying these same conditions, possess a given pair of conjugate points or conjugate lines respectively $(2\nu = \mu + \pi, 2\mu = \nu + \lambda)$. It was by means of these relations that the number of ordinary correlations was determined which satisfy any eight elementary conditions. Before they could be applied, however, the exceptional correlations of the first order which satisfy any seven elementary conditions) had to be directly determined, and this determination not unfrequently necessitated the consideration of the projective properties of curves of high order. In the present paper the writer shows that the object just referred to can be attained in a very much simpler manner by means of two general relations, hitherto unobserved, connecting the number of exceptional correlations of the second order, which satisfy any six conditions, with the numbers of exceptional correlations of the first order which, besides satisfying the six conditions in question, possess a given pair either of conjugate points or conjugate lines.—The secretary then read part of a paper by Prof. H. Lamb, of the University of Adelaide, on the free motion of a solid through an infinite mass of liquid. Suppose that we have a solid body of any form immersed in an infinite mass of perfect liquid, that motion is produced in this system from rest by the action of any set of impulsive forces applied to the solid, and that the system is then left to itself. The equations of motion of a body under these circumstances have been investigated independently by Thomson and by Kirchhoff, and completely integrated for certain special forms of the body. The object of the present communication is, in the first place, to examine the various kinds of permanent or *steady* motion of which the body is capable, without making any restrictions as to its form or constitution; and, in the second, to show that when the initiating impulses reduce to a couple only, the complete determination of the motion can be made to depend upon equations identical in form with Euler's well-known equations of motion of a perfectly free rigid body about its centre of inertia, although the interpretation of the solution is naturally more complex. Free use is made throughout the paper of the ideas and the nomenclature of the theory of screws as developed and established by Dr. Ball.—Herr Weichold (Head-master of the Johanneum, Zittau, Saxony) sent a paper (read in part by the secretary) containing a solution of the irreducible case, *i.e.*, of the problem to express the three roots of a complete equation of the third degree, in the case of *all* these roots being *real*, directly in terms of its coefficients, by means of purely algebraical and really performable operations, whose number shall always be limited, except in the case where all these roots are incommensurable.—Mr. H. Hart made three communications: First On the "Kinematic Paradox."—Prof. Sylvester has described a system of Peaucellier's cells, the poles of which all move in a straight line, but two of which not directly connected always remained at a constant distance. Such a result is very easily obtained by means of the following relations connecting six points A, B, C, D, E, F, lying on a straight line. If

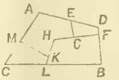


$$\left.\begin{aligned} AB \,.\, AC &= a^2 \\ BC \,.\, BD &= 4a^2 \\ EB \,.\, ED &= a^2 \\ FA \,.\, FE &= 2a^2 \end{aligned}\right\} \text{ then } FB = a.$$

He then spoke on the solution of the algebraical equation $f(x) = 0$ by linkwork, considering three points, the preparation of the equation (put under the form $\frac{A}{x+a} + \frac{B}{x+b} + \ldots = k$), the representation of the terms of this equation, and the method of adding these terms. He showed that for the solution of the cubic $x^3 + px^2 + qx + r = e$, treated under the form—

$$x + p + \frac{\left(q - \frac{r}{p}\right)x}{x^2 + \frac{r}{p}} = 0,$$

two reciprocators alone are required. He then spoke on the production of circular and rectilinear motion. The particular problem considered, he thus enunciated "to find if possible the relations that must exist between the fourteen segments of the bars placed as in the figure in order that the system may be capable of free motion."



He showed that seven equations can be obtained connecting the fourteen quantities only, so that any seven being given, the remaining seven can be determined in terms of them.—Mr. Hart then proceeded to the application to the cases of 5-bar motion, laid before the Society at its April meeting. Mr. Kempe stated that the cases submitted by Mr. Hart at the previous meeting had also occupied some of his attention, and he proceeded to remark that he had determined the positions that the lines GE, KM must have, and that the determination of one involved the determination of the other, as the position of either turned upon the fact that the angles at A and H must be equal. Prof. Cayley also made a few remarks on the subject. Mr. J. W. L. Glaisher stated that he had had all the cases in which there are more than fifty consecutive composite numbers looked out from Burckhardt's and Dase's tables, which cover six millions, and that he had found that in the first million there is a stretch of 111 numbers without a prime (about 310,000), and a stretch of 113 numbers without a prime (about 500,000); so that there are two very long sets of composite numbers in the first million, and these are longer, he thought, than anyone would have supposed likely. He exhibited the lists from which he drew the above results. Questions were put to the meeting, for information, by Profs. Cayley and Clifford.

Geological Society, May 9.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—James Dorrington, Rev. E. R. Lewis, Edward Penton, Henry Rosales, and Henry White were elected fellows of the Society.—On the Agassizian genera *Amblypterus*, *Palæoniscus*, *Gyrolepis*, and *Pygopterus*, by Ramsay H. Traquair, F.R.S.E. The author's object in this paper was to discuss the characters by which the above genera of fossil fishes have been supposed to be distinguished in the case of specimens from the carboniferous series.—On the circinate vernation, fructification, and varieties of *Sphenopteris affinis*, and on *Staphylopteris* (?) *peachii*, Etheridge and Balfour, a genus of plants new to British rocks, by C. W. Peach, A.L.S., communicated by Robert Etheridge, F.R.S., V.P.G.S.—On the occurrence of a Macrurous Decapod (*Anthrapalæmon woodwardi*, sp. nov.) in the red sandstone, or lowest group of the carboniferous formation in the south-east of Scotland, by Robert Etheridge, jun., F.G.S. After giving a detailed bibliography of the palæozoic malacostracous crustacea, the author described the remains of a small crustacean from the lower group of the carboniferous formation near Dunbar, and discussed its affinities and systematic position, which he regarded as being among the Macrurous Decapods, although the absence of the eyes in the preserved specimens, and some other characters, rendered it doubtful whether it might not in some respects approach the Stomapoda. Its position among the Macrura seemed, however, to be established by the well-developed abdominal somites and telson. He referred the fossil to Salter's genus, *Anthrapalæmon*, and named the species *A. woodwardi*.—On the stratigraphical position of the corals of the Lias of the Midland and Western Counties of England and of South Wales, by R. F. Tomes, communicated by R. Etheridge, F.R.S., V.P.G.S. The object of this paper was to give the precise stratigraphical position of the species of liassic corals collected by the author and his friends in the districts above mentioned. He noticed forty-one species, of which fifteen were described as new.

PARIS

Academy of Sciences, May 21.—M. Peligot in the chair.—The following papers were read:—Meridian observations of small planets at the Paris Observatory during the first quarter of 1877, by M. LeVerrier.—On Gay-Lussac's law of volumes, by M. H. St. Claire Deville. He considers recent researches have neither invalidated nor added to the law.—On an algebraic method for obtaining the *ensemble* of the fundamental variants and co-variants of a binary form, and of any combination of binary forms (continued), by Mr. Sylvester.—Report on M. Roudaire's project of making an interior sea in the south of Tunisia and the Province of Constantine. M. Favé reports favourably; but on the points, whether the sea would not dry up, whether the vapours would benefit neighbouring lands and not be carried to the sea by winds, and whether the eastern Algerian and Tunisian climate would recover its old fertility, and be improved hygienically, MM. Daubrée and Dumas (while adopting the general conclusions) express hesitation, and desire further researches.—Report on a memoir of M. Stanislas Meunier, entitled "Composition and Origin of the Diamantiferous Sand of Toit's Pan (in South Africa). M. Meunier has separated several mineral species not before noticed there, and offers an ingenious explanation of the way of filling up those vertical pits.—On the employment of oxygen of high tension as a process of physiological investigation; poisons and virus, by M. Bert. There is in anthracic blood a toxical and virulent principle which resists the action of compressed oxygen and alcohol, and which can be isolated like diastase. M. Bert is studying its nature and its relation to the bacteria. Lymph, too, and the pus of glanders, by resisting compressed oxygen, show that their virulent action is not due to living beings or cells.—On the employment of rotatory discs for the study of coloured sensations, by M. Rosenstiehl.—Dehydrated oxalic acid may serve to characterise polyatomic alcohols; chemical function of inosite, by M. Lorin.—Decomposition of chlorhydrate of trimethylamine by heat, by M. Vincent. This substance might be utilised to give ammoniacal products and pure chloride of methyl, the latter yielding the methylated aniline colours or pure methylic alcohol.—Observations of a disease of the vine known commonly as *white*, by Mr. Schnetzler.—New spectroscopic method, by Mr. Langley. Two spectra from the north and south poles of the sun respectively are put in juxtaposition (a considerable dispersion being used); let the instrument be adjusted so that the lines in both are continuous. On turning the spectroscope round its axis of collimation till the light comes from the east and west extremities of the equator the solar lines are displaced, while the atmospheric remain continuous. On turning 180° the spectra glide on one another like a Vernier on a scale. The point is, *simultaneous* observation of the *different* displacement of the solar and the atmospheric lines in the two spectra.—On a transmission of motion, by M. Rozé.—On the spectrum of the electric spark in a compressed gas, by M. Cazin. From experiments on air and nitrogen he concludes that the electric spark in a gas is similar to an ordinary hydrocarbon flame. In each there are luminous particles giving a spectrum of lines, and solid or liquid particles giving a continuous spectrum. The latter (in the case of the spark) come from the electrodes and the walls. When the pressure is increased these particles are more abundant; the continuous spectrum becomes more brilliant, and finally makes the linear spectrum disappear. The luminous spark called an *aureole* is of gaseous particles, and is to the total spark what the blue base of a candle flame is to the entire flame.—Studies on organ pipes, by M. Philbert.—On some new models of radiometers, by Mr. Crookes.—Thermo-chemical study of aniline and some other bodies of the same group, by M. Louguinine.—On the nitrates of bismuth, by M. Yvon.—On the properties of resorcine; molecular volumes, by M. Calderon. Resorcine in solution behaves as if it were solid and isolated from the solvent. In presence of water and potash it absorbs oxygen, though very slowly.—Anatomical characters of the blood in new-born infants during the first days of life, by M. Hayem. *Inter alia*, the red corpuscles are much more unequal in size than in the adult, and seem of a different composition. The number (in a cubic metre) is nearly as high as in the most vigorous adult. The number of white corpuscles is three or four times as great as in an adult. When the infant has reached its minimum weight (about the third day) the number of these suddenly falls; various fluctuations ensue (which are described).—On a process for estimation of alcohol in liquids, by M. Fleury.—On the filling of fissures in chalk with silex, by M. Robert.—M. Vinot presented a celestial map of the equatorial region.

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THURSDAY, JUNE 7, 1877

THE ANTIQUITY OF MAN

THE Conference on the Antiquity of Man lately held by the Anthropological Institute, and reported in these columns, has led to a result by no means unsatisfactory, when all the conditions of the problem are duly weighed. The result is merely negative, but in arriving at it several misconceptions and errors of fact seem to us to have been swept away. In its discussion there were two parties represented, one eager to show that the antiquity of man has been proved by modern discovery to be far older than the date which had been arrived at by the labours of Falconer, Lyell, Prestwich, Evans, Boyd Dawkins, and others, while the other contended that the subject had not advanced in the least degree during the last few years, and that the so-called discoveries were either errors of observation, or resulting from premises which were altogether unsatisfactory. The field of the discussion lay in matters geological rather than archæological, and the caution which the president urged upon the conference was certainly not urged in vain.

The chief interest of the debate turned upon the question as to whether there was any evidence in this country of man in the caves or river deposits older than post-glacial times. The readers of the works of Messrs. Croll and James Geikie will remember that they ascribe all the traces of palæolithic man in this country either to a pre- or inter-glacial age, basing their conclusions principally upon the fact that in the river deposits and caves some of the associated animals, such as the hyæna, lion, and hippopotamus are now only to be found in hot climates; and seeing that no traces of a warm climate are presented by any post-glacial deposit in Britain, they infer that those in question are of a much higher antiquity. They account for the association of southern and northern animals by the supposition that they occupied the country at different times, during glacial or interglacial æons of from five to twelve thousands years in length. To this it was objected that the intimate association of forms prove that both sets of animals inhabited the country at the same time, and were the result of the overlapping of different faunas during seasonal changes. The reindeer formed a large portion of *the prey* of the hyæna, and must therefore have been a contemporary. It was also pointed out by one of the speakers, that there is no evidence from the animals that there ever was anything like "the perpetual summer," advocated by Mr. Geikie at any time in the pleistocene age. The hippopotamus in Regent's Park takes his tub regularly in spite of the east winds so prevalent in the spring, which remind one of the glacial period; and the tiger crosses the frost-bound rivers of the Amoor to prey upon the reindeer. The lion, now found only in the south, lived in the days of Herodotus in the inclement mountains of Thrace. It seems, therefore, to us, that any argument based upon fossil animals as to a warm inter-glacial period, is worthless. And further, it is obviously unfair in treating of the fauna associated with man to adopt the forensic device of choosing some witnesses to the exclusion of others. It would be as easy to prove the climate in question to have been temperate from the associated remains of bison, stag, and horse, as it would be to prove it to have been arctic from the associated musk sheep, lemmings, and reindeer. It was probably a varying climate, with great extremes, similar to that in Central Siberia, in which the summer heat and winter cold are very severe.

The fossil mammalia of the pleiostocene tell us nothing as to the relation of man to the glacial period. The Arctic species invaded Europe probably from Asia, while the ice was finding its way southwards from the mountains of Scandinavia, and occupied the area north of the Alps and the Pyrenees, *while* the confluent glaciers covered the area north of the valley of the Thames. When the ice ultimately retreated they followed it, and thus were both pre- and post-glacial. Nor do the survivals from the pleiocene age tell us anything, such as the hippopotamus, the *Rhinoceros leptorhinus*, and the *Elephas antiquus*, since they belong both to the earlier and later pleistocene strata, and are also associated with remains of reindeer, and other northern species. The presence of the reindeer in all the palæolithic caverns stamps the age of man as late pleistocene, according to Prof. Boyd Dawkins, but it does not afford any clue as to his pre- or post-glacial age. The glacial period is not a hard and fast line dividing one fauna from another. One palæolithic cave, however, in this country, that of Pont Newydd, in the valley of the Elwy, near St. Asaph, is of well-ascertained post-glacial age.

The argument urged in favour of palæolithic man being pre- or inter-glacial, based upon the distribution of the mammalia in southern and eastern England, and in France, while they are conspicuous by their absence in the glaciated areas of Scotland, Cumberland, and Wales, was met by the view that the barren areas were covered with ice, *while* other districts further to the south were occupied by the animals. The hypothesis that the uplands of Wales and Northern Britain were ever stocked by the same animals as the fertile river-bottoms of the south, seems to us little less than absurd. Yet this is necessary for the view that their remains have been removed from the barren areas by the subsequent grinding of the ice-sheet.

In the course of the discussion the reputed cases of the occurrence of palæolithic remains in the deposits older than the post-glacial were minutely criticised. Prof. Busk stated that the fibula of the Victoria Cave, formerly supposed to be human, was altogether too insignificant a fragment to base any conclusion upon as to man's antiquity. Two small cut-bones, however, of goat were brought forward by Mr. Tiddeman in support of the pre- or inter-glacial age of man in the Victoria Cave. On the other hand, it was urged that these were derived from the superficial stratum containing Roman coins and pottery, &c., in which they were very abundant. From the nature of the cuts it seems to us that if it be established that they were discovered in the undisturbed stratum along with the hyænas, they would prove not only the presence of man, but of a user of a knife or chopper of bronze or iron. The absence of the goat, also (probably a domestic animal) from all undisturbed pleistocene deposits in this country, and in France, Belgium, and Germany, renders it very probable that the animal was introduced into those regions after the close of the pleistocene age. But even supposing that these difficulties be got over, the age of the deposit in which these fragments are stated to have been found is a matter of dispute in which the authorities are about equally balanced on either side.

The asserted inter-glacial age of the river gravels containing palæolithic implements proved equally unsatisfactory. The cases supposed to be decisive of the question in the neighbourhood of Brandon and Thetford, were considered by Prof. Hughes to throw no light upon it, since the deposits above them, supposed to be boulder clay, are not boulder clay *in situ*. It was forcibly urged by several speakers, and especially by Prof. Prestwich, that the flint implement-bearing strata are proved by their position in the valleys to be later than the glaciation of the district, in every case where it has been glaciated, or in other words, that they are decidedly of post-glacial age.

The general question of the antiquity of man in Europe was not discussed, although we gathered that the evidence of the presence of man in the Italian pleiocenes was not considered satisfactory. The general impression left upon our minds is that in Britain there is no evidence of any palæolithic men, either in caves or the river-deposits of an age older than post-glacial, and that the discoveries of the last fourteen years have merely given us interesting details as to the palæolithic savage, without telling us anything of his relation to the glacial period.

THE VALUE OF NATURAL HISTORY MUSEUMS

WELL-arranged museums are valuable to the state in many ways. The technological department ought to show in what new directions capital may and may not be invested; the geological and mineralogical should point out in what kind of rock and in what parts of the earth's crust ores and minerals are to be sought, and should save the expenditure of money in useless trials. The museum of the Royal School of Mines in Jermyn Street performs these functions. But they are valuable in a still higher sense as encouraging a love of knowledge for its own sake apart from any selfish aims. The visitors to the British Museum, however frivolous they may be, leave it all the better for having been there. It is impossible that they should not carry away some sort of idea, which otherwise would not have occurred to them, even if it be merely the recognition that outside their daily lives there is a world of knowledge vast and indefinite, but real and tangible. In this respect museums are educators of the masses, offering them a means of culture which would otherwise be out of their reach. And lastly, as instruments of training in natural history they are, as I have already observed, as necessary to the student as collections of books to the student in arts.

Natural history pursuits are in themselves one of the forms of higher education, and one that is especially adapted for the culture of the lower, sometimes falsely termed the working classes—as if the higher classes worked neither with head nor hand. In proof of this I may quote the following example, which I am free to mention by the death of the man to whom it relates. Some years ago a mechanic, one of the evening class students at Owens College, took me to see a collection of fossils made by "a hand" in a cotton-mill at Oldham. To my astonishment I found that it consisted not merely of fossils *au naturel*, shells, and the like, but of those of coal plants, polished, and in many cases cut into slices so as to show their minute structure. This had been done by rubbing them down on the kitchen floor, cementing them to a piece of glass, and then grinding them until they became transparent. The care and labour implied in a process of this kind can only be estimated by those who have tried it. But it was necessary to have a microscope to see them, and I actually discovered that the instrument which was given me to use was made by the man himself, who could not afford to buy more than the lenses, which he mounted in tubes that were made to slide in each other after the manner of a telescope. He was also a good local botanist. His collection of fossils, along with another made by a friend of his under similar circumstances, furnished the materials on which Prof. W. C. Williamson has to a great extent founded his admirable memoirs on the coal-plants, now being published by the Royal Society. From time to time I saw a good deal of my friend, and a man more completely lifted out of the usual level of his class into what I may call the unselfish horizon I never met. This could be traced directly to the scientific pursuits to which he was led by seeing somebody one day pick up a piece of coal shale, and hearing him say that there was a fish scale in it. He disbelieved this, examined for himself, took to collecting, and ultimately became what he was, devoting his early mornings and his late evenings not merely to collecting but to knowing. His knowledge embraced other things than natural history. James Whittaker, of Oldham, may be taken as a type of the effect of natural history in elevating a man's character. He is the representative of a small, though very important, body in the Northern Counties, a body which would be largely increased by the foundation of museums of the right sort. From personal contact with men like him I have arrived at the conclusion that in this direction we have a means of spreading culture among the intelligent mechanics, artisans, and mill-hands, who go neither to church nor chapel, who do not read very much, and very often have no aims higher than those of the mere animal life. Had they access to museums on holidays and in the evenings, I am sure that the receipts of public houses would ultimately be lessened. At present they have few recreations and little chance of self-improvement; for the so-called mechanics' institutes, which were originally intended for them, have generally passed into the hands of the class immediately above them.

W. BOYD DAWKINS

PHYSIOLOGICAL ÆSTHETICS

Physiological Æsthetics. By Grant Allan, B.A. 8vo. (London: Henry S. King and Co., 1877.)

WE have here a little work of some 300 pages, which deals with the philosophy of æsthetics almost exclusively on its physiological side. Of course, in thus restricting his subject, the author neglects all the more subtle and intricate parts of that philosophy; but every competent reader will agree with him that it is desirable, for the purpose of analysis, to separate as distinctly as possible the physiological from the psychological elements of æsthetics. For although the two classes are intimately blended in reality, this only makes it the more desirable to eliminate the one from the other in our analysis; so that we may perceive, as clearly as we can, how much of the total effect which our æsthetic consciousness supplies admits of being resolved into simple constituents, and how much remains over as complex constituents. Now in this respect Mr. Allan has profited well by the experience of previous writers; for while he treats his subject very thoroughly so far as it can be treated on the lower basis of physiology, he never permits himself to be tempted into the alluring superstructure of pure psychology. So rigidly, indeed, does he "stick to his text," that an uninformed reader might peruse the whole essay, and scarcely receive a hint that there is such a thing as "the association theory" in existence; while the names of Burke, Reynolds, Alison, Knight, Stewart, and Jeffrey are not even once mentioned.

The scope of the treatise being thus carefully confined to the more simple factors of our æsthetic emotions, space is afforded for a full exposition of numerous facts and theories relating to this important sub-division of psychological science. And, on the whole, the work has been well done. The arrangement is good, the style admirably lucid, and the spirit throughout scientific. True, there are no ideas of a strikingly original character; but a judicious compilation of facts already known, and a philosophical discussion of the more important theories which have been raised upon them, would be features in a work sufficient of themselves to make the latter a valuable addition to the literature of æsthetics. Mr. Allan, however, has done more than this. In his dedication he characterises his work as a "slight attempt to extend in a single direction the general principles which he (Mr. Herbert Spencer) has laid down;" and in this attempt we must allow that our author has been successful.

Setting out with the object of "exhibiting the purely physical origin of the sense of beauty, and its relativity to our nervous organisation," Mr. Allan begins by "investigating the nature of Pleasures and Pains generally." The most important part of this discussion is that in which he criticises the law thus enunciated by Prof. Bain: "States of pleasure are concomitant with an increase, and states of pain with an abatement, of some, or all, of the vital functions." On this law it is remarked, with justice, "in its endeavour to be antithetical, it misses the real relationship between the two states. If pleasures were the psychical concomitants of an *increase* of the vital functions, then our two greatest, if not our only pleasures ought to be digestion, and repose after exertion. . . . Mr. Bain has sighted this difficulty, but, not perceiving its full force, has endeavoured to avoid it by a supplementary theory of stimulation, which appears to me far more important than his main law. I believe the true principle of connection to be this: Pleasure is the concomitant of the healthy action of any or all of the organs or members supplied with afferent cerebro-spinal nerves, to an extent not exceeding the ordinary powers of reparation possessed by the system. And just as the two laws are not exactly antithetical, so too the feelings themselves are not directly and absolutely opposed to one another as will be seen in the sequel . . . In short, it will be seen that while Prof. Bain refers pleasure to an *increase* in the efficiency of the organism, it may better be regarded as the concomitant of a *normal amount of activity* in any portion or the whole of the organism." Thus "every activity when not excessive nor of a sort to prove destructive of the tissues, is doubtless in itself faintly pleasurable . . . but owing to the commonness and faintness of the feeling, we habitually disregard it." Nevertheless, when the whole organism is "under the influence of abundant food and good rest, the general stimulation of the nerves produces a consciousness of massive pleasure." Moreover, "the special stimulation of a single organ whose periods of activity are long intermittent, and which is at the culminating point of its nutrition, produces consciousness of acute pleasure." From considerations such as these, illustrated by a large number of subsequent examples, there is deduced the general formula, that "the amount of pleasure is probably in the direct ratio of the number of nerve fibres involved, and in the inverse ratio of the natural frequency of stimulation." Hence it is that the possible intensity of pleasures can never approach the possible intensity of pains; for while the organism, or parts of it; may be reduced or injured to a large extent before loss of sensibility supervenes, "efficient working cannot be raised very high above the average." Hence, too, "the æsthetically beautiful is that which affords the maximum of stimulation with the minimum of fatigue or waste, in processes not directly concerned with vital functions."[1]

Such may be said to be the foundation on which the present system of "Physiological Æsthetics" is raised. Thus, to select a few among the copious illustrations which are offered:—"The vulgar are pleased by great masses of colour, especially red, orange, and purple, which give their coarse nervous organisation the requisite stimulus; the refined, with nerves of less calibre but greater discriminativeness," require delicate combinations of *complementaries*. Similarly in music, the complex *harmony* of a Bach's fugue pleases the cultured ear, while a chorus of Offenbach, or the boisterous *melody* of a comic song, is more gratifying to the common people. Again, the æsthetic superiority of musical tones over mere noises is naturally explained by the fact, that "while the nervous apparatus for the perception of the latter receives frequent stimulation, each portion of the nervous apparatus for the perception of the former is comparatively seldom stimulated." Similarly, of course, simple tones are musically "poor," because they "can only arouse a sympathetic vibration in a single one of Cortis's organs;" while tones rich in harmonics are musically "full," because they stimulate a correspondingly greater number of Cortis's organs. *Beats*, again, are disagreeable, because "the ear is conscious of each separate interruption of the tone, and each subsequent reinforcement," thus receiving a *destructive* amount of intermittent stimulation. Similarly, though in a lesser degree, with *dissonance;* and similarly, too, with the optic nerve, when flashes of intermittent light follow one another too rapidly for the receptive material

[1] The latter qualification arises from a lengthy discussion in which Mr Spencer's view as to the origin of the Play-instinct from a superfluity of nervous energy is explained. This is explained in order that the æsthetic feelings, which by the theory are supposed to have a similar origin, may be analytically differentiated from the playful feelings—the distinction between Art and Play being supposed to consist largely in the fact that while the latter has reference to the over-fed motor fibres, the former has reference to the over-fed sensory fibres, "the organs of sight, hearing, &c.," or "the passive side of our nature"

to undergo repair during the intervals of darkness. The æsthetic superiority of the analytic colours over black, white, and grey is explained by considerations analogous to those which have just been mentioned in the case of musical tones and noises; while harmony of colours is treated in the same way as harmony of sounds. A somewhat curious speculation is ventured to explain the apparent deficiency of the red-perceiving elements. "It is clearly desirable that the eyes of the frugivorous animals should be *pleasurably* stimulated by reds, oranges, and purples; and the simplest contrivance for effecting this end would be to give the greatest possible rest to such elements as answer to stimulations of these orders. Accordingly, they ought only to be excited by comparatively powerful stimulations of their proper kinds."

Adopting Mr. Spencer's view[1] as to the ideal being a faint central stimulation of such nerve-fibres as would receive strong peripheral stimulation by the reality, Mr. Allan carries his analysis to the limit where "Physiological Æsthetics" must end, and where Psychological Æsthetics ought only to begin. Space, however, will not allow us to follow him into this division of his subject. Enough has been said to show that his work deserves the attention of psychologists; and it may be added that as he throughout clearly explains both the physics and the physiology of his subject, his entertaining little treatise will prove instructive to any general readers who may be desirous of observing the intimacy of those relations between psychology and the lower sciences, which the magnificent generalisations of recent years are now every day bringing into clearer prominence.

GEORGE J. ROMANES

OUR BOOK SHELF

Select Plants readily Eligible for Industrial Culture or Naturalisation in Victoria, with Indications of their Native Countries and some of their Uses. By Baron F. von Mueller, C.M.G., F.R.S., &c. (Melbourne: McCarron, Bird, and Co.)

THIS is another form of Baron Mueller's numerous and widely-spread contributions to the Acclimatisation Society of Victoria—numerous we say, because the Baron's pen is always at work upon botanical matters, the consideration of useful plants being apparently one of his favourite themes, and widely spread, because these papers on "select plants" seem to have been freely distributed not only in Australia and in this country, but also in America, where indeed some portion, if not all, have been republished. The present issue, Baron Mueller tells us, is a rearranged and largely supplemented form, which has been taken up by the Government of Victoria, and published under their authority. The book, which numbers some 293 pages octavo, contains references to an immense number of plants, the information attached to each being brief but withal accurate. The generic and specific names are arranged alphabetically from beginning to end, and this arrangement is perhaps the best for general use. After the scientific name, the vernacular name is given, then the geographical distribution or habitat, followed by a note as to the nature of the plant, whether a tree, shrub, or what not, and finally a brief description of its properties and uses. As a proof that Baron Mueller has corrected this latest issue of his papers, down quite to the present time we may mention that under *Nicotiana tabacum*, Lattakia tobacco is included, and it is only within a comparatively recent date that Mr. Thiselton Dyer has shown this to be right, nearly all previous writers having attributed it to *N. rustica*. At the conclusion of the book a very good plan is adopted of classifying the plants mentioned under distinct heads referring to their uses; thus, under alimentary plants, the generic names of all such are placed; the same under dye plants, fibrous plants, and so on. A good index is given of vernacular names only, which is quite sufficient when it is remembered that the scientific names are arranged alphabetically throughout the book.

Notes on the Ancient Glaciers of New Zealand. With Map. By J. C. Russell. Reprinted from the "Annals of the Lyceum of Natural History." (New York: November, 1876.)

MR. RUSSELL was attached to the U.S. Transit of Venus Expedition, and finding himself stationed on the shores of Lake Wakatipu among the snow-fields and glaciers of the South Island of New Zealand he read what had been written on the ice-work of that region and supplemented his reading by the personal observations recorded in these notes. Though he does not add any important new fact to our previous knowledge he gives an interesting *résumé* of the physical geography of the glacier region, pointing out the evidence for the former greater extension of the ice-fields of New Zealand, and dwelling especially on the proofs of enormous erosion shown by the valleys and lake-basins.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Nectar-secreting Glands

I HAVE briefly described in vol. xv. of the Linnæan Society's *Journal*, the nectar-glands found at the base of the fronds of the brake fern (*Pteris aquilina*) which are visited by ants for the sake of their sweet secretion. This case seemed to me to show in a striking manner that extra-floral nectar-glands are not necessarily protective in function, because the fern has, in England at least, extremely few enemies. The following extract of a letter lately received from Fritz Müller (of St. Catharina, Brazil) is of considerable interest in relation to this subject. He states that "the honey-glands on our *Pteris aquilina* serve, without doubt, to protect the ferns from the depredations of the leaf-cutting ants (Œcodoma), as is the case with Passiflora, Luffa, and many other plants. The glands of the Pteris are eagerly visited by a small black ant, Crematogaster, of which the Œcodoma seems to stand in great dread. On the other hand, when no protecting ants are present, I have seen Œcodoma gnawing the young fronds; here, as in other cases, it is only the young leaves that stand in need of protection, the older ones not being attacked by the leaf-cutting ants." This fact might, no doubt, be used as an argument by those who believe that all nectar-glands were originally developed as protective organs, and this argument would have great force if it could be shown that *Pteris aquilina* is a form which has arisen in countries where protection is needed; but even in that case there would remain the difficulty of accounting for the continued functional activity of the glands in districts where no such protection is required. Or it may be said that in past ages the glands on our European Pteris served as a protection against enemies which have now become extinct. But here we are again met by the difficulty of accounting for the continued activity of the glands. It is characteristic of evolution that great changes occur in the functions of organs, and I think that it will generally be allowed that even the most beauti fully adapted apparatus must have originated in an organ performing some comparatively simple function. The question at issue may perhaps be stated as follows:—In the cases where the nectar-glands are now well developed has there been a special course of structural development in close relation with the need of the plant for protection? Has there been a course of evolution such as we may believe has taken place in the formation of the food-bodies in *Acacia sphærocephala* and *Cecropia peltata*, or should we not rather believe that the sweet secretion has been developed in connection with some unknown process of nutrition; according to this view, a well developed system of glands may continue merely performing some obscure excretory function, and consequently, although the presence of nectar-glands has undoubtedly been of the utmost importance in determining the survival of certain species, yet it is hardly fair to assume that all nectar glands were originally protective in function. For many plants secrete large quantities of sweet fluid, which serves no such purpose. This argument is given by my father in his "Effects of Cross and Self-Fertilisation" (p. 402). In addition to the facts there given in support of this view, a curious case described by Prof. H. Hoffmann may be mentioned ("Ueber Honigthau," 1876). He states that numerous large drops of sweetish fluid appeared on the under-surface of the young leaves of a camellia. He also alludes to a similar abnormal production of honey-dew on an ivy plant.

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[1] Here, as indeed in most other places, Mr. Allan does not express his obligations. Doubtless, having a psychological public in view, he thought it superfluous to state the sources from which such well-known conceptions have sprung; but as his work is in all other respects adapted to badly-informed readers, it would have been desirable, on their account, to have supplied these omissions.

In the case of introduced plants, we see how an already existing quality may, without any special course of development, become of vital importance to its possessor. Thus, Mr. Belt shows ("Naturalist in Nicaragua," p. 74) that the lime, *Citrus limonum*, is able to exist in a wild state, because its leaves are, from some unknown reason, distasteful to the leaf-cutting ants; whereas the orange, *C. aurantium*, and the citron, *C. medica*, can only survive with the help of man.

Fritz Müller concludes his letter with some curious facts on kindred subjects:—

"The extreme variability of the nectar-glands on the leaves of many plants, is a somewhat remarkable fact. Thus our Citharexylon has normally two large glands at the base of the leaves, but sometimes there is only one, and sometimes none at all; besides these there are small glands scattered over the surface of the leaf, the number of which varies from twenty to none. Similar variations occur in the nectar-glands of *Alchornea erythrospermum*, and of a Xanthoxylon. It seems to me probable that in all the cases at present known, these glands serve to attract protecting ants; and I here agree with Delpino, although I do not hold with him that caterpillars are the chief enemies which are guarded against by Pheidole and Crematogaster; but I think with Belt that these latter ants protect the plant against the leaf-cutting species. Indeed it is precisely those plants which are free from the attacks of ants that seem to be especially well fitted for caterpillars. Thus the larvæ of Gynaeria live on *Cecropia peltata*, those of *Epicalia numilia* on *Alchornea erythrospermum*. On the Cayien (?) whose leaves are furnished with nectar-glands, and are visited by protecting ants, the caterpillars of many species of Callidryas are found. Finally, as far as I know, all the larvæ of the genus Heliconius feed on Passiflora. Moreover, the same relation holds in the case of plants protected in other ways, for instance, by stinging hairs or by poisonous sap. How numerous are the larvæ found on the European stinging-nettle. In this country we find the caterpillars of 'Ageronien' on the stinging Dalechampia; and again those of some species of Danais on Asclepias, which is protected by its milky juice." FRANCIS DARWIN

Down, Beckenham, May 21

Quartzite Implements at Brandon

AT the recent conference held by the Anthropological Society on the present state of the question of the antiquity of man, the president, Mr. John Evans, referred to the finding of implements made of quartzite at Brandon, and remarked that as that rock did not exist in the neighbourhood excepting in the glacial drift, the implements must have been made from pebbles obtained from the glacial beds, and were therefore of post-glacial age. This statement was made at the end of the meeting when there was no opportunity of replying to it, and as its effect must have been great, I shall be obliged if you will allow one who believes that none of the palæolithic implements are of post-glacial manufacture to make some remarks upon it.

For a full description of the implement-bearing deposits near Brandon I must refer to an able paper by Mr. J. W. Flower in the twenty-fifth volume of the *Quarterly Journal of the Geological Society*. Gravel Hill, near Brandon, is an isolated hill rising to a height of 91 feet above the river, from which it is nearly a mile distant. It is covered with gravel which is mostly, and in some places entirely, composed of quartzite pebbles. Mr. Flower estimates that three-fourths of the whole are of quartzite. In this the gravel differs from that of other pits in the neighbourhood of Brandon, as for instance, that at Bromhill, which contains only one thirtieth part of quartzose pebbles.

At Gravel Hill, along with some hundreds of flint implements, four made of quartzite, similar to that of the pebbles, have been found. All the implements are usually found at the bottom of the gravel, and occasionally lie on the chalk. On the supposition that the quartzose pebbles, from which some of the implements have been made, were brought by ice in the glacial period, some such succession of events as the following must have occurred. 1. Ice, from the north, carried thousands of quartzite stones and deposited them in immense abundance over a limited area. 2. Man afterwards made implements from some of them. 3. The whole of the pebbles were rearranged and formed into beds of gravel with the implements at the bottom, whilst the distinctive character of the deposit was retained.

This ingenious but complicated theory is not necessary, for quartzose boulders and pebbles are found in deposits much older than the glacial period not very far away from the locality, and may exist beneath the drift close to it. It has long been known to geologists that there are many fragments of old crystalline rock in the upper greensand. They have been described by Mr. Bonney in his geology of Cambridge, and very fully by Messrs. Sollas and Jukes Browne, who state that fragments of gneiss, mica, and hornblende schists, talcose schists, granites, vein quartz, grits, quartzites, and slates are very numerous in this bed. It ranges northward from Cambridge, and is lost beneath the surface gravels and boulder clays, but it is not at all improbable that it may run along to the west of Brandon, and there contain even more quartzose fragments than in Cambridgeshire.

I may remark in conclusion that Mr. Flower, in his description, states, that he is disposed with the French geologists, to ascribe the outspread of the gravels to some powerful cataclysmal action, and that he does not know of any boulder clays in the course of the river from which such a mass of pebbles could have been derived. THOMAS BELT

Cornwall House, Ealing

The Migration of the Swiss Miocene Flora

WITH reference to the route the plants took which formed the European miocene flora, I should be glad to know *why* Dr. Unger considers it to have been from America to Europe. He says: "There is more than *one* reason for thinking that the centre from which our lignite flora has sprung was far away from Europe—in the southern parts of the United States" (*Journ. of Bot.*, iii. 17). He thinks that the living flora of that part of America is the lineal descendant of that which gave rise, by aid of "Atlantis," to the Swiss miocene flora. But is enough known of the miocene flora of the United States to infer this? Prof. Heer says that the methods of comparison he employed "incontestably prove that Switzerland was inhabited by types now scattered over every part of the world [agreeing in that respect with the existing Arctic flora], but of which the majority correspond with species of South U.S. of America; the Mediterranean region of Europe ranks second; Asia Minor, the Caucasus, and Japan third; the Atlantic Isles fourth, and North Holland fifth" (*Nat. Hist. Rev.*, 1862, p. 154, quoted by Oliver). Prof. Oliver and Sir Charles Lyell think that the route was by Japan, and not by the Atlantis; but still (Sir Charles, at least) *from* America *to* Europe. Heer, in his "Primæval World of Switzerland" (vol. i. p. 325, Eng. ed.), says the *Glyptostrobus heterophyllus* of Japan "has probably been derived from the tertiary species" [of Europe]. Similarly, in comparing the *Taxodium distichum miocenum* with that of America, he observes: "It is very interesting to find that the ancestors of the existing American swamp-cypresses were formerly spread over the whole of Europe, as far as 78° N. lat. Again, of *Sequoia Langsdorfii*, he observes: "It probably formed a zone round the whole earth in high northern latitudes."

Instead, then, of regarding either Switzerland or the South U.S. as a "centre," I would suggest that the miocene flora was uniformly spread over the whole of the regions bordering the entire Arctic circle, just as the Arctic flora is now; Asia, Japan, and North-West America being then continuous; that as the climate became cooler in the pliocene epoch, it was driven southwards along every meridian, its descendants now existing in the localities above mentioned. It will be seen that these form a belt all round the globe, roughly speaking, between the 30th and 40th parallels of latitude. Migration to some extent might have taken place along that belt, but the great migration I suggest was probably from north to south, and not from east to west, or *vice versa*.

The above theory is simply an adaptation of that given by Dr. Hooker for a similar dispersion of the existing Arctic flora, which replaced the temperate floras of Europe, Asia, and America; but which on the return of a temperate climate retreated northwards as well as up temperate and even tropical mountains, perishing, however, in the low lands.

GEORGE HENSLOW

The Fertilisation of Orchids

WHILE botanising this spring in Portugal, I was struck with the fact that scarcely one of the orchids—species of Ophrys principally—that I had collected for my herbarium, or examined in the field, seemed to be fertilised, for none presented the least indication of having had pollen applied to its stigmatic surface; and I examined flowers in every stage of expansion, from the opening of the bud to the withered and shrunk up floral envelopes. Each one, I remarked besides, contained its own pollinia, their caudicles in their respective glands and in their natural position. I was so struck with this, that one day (March 31) I gathered and examined forty-five different flower-heads, and of all these only one was found to have pollen-grains on its stigma, and all, the fertilised one included, had their pollinia intact. The locality was the Tapada d'Ajuda, or Royal Park, situated just outside the city walls of Lisbon, an inclosure containing many acres of land, clothed in spring with a rich flora, and a favourite entomological hunting ground, teeming with Coleoptera, Hymenoptera, and the commoner Lepidoptera. Several of the orchids contained aphides, and a few harboured a species of small red ant.

On April 21 I again made similar observations, selecting the same place, as it was most accessible to me, and because several orchideæ grew there in the greatest profusion. On this occasion I examined over thirty flowers—none of them the same as I had examined in March, for I had plucked these at the time, but there was not one that did not possess its pollinia, and, as on the previous occasion, none of them showed any appearance of having had pollen-grains in contact with their stigmas.

Yet the Tapada, during the spring, produces these orchids by the thousand, vigorous, healthy, sweet-smelling plants, and in sunny days its air is perfectly alive with insect life, as I have said, of every kind.

HENRY O. FORBES

Old Hall, Highgate, N.

New Meteor Radiant

THE "two remarkable meteors" closely following each other, observed by Mr. Hope on May 13 (NATURE, vol. xvi. p. 43), proceeded most probably from a shower in the small southern constellation Crater. Your correspondent describes the point of first appearance as a *little* south of Arcturus. If this means about 7° or 8° below that star, then the observed courses accord well with the centre indicated, which is apparently quite a new radiant. From the Italian catalogue of 7,512 meteor paths (observed by Schiaparelli, Denza, and others in 1872) which I have lately been reducing, I found this shower at R.A. 170°, Dec. 10° S. (near $\epsilon - \theta$ Crateris) for May 3–15 from nine meteors.

Ashley Down, Bristol, May 27 W. F. DENNING

OUR ASTRONOMICAL COLUMN

ANTHELM'S STAR OF 1670.—The small star which is very close to the position of this object, as determined from the observations contained in Lemonnier's "Histoire Celeste," deserves to be closely watched, as there is more than a suspicion of its variation within narrow limits. Thus in August, 1872, it was exactly equal to a star which follows it 12·5s. in R.A., 4′·9 to the north; while in November, 1874, it was as certainly fainter by half a magnitude. This small star precedes the variable S. Vulpeculæ 43·6s., and is north of it 2′ 11″; two other small stars follow less than 2′ from the parallel, 22·4s. and 30·6s. respectively.

Prof. Schönfeld found the place of Anthelm's star from the observations of Hevelius and Picard,

R.A. 19h. 41m. 37s. N.P.D. 63° 2′·3 for 1855·0.

The place of the suspicious star alluded to above is, for the same epoch,

R.A. 19h. 41m. 43·5. N.P.D. 63° 2′ 7″.

It was meridionally observed at Greenwich in 1872.

D'ARREST'S COMET.—As yet we hear nothing of observations of the short-period comet of D'Arrest, for which M. Leveau has worked so laboriously, with the view to facilitate its discovery at the present return. The intensity of light was at a maximum about May 22, but diminishes gradually during the summer. Nevertheless, early in August it is still of the same amount as when Prof. Schmidt discontinued his observations at Athens in December, 1870, at which time he stated he could have followed it longer but for the want of an ephemeris. Subjoined are the comet's calculated positions during the absence of moonlight in the present month, for Paris noon:—

	Right Ascension.	North Polar Distance.	Distance from the Earth.
	h. m. s.	° ′	
June 7	1 18 58	85 26·2	1·613
,, 9	1 25 14	85 11·8	—
,, 11	1 31 27	84 58·2	1·611
,, 13	1 37 35	84 45·2	—
,, 15	1 43 40	84 33·0	1·609
,, 17	1 49 41	84 21·5	—
,, 19	1 55 38	84 10·7	1·607

Though the comet will not arrive at its least distance from the earth during the present visit (1·396) until October 20, it passed its perihelion on May 10. The period of revolution is now 2,434 days, or 35½ days longer than at its last appearance in 1870.

THE D'ANGOS COMET OF 1784.—Perhaps no person who has been occupied in astronomical observation and calculation has obtained for himself, rightly or wrongly, a more unenviable notoriety than the Chevalier D'Angos, who, in the latter part of the eighteenth century, was possessed of a small observatory in the island of Malta. From the unusual character of some of his statements his name came to be associated with anything in the way of observation that appeared to be apocryphal, and we find not only Zach was in the habit of terming doubtful assertions "*Angosiades*," but even Pastorff, who himself put upon record more than one suspicious statement, appeared to consider that he was establishing the good faith of an observation of a comet in transit across the sun's disc by declaring that it was not an observation made "à la D'Angos." We pass over on this occasion the reported observations by D'Angos in 1784 and 1798 of a comet or planet upon the sun, with the view to presenting the reader with a brief outline of the actual state of a case that has been open to still greater suspicion, viz., his observation and calculation of what appears in some of our catalogues as the second comet of 1784; and we may be pardoned for bringing together here particulars which though probably known to those who have access to a good astronomical library, are not so likely to be within the cognisance of those who cannot command such a collection. And further, it is almost essential to bring the main points in the case into one view, to enable the reader to judge for himself whether D'Angos is deserving of the opprobrium which has been cast upon him or not.

Writing from Malta on April 15, 1784, D'Angos apprised Messier at Paris that he had discovered a comet in Vulpecula on April 11, and he inclosed two approximate positions observed on the mornings of these days. He stated that the comet was very small, without tail, and with only "a slight appearance of nebulosity." Messier did not receive this letter until May 14, when he sought in vain for the comet. Pingré who wrote before any suspicion had been raised with respect to D'Angos, attributed this to its having in the interval receded to too great a distance from the earth, or having attained too great south declination. It appears that Messier did not receive any further observations from Malta, but D'Angos some time afterwards communicated to him elements of the orbit, calculated by himself, and it was to be presumed with the aid of further positions. The observatory at Malta was burnt at a subsequent period, and the whole of the papers, &c., of D'Angos were stated to have fallen a prey to the flames, so that it was supposed in France that the observations were irrecoverably lost. Burckhardt had endeavoured by successive hypotheses to extract some idea of the nature of the orbit from the two rough observations which he had received, and as his results differed widely from those of D'Angos, and even the elements of the latter did not represent these observations, Delambre, at the instance of Burckhardt, wrote for further particulars. In reply, D'Angos stated that he had only saved from the fire his meteorological journal, in which, under date April 22, was mentioned an observation of the zodiacal light, without any reference to the comet, whence he concluded that on this date the latter was no longer visible.

This assertion will appear a most extraordinary one when it is stated that so far from the observations being lost, they had appeared in a memoir drawn up by D'Angos himself, in a periodical conducted by Bernoulli and Hindenberg, entitled—*Leipziger Magazin für reine und angewandte Mathematik*, Leipzig, 1786, where they were discovered by Olbers, as he mentions in a letter to Encke, inviting his discussion of them. Positions of the comet in longitude and latitude are there given for fourteen nights between April 10 and May 1, and they are followed by the elements of the orbit, which D'Angos says he had calculated from them.

Zach in 1812 had suspected that the observations of the second comet of 1784 were imaginary, and had suggested that the orbit should be omitted from the catalogues, but he adds as he had only great probabilities and moral, not mathematical, proofs to support his view, he did not insist upon it. To provoke an explanation, however, he states he had enveloped "ce mystère d'iniquité" in a problem in vol. iii. of his *Correspondance Astronomique*, where he printed a series of positions of a body, which he invited his readers to explain, and which puzzled Olbers and Bessel who failed, like others, to discover Zach's meaning. Burckhardt also on receiving intimation from Olbers of his having brought to light what purported to be the observations of D'Angos, remarked upon the importance attaching to the circumstance, since it might lead to proof that they had been fabricated.

It remains to describe in a future note or notes, the results of Encke's investigation and of later inquiries relative to the comet of D'Angos.

PROF. SYLVESTER ON TEACHING AND "RESEARCHING"

IN the address of Prof. Sylvester at the Johns Hopkins University, to which we have already referred, he spoke as follows on the above subject:—

Let me take this opportunity of making my profession of faith on a subject much mooted at the present day, as to whether the highest grade of university appointments should be conferred with or without the condition of teaching annexed.

I hesitate not to say that, in my opinion, the two functions of teaching and working in science should never be divorced. I believe that none are so well fitted to impart knowledge (if they will but recognise as existing, and take the necessary pains to acquire, the art of presentation) as those who are engaged in reviewing its methods and extending its boundaries—and I am sure that there is no stimulus so advantageous to the original investigator as that which springs from contact with other minds and the necessity for going afresh to the foundations of his knowledge, which the work of teaching imposes upon him. I look forward to the courses of lectures that I hope to deliver in succession within the walls of this university as marking the inauguration of a new era of productivity in my own scientific existence; nor need I consider any subject too low (as it is sometimes foolishly termed) for me to teach, when I remember to have seen the minutes of the conversation held between the delegates of the Convention, at the time of the French Revolution, and the illustrious Lagrange, the son of the pastry-cook of Turin, possibly the progenitor of the Marquis Lagrange, of turf celebrity (Citoyen Lagrange, as he is styled in the record), who, when asked what subject he would be willing to profess for the benefit of the community, answered meekly, "I will lecture on Arithmetic."

At this moment I happen to be engaged in a research of fascinating interest to myself, and which, if the day only responds to the promise of its dawn, will meet, I believe, a sympathetic response from the Professors of our divine Algebraical art wherever scattered through the world.

These are things called Algebraical Forms. Prof. Cayley calls them Quantics. These are not, properly speaking, Geometrical Forms, although capable, to some extent, of being embodied in them, but rather schemes of processes, or of operations for forming, for calling into existence, as it were, algebraic quantities.

To every such Quantic is associated an infinite variety of other forms that may be regarded as engendered from and floating, like an atmosphere, around it—but infinite in number as are these derived existences, these emanations from the parent form, it is found that they admit of being obtained by composition, by mixture, so to say, of a certain limited number of fundamental forms, standard rays, as they might be termed in the Algebraic Spectrum of the Quantic to which they belong. And, as it is a leading pursuit of the Physicists of the present day to ascertain the fixed lines in the spectrum of every chemical substance, so it is the aim and object of a great school of mathematicians to make out the fundamental derived forms, the Covariants and Invariants, as they are called, of these Quantics.

This is the kind of investigation in which I have, for the last month or two been immersed, and which I entertain great hopes of bringing to a successful issue. Why do I mention it here? It is to illustrate my opinion as to the invaluable aid of teaching to the teacher, in throwing him back upon his own thoughts and leading him to evolve new results from ideas that would have otherwise remained passive or dormant in his mind.

But for the persistence of a student of this University in urging upon me his desire to study with me the modern Algebra I should never have been led into this investigation; and the new facts and principles which I have discovered in regard to it (important facts, I believe,) would, so far as I am concerned, have remained still hidden in the womb of time. In vain I represented to this inquisitive student that he would do better to take up some other subject lying less off the beaten track of study, such as the higher parts of the Calculus or Elliptic Functions, or the theory of Substitutions, or I wot not what besides. He stuck with perfect respectfulness, but with invincible pertinacity, to his point. He would have the New Algebra (Heaven knows where he had heard about it, for it is almost unknown in this continent), that or nothing. I was obliged to yield, and what was the consequence?

In trying to throw light upon an obscure explanation in our text-book, my brain took fire, I plunged with re-quickened zeal into a subject which I had for years abandoned, and found food for thoughts which have engaged my attention for a considerable time past, and will probably occupy all my powers of contemplation advantageously for several months to come.

OUR INSECT FOES

AN important conference was held at the Society of Arts on Tuesday afternoon on the subject of insects injurious to agriculture and methods of stamping them out. Its origin was a proposition by Mr. Andrew Murray (who has had the arrangement of the collection of economic entomology made by the Science and Art Department), which he laid before the Lord President of the Council. The proposition was printed by order of the president, and copies were sent to the agricultural societies and chambers of agriculture of the country. After the proposition had been before them for two months and there was no indication of any notice being taken of it, it was arranged that a conference of delegates of agricultural societies should be held at the Society of Arts. The Duke of Buccleuch, K.G., took the chair, and there were present representatives of the Scottish, Cheshire, Warwick, Hampshire, and Banbury Chambers of Agriculture; the Farmers' Club, Dr. Maxwell Masters, representing the president of the Royal Society, Prof. Voelcker (chemist to the Royal Agricultural Society), Mr. Sewell Reed, M.P., &c. The conference was opened by a paper read by Mr. Murray.

The paper commenced by assuming as an axiom that, besides the occasional great injury done by insects, by which whole districts are ravaged, a continual drain is constantly kept up by them, which constitutes a very perceptible percentage of deduction from the cultivators' profits; and, further, that where this loss can be prevented at less cost than the loss it occasions, it should be prevented.

It next maintained that, if we wish to rid a district or a country of an injurious insect, to be effective, any attempt to do so must be simultaneous and combined, for to what purpose would it be if one man cleared his farm if his neighbour did not clear his; or if the one cleared his one year, and the other cleared his another? A central authority, therefore, is needed to secure united action.

It next considered the various ways in which the insects injurious to agriculture might be extirpated. The first, the simplest, the most powerful, and the most efficient of these is county or district rotation of cropping. Farmers know well enough the advantage of a rotation of cropping (or its equivalent) on their own farms. By long-continued growth of the same crop on the same land the soil becomes exhausted of some of the elements necessary for the proper development of that kind of crop, and a change of crop brings other elements into use, and relaxes the demand upon those that have been too much drawn upon.

Exactly the converse of this takes place with regard to certain insects. The great majority of vegetable-feeding insects do not feed on all kinds of plants indiscriminately; most of them are restricted to one kind of plant, and if by cultivation of that plant its numbers are enormously increased, so will naturally be the number of the insects that feed upon it; while, if we should cease to grow that plant, the number of the insects would correspondingly diminish. Thus, for instance, if a district is almost entirely in pasture, there will be very few wheat-feeding insects in it, but if it is turned into a wheat country they will be myriads. If these numbers reach such a pitch as to deteriorate the crops the remedy is plain. Change the rotation, and grow some other crop instead of wheat. Most of the wheat insects are only annuals. If they could be banished for one year they would be banished entirely, or until re-introduced. Now, if there were a controlling authority, what would be easier than to say to the farmers, "Gentlemen, in the common interest you will substitute barley for wheat in your next year's rotation." The insect, deprived of its proper nidus, must then either lay its eggs in an unsuitable place where they will perish, or have recourse to the pasture fields for *Triticum repens*, or other suitable grasses. By this, of course, the fly would not be exterminated, but its numbers would be so reduced as to render it comparatively harmless, at all events for a time, when, if it again reappeared in force, the same means of defence would be resorted to. Nay, it might be so arranged that two or more counties might brigade themselves together, so as to establish a permanent see-saw by which they should play into each other's hands. But no single man can carry out such a rotation. He may try it upon his own fields, but they will be replenished continually from the fields of his neighbours, unless they at the same time are compelled to follow the same rotation.

Mr. Murray then went over the various other means of extirpation—picking and burning infected plants, the collecting caterpillars, poisons, and local remedies, in relation to which he drew attention to the destruction of what are called ticks and lice upon sheep. Everyone knows how readily such vermin can be communicated by contact or even proximity, and it does seem a very hard case that a man, who has kept his flock clean by taking proper precautions, should be liable to have them infected by a neighbouring neglected flock, by stray sheep, or even by sheep passing along the road. It is said that, *cæteris paribus*, the difference in value between a sheep that has been kept clean for the season and one that has been worried by vermin will be 20*s*. If that is so it is a wonder that sheep farmers have not long since clamoured for some supervision.

At the conclusion of the paper the following resolution was put from the chair and carried:—"That thanks are due to the President and Lords of the Council for having brought the subject of insect damage under the consideration of the agricultural bodies of the kingdom."

Dr. Maxwell Masters moved the next resolution, and in doing so said he was charged to express the regret of the President of the Royal Society that he was unable to be present. He spoke of the great ignorance throughout the country on the subject of insect damage, and as an indication of the amount of damage done, said that half the time of the Scientific Committee of the Royal Horticultural Society was occupied with answering inquiries from all parts as to how to deal with insect foes. The resolution he moved was,—"That much of the loss occasioned by insects is preventible, and ought to be prevented." This was seconded by Mr. Maclagan, and carried.

Mr. Mechi then moved—"That it properly belongs to Government to provide the necessary means for protecting cultivators from this loss, as it is only by combined and simultaneous action over considerable districts that it can be effectually done, and Government alone possesses or can obtain the requisite means of enforcing such action."

Both Mr. Mechi in moving it and Prof. Voelcker in seconding it, spoke of the want of knowledge throughout the country on the subject. Mr. Sewell Reed urged it was not a question for government but for agricultural societies. The resolution was declared carried, though many hands were held up against it.

The last resolution was—"That the President and Lords of the Council and the Agricultural Societies of the United Kingdom be informed of the opinion of this Conference, and urged to take the subject at once into their consideration, with a view of providing a remedy," which, after a long discussion, was carried.

THE VOLCANOES OF ICELAND

DURING the past year the Danish Government despatched the well-known geologist, Prof. Johnstrup, to Iceland, for the purpose of making a thorough scientific investigation of the scene of the recent volcanic disturbances. A short time since he laid before the Danish Parliament a report of his journey, with a brief account of the results so far obtained. The first part of the expedition was devoted to the volcanoes in the Dyngju Mountains, encircling the valley of Askja, and was accompanied with many difficulties resulting from the conformation of the region and the prevalence of violent snow-storms. The mountains themselves are not of volcanic origin, but consist of basalt and palagonite-breccia. In former times the Askja Valley was evidently much deeper than at present. Repeated flows of lava have gradually filled it up, and these Prof. Johnstrup believes to have occurred within the historic period, although no mention of volcanic disturbances in this district is to be found in the annals of the island. Along the outer edge of the Dyngju Mountains are numerous craters, some of considerable size, which have contributed most of the lava covering the plain of Odadahrann to the extent of sixty square geographical miles. Part of this enormous quantity of lava had its origin in the neighbouring volcano of Trölladyngja. It is, however, sharply distinguished from the twisted, contorted, masses of the former, by its more regular character and smooth crusts. In the neighbourhood of the newly-formed craters the earth is covered to the distance of over a mile with the bright yellow pumice-stone ejected during the eruption of March 29, 1875. Most of the pieces are seven to eight inches in diameter; many contained two to three cubic feet. In places where the pumice-stone is several feet in depth, it covers a layer of snow twenty-five feet deep, which fell in the winter of 1874-1875, and has been protected from the effects of solar warmth by the feeble conductive power of the pumice-stone. It is fortunate for the land that the outbreak was of this nature, for from its lightness the pumice-stone can be easily removed from the surface of the country. The party examined the most northerly of the craters, which was 300 feet wide and 150 feet deep. It was filled with steam, which was driven out with such force as to give rise to a most deafening roar. No solid matter, however, was borne along with the vapour. Not far from the crater an extensive depression in the valley of Askja has taken place, and the fresh surfaces of rock exposed thereby give a clear picture of the peculiar formation of the valley by successive deposits. It presents a remarkable similarity to the basalt and dolerite formations so prevalent in the mountain ranges of Iceland.

The Oskjagja. (From Watts's "Across the Vatna Jökull.")

The most surprising feature of these late eruptions was the ejection of such enormous masses of pumice-stone, while not a trace of a lava stream is to be found. A similar outbreak is not mentioned in the records of the island. On account also of the vast development of steam, which gave rise to the pumice-stone formation, they are without a parallel amongst volcanic phenomena. At present the craters are to be regarded as gigantic steam escape tubes, the activity of which will continue for an uncertain period, but with gradually decreasing intensity. As long as these safety valves remain open it is not probable that a repetition of the eruptions will occur in the immediate future.

The volcanoes in Myvatns Öræfi were found to present entirely different characteristics. This barren plain is about thirty-five miles long and thirteen miles wide. Suddenly, on February 18, 1875, a volcano appeared in the centre. Four others appeared at subsequent dates, all of the craters falling into a straight line north and south. No eruptions have occurred here within historic times. The mass of lava which issued from these various craters is estimated at 10,000,000,000 cubic feet, eighteen times the amount supposed to have been emitted by Vesuvius in 1794 and 1855. The lava was basaltic and viscous when emitted, and crystals of chloride of ammonium were found in the vicinity of the craters. Only slight traces of the volcanic action remain now, where warm air arises from the thicker layers of lava.

Prof. Johnstrup is engaged at present in the preparation of maps showing the successive deposits of lava from the older eruptions, as well as from the more recent. The Hlidar range, hitherto regarded as a palagonite formation, was found by him to consist of trachytic masses, a more ancient, and in Iceland rarer, formation than palagonite.

In connection with Prof. Johnstrup's Report we may refer to Mr. Watts's interesting narrative of his journey across the Vatna Jökull.[1] Mr. Watts's name is already well known in connection with recent exploration in Iceland. He has for long had a strong desire to cross the Vatna Jökull, and at last succeeded. We infer—for his narrative is almost innocent of dates—that the feat took place in the summer of 1875. The preparations made remind one of those necessary before setting out on an Arctic expedition, and the whole journey bore a strong resemblance to those sledge journeys we read of in connection with the recent polar expedition. There were sledges, tent, sleeping bag, pemmican, and similar stores; frost-bites, snow-storms, and weary detentions for favourable weather and ground. The Vatna Jökull, we learn from Mr. Watts, is a vast accumulation of volcanoes, ice, and snow, covering an area of over 3,000 square miles in the south-east of Iceland. It is a plateau of from 4,000 to 6,000 feet high, is surrounded on all sides by volcanic mountains, and gives birth to glaciers on various sides. On the south especially it seems to be advancing, and there the glacier may soon reach the sea and give birth to miniature icebergs. Mr. Watts crossed at the east side, and after suffering considerable hardships he and his party reached the farm of Grimstadr, in the north of Iceland. From here Mr. Watts returned southwards to the northern edge of the Vatna Jökull for the purpose of examining the Oskjagja, a huge and active crater on the south of the Askja, or Dyngjufjall, referred to by Prof. Johnstrup. Mr. Watts gives many interesting and important details concerning this mountain and the desolate country in its vicinity, covered with pumice dust and other products of eruption. Mr. Watts also visited the region around the Myvatn Lake, near which are the sulphur deposits which a company was started to work. After visiting one or two places on the north coast he returned to Reykjavik right across the centre of the country. Notwithstanding the defects of style, the want of dates, and occasional vagueness, Mr. Watts's narrative is a really valuable and interesting contribution to a knowledge of the physical geography of Iceland, and he has the honour to be the first, so far as known, to have crossed the great Icelandic waste.

THE ANTIQUITY OF MAN

IN the number for May 24 we gave abstracts of the papers read by Professors Dawkins and Hughes, and Mr. Tiddeman at the Conference on the subject of the Antiquity of Man at the Anthropological Institute, and this week we give a report of the discussion which followed the reading of these papers, the remarks of the various speakers, we may state, having been revised by themselves.

Prof. Busk wished to explain, before the discussion commenced, the circumstances connected with the interesting fragment of bone, for the determination of which he was personally responsible. This "bone of contention" was represented by the cast which he held in his hand. He was surprised that such a large superstructure had been raised upon that particular piece. It was merely a fragment, evidently of a fibula, one of the most variable bones in the body. It was received by him, together with a large collection of other remains from Mr. Tiddeman, and for a long time remained an insoluble problem. At last, after many conjectural determinations by himself and others, Mr. James Flower, the well-known articulator to the Royal College of Surgeons, discovered in the College a human fibula of unusual size, and with which, as he pointed out, the Victoria Cave bone corresponded in many particulars. This determination, with the reasons for it, and illustrated by figures, was published in the *Journal* of the Institute. At the same time Mr. Busk was perfectly open to be convinced that it might be ursine. But although Prof. Boyd Dawkins had been good enough to show him bones of fossil bears of surprising size, none of them quite came up to the one in question. Nor at Toulouse, where there is such an enormous collection of ursine remains, did Mr. Busk observe any of corresponding dimensions. He was himself still disposed to regard the specimen as a fragment of an abnormally large human fibula, but thought that at present it would be unsafe to build any strong conclusions upon it.

Prof. Rolleston stated that in digging out a British skeleton he came upon a fibula standing vertically. They went on and he took out every bone with his own hands and they came to a skeleton, contracted in the ordinary British way, which was whole, minus that one fibula. A man is put into the ground with his flesh and bones all upon him, the flesh decays, the stones get upon him, the bones are loose and consequently the fibula gets disturbed. Even granting that the one before them was a human fibula he would lay less stress upon it than on any other bone. In the Gibraltar Cave series the fibulæ, owing to their liability to displacement, were very often missing. He did not in the least dispute the antiquity of the deposits in the Victoria Cave. With respect to the reindeer and the hippopotamus, they might judge something from what they saw in the life and in the flesh. He had seen the hippopotamus walking about in very cold weather in the Zoological Gardens seeming extremely comfortable, and the rhinoceros and reindeer the same. Mr. Evelyn, of Wotton, had kept reindeer alive for considerable periods in England. At the time of Julius Cæsar the reindeer lived in Germany. At the present time the reindeer was the food of the tiger in the Isle of Saghalien, North of Japan. There the tiger, which has a black and thick fur, crosses the ice after the reindeer. The skull of a young hippopotamus was found in England, showing that the hippopotamus really did live here and breed here too. Hence, mammals were not good indicators of temperature.

Prof. Prestwich referred to the observation of the president, that to consider the present subject thoroughly required the knowledge of the palæontologist, the anthropologist, the archæologist, and the geologist. He thought that it specially concerned the geologist with regard to the sequence of events. The palæontological evidence hardly presented sufficient differences. We had to deal with the sequence of man from his first appearance in time geologically to the present period. He would confine himself to the evidence in the south of England and in the north of France. In the south of England it was particularly clear and decisive; the datum line was distinct. It was afforded by the deposit of the boulder clay, which ranged as far south as London. That represented the glacial period. The post-glacial period he considered to be subsequent to the period of the deposit of the boulder clay. Most of the discoveries made in this country have been made in the districts of the south which have been covered by the boulder clay, and it is in the drift and gravel of the valleys excavated in the boulder clay of those districts that the flint implements have been so largely found; therefore he believed that in all that area man is of post-glacial age. If we got two levels on either side of a valley, so many feet above sea-level, with the boulder clay cut off on either side, then of course the *débris* at the bottom of the valley would consist of gravel, and so on, derived from materials which had been formed by the destruction of the several strata which originally traversed that valley. The materials so spread out were necessarily newer than the boulder clay; consequently man in the valleys was post-glacial. There were sometimes two or three successive levels of gravels in those valleys. If a valley was excavated to a certain depth, and a deposit was formed in which they could find no traces of the existence of man, whilst at another and deeper level flint implements were found, then man was introduced in that place only when the valley was excavated to its greatest depth and the gravel was spread out on the site now nearly occupied by our present rivers. Unfortunately the mammalian remains of those nearly connected periods were so alike that it was impossible to determine from the distinction of age. Bone caves were also found on the sides of valleys and in districts where there was scarcely any boulder clay, and we were then left to the palæontological evidence. With regard to the possible correlation of other deposits found in the south of England with the deposits which preceded the glacial period in the north, there was evidence in both areas of the land having been inhabited previous to the boulder clay period by animals which were likely to serve as the food of man. There was no *à priori* reason why man should not have existed before that period. Much would have to depend upon that complete palæontological evidence which possibly Mr. Tiddeman might have at some future period to bring before them rather than upon geological position. He was disposed to consider with Mr. Tiddeman that the cave he was now investigating might be of pre-glacial age. He thought that the evidence rather tended to show it was pre-glacial, but it was not conclusive. What might be decided upon that particular point must, however, depend upon further research. Taking again the valley of the Thames, we found flint implements in terraces raised some twenty, thirty, or forty feet above the present level of the river. At Reculver we found such evidence of the existence of man in a gravel eighty feet high, but as we ascended the valley we found the flint implements confined to the lower levels. At Reading no flint implements or mammalians are found in the high-level gravel. So also in the neighbourhood of Oxford mammalian remains and implements are found in the low-level gravel but none in the higher. Thus at the entrance of the Thames valley near to France we find evidence of man in the later high-level gravels, but man had not then penetrated into the Upper Thames valley. It was evident that at the period that those higher terraces were deposited in the upper valley of the Thames as far down as Maidenhead, very cold conditions pervaded, though post-glacial to the boulder clay. In the neighbourhood of Oxford there have been found in this upper gravel boulders of several tons in weight which had been carried from a very long distance, and he had recently observed in the neighbourhood of Reading some high-level gravel resting upon an ice-pitted surface of stiff clay in which there was no calcareous matter, presenting that sort of section (drawing it on the black board). A surface the size of that room was exposed. It seemed to him, however, that with respect to pre-glacial man there was an important "suspense account" now accumulating. In France an large series of observations had been made by competent observers, and it would not do to ignore the points they had brought forward. He had some reason now to believe from his own observations that there was evidence of man being pre-glacial even in the north of France. He also produced one specimen from the Red Crag which had been in his possession for many years. He could not answer for the labelling but only for the locality and the condition of the bone, but from the peculiar way in which it had been cut and then broken it had all the appearance of having been artificially worked, but he should certainly only put it to a suspense account. With respect to one observation of Prof. Dawkins, that the oldest implements were ruder than the newer ones, he would remark that one cause why the implements of Creswell Cave were so rude was because they were made of quartzite, which could not be finished in the same way as flint. At Amiens the older high-level implements were often more finished and finer than those of the low-level gravel.

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[1] "Across the Vatna Jökull; or, Scenes in Iceland," by William Lord Watts. (London: Longmans and Co.)

Col. Lane Fox wished to say a few words upon a point not yet touched upon in any of the papers which had been read, viz., the means by which valleys had been eroded, and the time necessary to accomplish it. The uniformitarian theory, by which it was assumed that all the work of excavating valleys had been performed by means of their rivers flowing under the same conditions as at present, had been a good deal modified of late years, and he thought he could add a few facts from personal observation tending to show that some modification of the theory was necessary. With respect to the valley of the Somme, there was evidence afforded by relics of the Roman and bronze age found in the peat in the bottom of the valley, that the river had not materially lowered its bed since those relics were deposited, and therefore it must have taken an enormous time to work out the whole valley by means of a river which flowed with the same eroding power as at present. The valley of the Somme, however, was so comparatively narrow that it was possible the whole of it might have been eroded by such means, if sufficient time were allowed. But if it could be shown that the same conditions prevailed in other very much larger valleys where the work to be done was much greater, that would afford fair presumptive evidence that the eroding force must have been greater. He could mention one or two facts which showed that the Thames like the Somme had never shifted its bed since the bronze period. The first of these was that the river some way below Oxford, at the village of Dorchester, made a great bend; the ground on one side was high, and on the other, in the space inclosed by the bend perfectly, flat and low; there was an ancient intrenchment running across this low ground from bank to bank, and converting the promontory formed by the bend of the river into a fortress. It had been ascertained by means of the relics, consisting of pottery, flints, bronze implements, &c., associated with this intrenchment, that it was certainly as early as the bronze period, and perhaps earlier, no relic of Roman work having been found there, although Dorchester, close by, was a Roman station. The intrenchment in order to serve its purpose must have rested its flanks on the river at the time it was made, and the fact of their resting on the banks at the present time, although they are only a foot or two in height, showed that the river had not shifted or lowered its bed since the bronze age. Other evidence giving the same results was found in the same river lower down. Between Richmond and Battersea the Thames makes three or four bends in the comparatively flat bottom of the valley which is here more than four miles wide. He had found flint implements of the drift type deposited in sedimentary sand and gravel at Acton eighty feet above the present river, the discovery of which was communicated by him to the Geological Society and published in their journal. The river then since these implements were deposited must not only have lowered its bed eighty feet, but, according to the uniformitarian theory, must at each successive level have shifted its bed repeatedly so as to work out the valley here more than four miles wide. Yet bronze and stone implements have been found in considerable numbers in all the various bends of the present river dredged up from the gravel at the bottom by the dredging machines that have been employed of late years, and proving that the river had neither lowered nor shifted its bed since the bronze period, but if anything it had risen since that time. Was it possible, he would submit, that at this rate of progress, if progress it could be called, the erosion of the valley could be attributed to the present river flowing under the same conditions as at present? But if, as believed by Prof. Boyd Dawkins and Mr. Tiddeman, man existed in these parts during the subsidence of the glacial epoch, that would account, he thought, for a much greater flow of water having passed down these valleys in palæolithic times than was the case at present. In the valley of the Solent the same class of evidence was obtained. Mr. Evans had shown what a large amount of depression and erosion must have taken place in this valley since drift implements were deposited on the hill at Southampton. The valley of the Solent, from Portsdown to the Isle of Wight, is nine miles wide, and we have evidence in the Roman fortress at Porchester how little it has changed in modern times; yet in the centre of this valley near Southsea common, Col. Fox had some years ago discovered a flint station of the neolithic age, including celts, scrapers, and flakes in great abundance, the site of which was less than ten feet above the present high-water mark, showing that flint implements continued to be fabricated in the valley after land and water had assumed its present distribution. All these facts, he thought, favoured the opinion that powerful eroding forces must have been at work before that time. The very valuable papers which had been read treated only the geological aspects of the question, but as the President had observed there were ethnological and sociological problems to be solved, how long would it have required for the various races of man to diverge, and the earliest traces of culture to be evolved? He trusted that even if no other result came of the conference it would show that we had not yet exhausted the subject.

Prof. A. H. Sayce had to confess that the evidence of language as regarded the antiquity of man was not so decisive as that of geology. Under certain conditions the vocabulary of a language changed rapidly, under other conditions it changed slowly. The grammar of a language may be said to change never, and its structure to change very rarely. If these conclusions were applied to two or three of the principal families of speech, the results would be something like this: Take the Semitic class of languages; by means of the Assyrian monuments we are able to get back to 2000 B.C. for a starting point, when those languages were pretty much as they are today. Scarcely any of the structure, or grammar, or vocabulary has changed, but it is plain enough that they pre-supposed several earlier stages of existence, and when compared with the grammar of the old Egyptian there was a time when the parent language seems to have been the parent also of the old Egyptian. But in order to allow for the changes that had taken place in the structure of the Semitic languages, and the structure of the Egyptian language, we must assume a very great period of time. With regard to the Aryan family, the different dialects could be traced back to the parent speech spoken in some part of Western Asia. That parent language could be restored by comparison with the later languages and dialects. In all points that parent speech was as fully developed as Sanscrit, or Greek, or Latin, the people who spoke it were in an advanced stage of civilisation, and the language itself was in a highly advanced condition. When the grammatical details of the language were analysed, it became quite plain that it was the product of a long series of successive stages of growth. Take another language, the old language of Chaldea. The earliest monuments that contained that language were between 3000 and 2000 B.C. On these monuments the language appeared in a stage of the most utter decline and decay. Therefore there was evidence of a language which had behind it a long and undetermined past. If, as several scholars believed, that language belonged to the Ural-Altaic family, in order to get back to a period when those languages were one and the same, they must suppose an enormous period of time. There was another consideration connected with the evidence of language. It would seem that most languages, whatever their present structure might be, were at one time in a condition similar to that of the Esquimaux language at the present time, that is to say, a time when as yet the single word is not distinguished from the sentence as an independent unit, but forms part of the sentence in which it is embodied. In the case of languages so highly developed as, say, the Aryan languages, in order to get back to a time when those languages were in a condition similar to the present condition of the Esquimaux language, they must allow not hundreds but thousands of years. Those were the conclusions to which the present investigations of language would appear to point.

Mr. T. K. Callard, referring to the outline of the head of a horse, drawn upon a bone represented as belonging to the palæolithic age, found in association with extinct animals, said they had always been led to think that palæolithic man was a rude savage who could only chip his flint implement, but who could not smooth it (that would indicate the neolithic period), but they were now getting evidence of a different character. They had heard of a bone needle being found in the cave-earth, which at once suggested a step in civilisation, as men did not make bone needles unless they intended to use them, and that would lead their thoughts to a palæolithic tailor. In that very cave were found traces of a no mean artist, for not one man in three at the present time could make a sketch like that of the horse. It struck him that that Royal Academician of the palæolithic age had for his model a horse with his mane *clipped*, which indicated another stage of civilisation. Were they justified in saying that because the remains of mammoth and woolly rhinoceros were found in close proximity to the remains of man, therefore man lived at such a remote period? He was inclined to think that it proved not so much the antiquity of man, as that the extinct mammalia were more modern than they are supposed to be. The works of man being found with the remains of the extinct mammalia, tells nothing of the period of man's existence, unless it is also proved when the mammalia referred to became extinct; of this there was no proof adduced, and therefore, to his mind, the argument for man's antiquity based on the contemporaneity of man and the extinct mammalia has not been sustained.

Mr. Harrison said the palæolithic character of the flint implements found at Cissbury in connection with the remains of existing fauna, including goat and pig, showed that the form and finish of prehistoric tools and weapons were not of themselves a safe criterion of age. Though the earliest implements would necessarily have been the rudest, the converse was by no means true. There were doubtless art-centres in early times, as there are now, and Cissbury would not appear to have been one of them, but rather belonged to the far larger class of village manufactories. Some of the pits, he wished to say as the result of personal observation, may have been opened but a short period before our era. Their age does not directly affect the question of the antiquity of man in this country, which depends for its solution on geological facts.

The President, Mr. Evans, in summing up, said the questions principally discussed were—In the first place were they to assign any implement found in this country to a pre-glacial or inter-glacial period? or must they restrict them to a post-glacial period? Some of the implements found in the river gravels were made from stones derived from glacial drift, and were therefore clearly post-glacial. The characteristic forms of the implements gave a guide by which they might fairly argue that others of a similar character belonged approximately to the same date. Some implements were very persistent in their type; but if in a certain part of England post-glacial implements were found associated with a certain fauna, and in another part the same forms of implements were found alone, these also would appear to be post-glacial. There were certain distinctions to be pointed out in cave-deposits. In the cave described by Prof. Dawkins there were a succession of beds, and he thought it was in the upper beds of more recent date that the relics of the tailor and the artist were found. Looking at the enormous lapse of time comprised in the palæolithic period, which was evidenced by the amount of time requisite for the erosion of river valleys, he thought they would eventually be able to establish some chronology. If they could form any idea of the amount of time requisite for the excavation of a valley such as the valley of the Thames, they could approximately estimate the antiquity of man in this country, but for the last 2,500 years the variation of the river bed and its level were practically nothing, and therefore they were entirely at a loss without falling back on some hypothesis as to variations in the climate. It was difficult to say with certainty whether the implements discovered abroad in reputed miocene and pliocene beds were of necessity worked by the hand of man, and whether they had in all cases been found under the circumstances which were attributed to them. With regard to the other deposits by which the early existence of man had been traced, such as the skull alluded to by Prof. Rolleston, if it was found with a highly-finished spear-head, he (the speaker) could not regard it as of pleistocene date. The evidence of cut bones was by no means satisfactory. Some of those incisions were probably induced by natural causes. Some present might remember a pair of horns of an Irish elk which by mere pressure were embedded in each other. Still, all such evidence should be carefully collected, and it would become to a certain extent accumulative. The question as to the distinction between the glacial period in the South of England and that of the North was of very great importance. If geologists carried back the early appearance of man in this country to a time but little removed from the glacial period, they might safely infer that he must have existed in other parts of Europe at a much earlier period.

As this interesting discussion could not well be postponed, and as the time at the disposal of the Conference was necessarily brief, it now only remained for the three principals to reply to any objections that may have been raised to their statements and arguments.

Prof. Boyd Dawkins said that the first point to be considered was the antiquity of man in the Victoria Cave, based upon a small fragment of fibula, and two fragments of goat's bones which presented the appearance of having been cut. The fibula seemed to him to be ursine rather than human, and in size came within a very little (two-tenths of an inch) of the circumference of one of *Ursus spelæus* from Lozère. With regard to the goat's bones, he shared the opinion of Mr. Davies, of the British Museum, that they are not fossil, but recent, in other words, he did not believe that they were originally imbedded in the stratum containing the remains of the hyænas, but were derived from an upper stratum of post-Roman age in the cave, in which they are exceedingly abundant. The goat hitherto has not been found in any pleistocene strata in this country or in France, all the repeated cases of its occurrence turning out on examination to be the result of the mixing of two suites of animal remains, the one pleistocene, and the other historic or pre-historic. This is very generally done by the workmen, and this was probably the case in the Victoria Cave. But if these equivocal data be assumed to prove that man was living in this district while hyænas occupied the cave, the evidence is still unsatisfactory as to their pre- or post-glacial age. The hyæna stratum itself appeared to him, while the explorations were under his direction, not to be of clearly defined pre- or inter-glacial age; and his doubts as to this point were, he believed, shared by Prof. Hughes. He further remarked that the reindeer found in the hyæna stratum had been omitted from Mr. Tiddeman's list of species. The rudeness of the palæolithic implements in the Cresswell caves from the lower strata as compared with the more highly finished ones found above them, seemed to him to imply a progress in the arts in that district.

A priori, the more highly finished should succeed the ruder implements, although of course many cases of their being mixed together were on record. Into the other avenues of discussion he would forbear to enter.

Prof. Hughes, in reply, said that he wished the subject had been divided, so that they might have considered separately the different parts of the evidence and the different sources of error which had still to be eliminated. For instance, he thought it would be very well if they could have an exhibition of, and discussion on, the various ways in which nature breaks, cuts, and otherwise marks bone and stone as well as of various ruder forms known to be the result of human agency, so as to get clearer ideas as to what might really be taken as evidence of design. He pointed out that the measure of the antiquity of the deposits containing the remains of man depended chiefly upon the time it would take to bring about certain geographical changes, either assuming that surrounding conditions had practically remained the same, or allowing for such differences as must have occurred, and of which we can estimate the effect. Applying this, while he agreed with Col. Lane Fox's remarks on the slow rate of waste of the Thames valley, he felt that we must make a very considerable allowance for the probability that during the period from the bronze age to our own man had interfered far more with the free course of the river than during all previous time. Nature might also entirely change the rate of waste in such a case by a gentle upheaval or depression causing the more rapid or slower cutting back of the stream. With regard to the existence of depressions in non-calcareous strata he thought we could detect two ways in which they were formed. One by the forcing out of the plastic material all round the mass of gravel or clay thrown on it, and another when the gravel worked down into the puddled surface of a clay, the softer portions of which oozed up between the sinking stones. In all the cases which had come under his observation in which such phenomena occurred above palæolithic beds, the last appeared to be the explanation, as also in most cases where it was the only evidence for the more southerly extension of glacial phenomena.

With regard to the Victoria Cave, he thought that the evidence was as yet decidedly against the pre-glacial age of any of the deposits containing even a suspicion of man. He believed that the deposits along the sides and in the side chambers of a limestone cave were frequently newer than those in the main cave, as the carbonated water, being thrown off the clay, must work the sides down. Whatever might be the age of the boulder clay on the floor at the mouth of the cave, he believed that the thin layer which occurred in the talus had fallen out of a pipe of which there were plenty in the limestone above, and that this clayey bank had ponded back the flood-waters and caused the accumulation of mud in the talus inside and the formation of the laminated clay.

Mr. Tiddeman[1] had thought it unnecessary on this occasion to call attention to geological minutiæ at the cave mouth, but as Prof. Hughes had raised the question of the age of the boulder clay there he was bound to follow him. Prof. Hughes said the boulder clay fell from the cliff at a time long subsequent to the date when the bones were deposited, but in drawing his section he had omitted a very important feature. They had to dig through twenty feet of talus before they came to the boulder clay, at the back of which was the hyæna bed; that represented a very considerable lapse of time since the boulders were deposited there. If the boulder clay fell at a subsequent period how was it that it was at the base of all the talus and not mixed up with it. If it fell before the talus began to form it might practically be considered of glacial age. As regards the reindeer in the lower bed, only one very doubtful specimen had been found since he had had charge of the excavations. The chief matter to be considered was whether this fauna which had been found in Europe and in England with human handiwork, occurred at a time which could be correlated with certain great physical events. All the facts which he had noticed seemed to harmonise with the idea that there had been in England two well-marked glacial periods, and these both prior to the much lesser event of the upper boulder clay of Lancashire. For instance, the boulders made into implements which Prof. Hughes had referred to in Pontnewydd Cave, need not have belonged to the latest glaciation of that country. Mr. Tiddeman would not dispute the fact of a fibula making its way down into the earth, especially after Prof. Rolleston's experience, but in the Victoria Cave it would have great difficulty. If it were soft mud it might have a chance of getting down, but if it were modern other modern things would go down with it unless it had a start. [Prof. Rolleston said it was pointed at both ends.] Mr. Tiddeman did not think there was a possibility of its working its way down. There were large blocks of stone and beds of stalagmite which had to be blasted in getting down to it. He hoped geologists would bear in mind as new facts cropped up, the suggestion that we had had two glacial periods.

THE GREENWICH OBSERVATORY REPORT

THE Report of the Astronomer-Royal at the annual visitation on Saturday contained nothing extraordinary with respect to the ordinary work of the Observatory. With reference to extraneous work, there are one or two points worthy of notice.

First, as regards the operations for the transit of Venus, the Astronomer-Royal reports as follows:—

The computing staff under Capt. Tupman has by degrees been reduced to two junior computers within the Observatory; and one or two computers external to the Observatory, who are employed on large groups of systematic calculations, for which they are remunerated by tariff. The principal part of the calculations remaining at the last report was that applying to the determination of the geographical longitudes of fundamental stations. At the moment of my writing, the last of these (the longitude of Observatory Bay, Kerguelen) is not absolutely finished; but I trust that it will be so before my presentation of this report; and then I shall be in position to offer the first determination of correction to parallax from eye-observations of the transit.

The method of determining the geographical longitude of the principal station in each group by vertical transits of the moon has been found very successful at Honolulu and Rodriguez. For stations in high south latitude, horizontal transits are preferable; for Kerguelen, as I have mentioned, the work is not quite completed. (It will be remembered that the longitude of Mokattam, the principal Egyptian station, was determined by telegraph.) The corrections to the moon's tabular places have been determined with much care from meridional observations at the principal European observatories.

The differences of longitude, or the relations of clock-times, within the groups of stations, are ascertained.

These calculations must be followed by the preparation of the factors of errors of various elements. Little progress is made in these; the work will not be heavy.

No further advance is made in the photographic reductions. The work is large, but it is simple, and will not be oppressive.

Second, as regards the numerical lunar theory:—

In the algebraical theory an alteration has been made, by the substitution of the equation of radial forces for the equation of *vis viva*. Nearly all the numbers had been computed, and the additional numerical operation was small.

The numerical calculations of the factors of symbolical variations are advancing; and the computations of the perturbing side of the equations, with due attention to the terms requiring extension of decimals, are in progress.

The numerical errors to which I alluded in the last report are corrected; and I do not think that any systematic error now remains.

With the view of preserving, against the ordinary chances of destruction or abandonment, a work which is already one of considerable magnitude, I have prepared and have printed as Appendix to the Greenwich Observations (with additional copies as for a separate work) the ordinary equations of lunar disturbance, the novel theory of symbolical variations, and the numerical developments of the quantities on the first side of the equations. The last of these will ultimately require some additions for the terms whose magnitude is increased (in algebraical development).

The work is perhaps somewhat larger than I anticipated, and the regularity of its progress has been disturbed by very frequent interruptions of my own attention, occasioned chiefly by annoying occupations on the transit of Venus. I trust that it will in future go on in a more orderly and more rapid way.

Sir George Airy concludes his report with the following general remarks:—

The subject which, I think, must first present itself to the mind of anyone who has traced the history of the observatory is the increase in the number and the fulness of our occupations.

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[1] In the abstract of Mr. Tiddeman's paper, p. 70, line 40, *non-gravels* should be *river-gravels*; line 44, *then* post-glacial should be *there* post-glacial.

Of these one in particular (altazimuth-observations of the moon) has originated with myself; others, from the suggestions of the Board of Visitors, or from the obvious demands of the scientific world.

This increase is felt even in our buildings and grounds; every corner of every room is or will shortly be occupied; and the form of the ground almost forbids extension.

The printing of the steps of the reductions of observations (which originated with myself more than forty years ago) naturally increased the labour within the observatory, as well as the expenses without it. This printing, however, must never be abandoned. But there is another part, of which the policy still appears to me somewhat doubtful, namely, the printing *in extenso* of every figure of original observations, it being remarked that the originals or extracts are always open to astronomers. I brought the question of suppressing these before the Board of Visitors many years ago; but the opinions of astronomers (I cite in particular the honoured name of M. Biot and that of Mr. Johnson) were so strongly adverse to it, that I laid aside all further thoughts of it; and I do not even now profess to entertain a decided opinion.

The three points, however, to which I have alluded (the extent of scientific occupations, the enlargement of buildings, and the amount of printing) must before long engage the attention of the Visitors.

RECENT RESEARCHES AMONG THE LOWER SARCODE ORGANISMS

THE customary annual address on the occasion of the anniversary of the Linnean Society was, on Wednesday, the 24th May, delivered by the President, Prof. Allman, F.R.S. In continuation of his last year's summary of the progress in this department of biology, he dwelt upon the important additions to our knowledge of these organisms, due to the investigations of Archer in our own country and of Hertwig and Lesser, Franz Eilhard Schulze, and Greeff in Germany.

The discovery of many new monothalamic Rhizopods of fresh water and the important additions made by the British and German investigators to our knowledge of their protoplasmic bodies were brought in review before the meeting. These monothalamic forms may be divided in accordance with the nature of their pseudopodia; in some these processes being short, thick, and finger-shaped (*Lobosa*); in others long, slim, and filiform (*Filifera*). The former were illustrated by *Hyalosphenia*, with its smooth, transparent shell, and by *Quadrula*, with beautifully sculptured shell; and the latter by *Gromia*, with its very long filiform reticulated pseudopodia; and by *Microgromia socialis*, which has the curious habit of forming colonies by the association of numerous individuals, which become united to one another by the mutual fusion of their pseudopodia. The remarkable form of reproduction discovered by Hertwig in *Microgromia* was also described. Hertwig had shown that in this Rhizopod the protoplasm divides by spontaneous fission into two segments, one of which remains in the shell, while the other forces its way out, assumes an oval shape, develops, instead of pseudopodia, two vibratile flagella, and becomes a free-swimming flagellate Zoospore, capable of ultimate development into the form of the adult. The very interesting discovery by Haeckel, that the contents of the so-called "yellow cells" of the Radiolaria become of a deep violet colour under the action of iodine, and are therefore mainly composed of starch, was also referred to among recent additions to our knowledge of the lower organisms. An account was then given of the remarkable and very significant researches of Messrs. Dallinger and Drysdale among the so-called "Monads,"—microscopic organisms which become developed in putrifying solutions of organic matter, and which, in their ordinary and apparently adult state swim about by the aid of vibratile flagella. These laborious and trustworthy investigators have shown that the flagellate monads may acquire an amœboid condition and move about by the aid of pseudopodia; that two such amœboid forms when they come in contact with one another become instantly blended together at the point of contact, that this blending becomes more and more intimate until the two individuals become completely fused together, when their mingled protoplasm assumes the form of a spherical sac filled with particles of immeasurable minuteness. These particles are germs destined for the reproduction of the individual. Their form can be demonstrated only by the highest powers of the microscope; and by following them by means of a one-fiftieth of an inch object glass, Messrs. Dallinger and Drysdale were enabled to trace their gradual development into the form of the adult. They further proved the remarkable and unexpected fact that these minute germs may be subjected to a temperature of from 258° F. to 300° F. without losing their vitality and power of development, a fact of vast significance in its bearing on experiments connected with the question of spontaneous generation. Finally attention was drawn to the quite recent discovery of Hertwig and F. E. Schulze of a nucleus in the Foraminifera. By this discovery the true systematic position can now be assigned to the Foraminifera, which must accordingly be removed from the region of Cytodes or non-nucleated protoplasm masses (to which they had been hitherto relegated), and placed on a much higher stage in the great division of the Rhizopoda. Resting on these facts F. E. Schulze has attempted to represent by the aid of a genealogical tree the mutual affinities and derivation from one another of the various members of the Rhizopoda. The base of the tree where its stem is as yet undivided, consists of the primitive forms—mere non-nucleated Cytodes represented by Haeckel's Monera (*Protogenes*, *Protamœba*, &c.). From these by the differentiation of a nucleus in their protoplasm are evolved the nucleated forms (*Amœba*, fresh-water Monothalamia, Foraminifera, Heliozoa, &c.) which constitute the sub-divisions into which the stem branches off. These repeat the various modifications of pseudopodia (Lobose, Filiform, &c.) which had already existed in the primitive forms, and which they thus derive by inheritance from their non-nucleated progenitors. Finally through the branch of the Heliozoa we are conducted to the ultimate twigs formed by the families of the Radiolaria, in which we find not only nuclei but a "central capsule" indicating the highest grade of differentiation attained by any member of the group.

THE NORWEGIAN DEEP-SEA EXPEDITION

THE Norwegian Deep-Sea Expedition is about to start from Bergen on its second summer cruise in the steamer *Bæringen*. It has just been decided by the proper authorities that the expedition, like last year's, shall be commanded by Capt. Wille with Lieut. Petersen as first officer. The scientific staff of the expedition is also the same as the previous year with the exception of the chemist, whose post is this year filled by Herr H. Tornœ.

The following is the approved plan of the expedition of the present year.

The equipment of the vessel and the determination of its magnetic constants were to be completed by June 1. In studying the temperature in the deep sea over the banks off the West Coast, it has become evident that accurate observations are wanting in the Norwegian Rende. In order to obtain these the vessel will go from Bergen direct to sea, and following the bottom of the Rende, take accurate observations there. Farther to the north several of last year's observations may also be verified.

The first proper field of work is the Norwegian coast banks to the north of Ramdalen. From existing observations it is probable that the "Havbro," where the bank sinks toward the depths of the Polar Sea, and where the ice-cold water begins at the bottom, lies at least twenty-five geographical miles from the coast. Between Rœst and the point off Ramdalen, where the expedition last year found a depth of about sixty fathoms with a rocky bottom ten miles from land, it is considered probable that there runs a more or less continuous ridge of rock.

The position and characteristics of the "Havbro" and the supposed ridge form main points in the examination of the banks. This goes on by forming cross-sections perpendicular to the coast. The sections, like last year's, are to be at a distance of twelve or thirteen geographical miles asunder. Their inner boundary is to be the outermost line of the special hydrographical survey. Their outer boundary is where the temperature at the bottom of the sea is 1° C., or thereby. In each section besides the observation of the temperature at each sounding, at least three other series of observations are required, one at the inner boundary, one at the "Havbro," at its inner edge, and one at the outer limit of the section. The number of soundings will depend on the bottom being found more or less even as the work goes on.

In order to leave as much time as possible for work in the depths of the Polar Sea, and at Jan Mayen and the Greenland ice, there will be carried on, along with the survey of the banks, the examination of the Umbellularia region to a depth of 1,000 fathoms in every third cross-section. If circumstances permit several other bank-sections may also be extended to this depth. While working in this region it will be proper to call at Bodoe.

A series of observations of temperature made in West Fiord with the newest deep-water thermometers will be carried out both during the voyage northwards in June and during the return voyage in August, in order to examine the abnormal state of things found there in the summer of 1875, the minimum temperature being at a depth of seventy fathoms. As the zoological surveys in West Fiord have hitherto only embraced the fauna of the coast, the opportunity will be sought to be utilised for zoological work in this fiord at a greater distance from land.

Magnetic absolute observations will be carried out at Rœst, where circumstances, from Lieut. Petersen's observations in 1875, appear favourable, and where the most projecting point is found for comparison with the proposed observations on the Greenland ice.

In the course of June it is supposed, with average weather, and with the experience gained last year, that the survey of the banks and of the Umbellularia Region according to this plan will have reached the latitude of Tromsœ.

The expedition will in the end of June or beginning of July, be equipped for a cruise to the westward, going first along the line Andœ, Jan Mayen, surveying it and its neighbourhood on both sides. Round the north-east end of Jan Mayen as a centre, several short series of soundings are taken towards the north-east, the north, and the north-west. For in that neighbourhood where the outermost mountain of the Iceland volcanic region is supposed to be, the bottom appears to sink very rapidly towards the deep sea in the directions named.

Provided Jan Mayen can be reached, it is proposed to land on it for the purpose of undertaking an examination of its geography, geology, hydrography, zoology, botany, &c. It is also intended to make a survey of the sea on the west and south sides of Jan Mayen, for there is ground for supposing that Jan Mayen is connected with Iceland by a sub-oceanic ridge.

In order to examine the phenomena at the boundary between the warm surface current from the Atlantic and the cold polar current in the Greenland sea, the Greenland ice north-west of Jan Mayen will be visited. When the examination of a part of the Greenland ice is completed the course will be taken to a point about midway between Iceland and Jan Mayen, and the cross-section is to be examined from this point in the direction of Ranen with the view of discovering and exploring the supposed sub-oceanic ridge. When this cross-section is worked out to the Norwegian Umbellularia region, previously examined, the expedition will sail to Tromsœ to carry on work to the northward, if time permit.

As July is supposed to be devoted to the Jan Mayen cruise, the first half of August will be employed in a survey of the banks, the "Havbro," and part of the Polar Sea between Norway and South Spitzbergen. The eastern limit will be the line North Cape, Bear Island, South Cape. The latter half of August will be devoted to the return voyage to Bergen and dismantling. The scientific work will be carried out mainly in the same way as in 1876. In the zoological work, along with the use of the dredge, the trawl, and the swab, special weight is laid on the use of nets in the intermediate depths and fishing on the banks. On Jan Mayen the capture of birds ought to be an object of importance, and on the Greenland ice possibly seal and bear-hunting, &c.

In taking soundings the form and extent of the banks is to be determined, and the way in which the bottom falls off from these to the greatest depths of the polar sea. It is of fundamental importance for understanding the orography and geology of this sea and the neighbouring land and for the distribution of animal life, whether the bottom falls off towards the deep sea with an even slope or in terraces and escarpments with plateaus lying between. The position and number of the soundings is therefore to be determined with this view.

The measurements of the temperature in the deep sea are to be carried out to the extent necessary for a certain determination of the isotherms in the deep water of the cross-sections. It is also to be kept in view that the points in the cross-sections may be used for longitudinal sections. The newest deep-sea thermometers are to be used as frequently as possible, along with those employed last year.

At every sounding a specimen of water is to be taken from the bottom, and at chosen places at intermediate depths. The specific gravity of all specimens is to be ascertained. At every sounding, also, specimens of the bottom are to be taken and preserved for future examination, which is also to be done with the material of the bottom brought up by the dredge and trawl.

The chemical work is to be carried on mainly as last year. The specific gravity of the surface-water is to be determined once or twice a day, and oftener at places where the state of the currents or other circumstances render it desirable. Observations of the currents in the sea are to be carried out where circumstances make it desirable, and the weather permits.

Magnetic observations are to be carried out on board at sea, special weight being laid on obtaining deviation observations. Absolute determinations are to be carried out at Rœst, and on the Greenland ice, besides the determination of the ship's magnetic constants at Husœ, and elsewhere where possible.

A geological survey is to be made of Jan Mayen, if a landing there is possible, to the extent circumstances may permit. It would be of great interest to carry out astronomical determinations of geographical position, topographical and hydrographical surveys on Jan Mayen. A determination of geographical position at Rœst, in connection with the magnetic observations, is also desirable, as the place is not connected with the trigonometrical net.

Botanical observations and collections are to be carrried out on Jan Mayen to the greatest possible extent.

The meteorological observations on board are to be carried out in all essential points as in 1876. H. MOHN.

NOTES

AMONG those on whom honours were conferred on the occasion of Her Majesty's birthday, was Dr. J. D. Hooker, President of the Royal Society and Director of the Royal Gardens, Kew. Dr. Hooker has been made a K.C.S.I.

THE twenty-sixth annual meeting of the American Association for the Advancement of Science commences at Nashville on August 29. Our own association meets at Plymouth this year on August 15, under the presidency of Prof. Allen Thomson, of Glasgow.

WE have received the programme of an Anthropological Exhibition to be held in connection with the Paris International Exhibition of 1878, under the superintendence of the Paris Anthropological Society. That Society has nominated a large Commission to organise the exhibition, with Prof. de Quatrefages as president. M. Krantz has placed at the disposal of the Society a spacious and fine position in the central pavilion of the Trocadero Palace. The Commission makes a warm appeal to all interested in the progress of the anthropological sciences, both in France and abroad. It wishes to prepare a complete inventory of the present state of these sciences. The following are the classes under which the exhibition will be arranged:—1. Crania and bones, mummies, and specimens relating to the comparative anatomy of the human races; 2. Instruments, methods of education; 3. Prehistoric and ethnological collections; 4. Photographs, paintings and drawings, sculpture, and modelling; 5. Geographical maps and tables relating to ethnology, prehistoric archæology, linguistic, demography, medical geography, &c.; 6. Books, journals, brochures. In order to facilitate the work of collection and arrangement, the Commission has appointed the following members to superintend the special departments named:—Dr. Broca, 1, rue des Saints-Pères, for what concerns Anthropological Societies; Dr. de Ranse, 4, place St. Michel, Anthropological Instruction; Dr. Topinard, 97, rue Rennes, General Anthropology and Craniology; M. Gabriel de Mortillet, Château de St. Germain-en-Laye (Seine-et-Oise), Prehistoric Archæology and Anthropology; M. Girard de Rialle, 64, rue de Clichy, Ethnography of Europe; M. Abel Hovelacque, 35, rue de l'Université, Linguistic Anthropology; Dr. Dureau, 16, rue de la Tour-d'Auvergne, Bibliography; Dr. Bertillon, 20, rue Monsieur-le-Prince, Demography, or the Statistical Study of Population, and Medical Geography; M. Louis Leguay, 3, rue de la Sainte-Chapelle, for all concerning the general management and arrangements. Each of the gentlemen named should be communicated with in relation to all concerning his special department. The programme is a very inviting one, and if successfully carried out the result must be highly interesting and valuable. We hope British anthropologists will do all in their power to help the Commission to carry out their aims.

THE Maritime and Piscatorial Exhibition, which was opened at the Aquarium on Monday, contains a large collection of stuffed river fish—the largest collection, probably, ever brought together. Fourteen angling clubs, and many private individuals, have sent contributions. The Prince of Wales has also lent his collection of Indian fish obtained during his tour, and there is also a collection from the Indian Museum. As the exhibition includes, among other things, all subjects connected with fishing, there is a good show of fishing-tackle, and with them a fine set of flies. Those by Mr. Ogden Smith are quite works of art. Messrs. Sotheran and Co. have, at considerable trouble, made a collection of books on fish and pisciculture, which deserves attention.

A HUNGARIAN prelate, the Archbishop Louis of Haynald, has constructed an astronomical observatory at his own expense at Kalocsa, lat. 46° 31′, long. 16° 32′. Among the instruments are a Browning telescope, a small (4-inch) Merz refractor, and a Cooke transit instrument. The arrangement of the new observatory is superintended by M. de Konkoly, already known as having built on his own property, O-Gyalla, a well-furnished observatory. We may add that the Archbishop of Haynald has already devoted considerable sums to botanical researches.

THE *conversazione* of the Society of Arts is fixed to take place at the South Kensington Museum, on Wednesday, June 27.

THE Commission appointed by the U.S. Government to examine Capt. Howgate's proposal for the establishment of an exploring colony within 400 miles of the North Pole recommend that 50,000 dollars be granted for the purpose.

AT the meeting of the Zoological Society of France on June 1, M. Perier, Professor of Conchology at the Paris Museum, explained that the number of specimens was far too large to be properly exhibited in the galleries, and that consequently it had been resolved to exhibit only specimens of each genus, and to have the types of species arranged systematically in drawers. Each species is to be entered in an alphabetical and systematic catalogue, so carefully compiled that the information it contains may be got at instantly. Any visitor wanting to inspect a particular species will have only to make an application to the galleries for conchology. The work is immense, but it is supposed that in ten years it will be completed.

THE French Society of "Amis des Sciences," instituted twelve years ago for distributing pensions to the families of deceased men of science or to *savants* themselves when incapacitated by old age, has held its anniversary meeting under the presidency of Prof. Berthelot. The report was read by M. Pasteur. It shows that the assets of the Society amount to 19,000*l*. The sum spent in pensions was 1,200*l*. last year. An *éloge* of M. Charles Sainte Claire Deville was read by M. Fouquier, his successor as professor at the Collége de France.

WE have received the yearly report for 1876–7 of the new Scientific Club of Vienna, and its perusal is well calculated to fill a cultured Londoner of moderate means with envy. For an entry money of five florins and a yearly contribution (payable quarterly!) of sixteen florins all the advantages of a good London club can be obtained combined with those of the Royal Institution. The club possesses a spacious building with lecture-halls, reading and conversation-rooms for smokers and non-smokers, writing-rooms, refreshment-rooms, splendid library, all the best journals of every kind from all parts of the world. During the greater part of the year there are scientific and other lectures, entertainments and receptions, excursions during summer, and it is contemplated to publish a weekly journal connected with the affairs of the club. The members, numbering already upwards of 500, belong to all classes of society and to all professions, their only bond of union being a desire for cultured intercourse. The only institution in London at all approaching to this Vienna club is the modest German Athenæum.

UNDER the care of Dr. P. P. C. Hoek there has been published in German a *catalogue raisonné* of zoological works and papers that have appeared in the Netherlands during 1875–6. There are in all eighty-seven titles. The title is "Die Zoologie in den Niederlanden," and the publishers are E. J. Brill, of Leiden, and C. F. Winter, Leipzig.

INTELLIGENCE received at New York, June 1, from the Sandwich Islands announces that simultaneously with the earthquake at Iquique, Peru, a tidal wave struck the group of islands on May 10, between 4 and 5 A.M. The sea suddenly receded and returned with great violence in a wave sixteen feet high, which entered the harbour at Hilo, and swept away the wharves and storehouses in the front part of the town. All the houses at Waiakin within 100 yards of the shore were destroyed. Five persons were drowned; many were picked up in the harbour. The earthquake undulations continued during the day, the difference between the highest and lowest water-mark varying from three feet to thirty-six feet in various parts of the islands. Cocoanut (*sic*, but probably Molokai) island was entirely submerged, and the hospital at that place was swept away. A fresh eruption of the Kilauea volcano commenced simultaneously with this oceanic disturbance. The same earthquake wave was also felt all along the Mexican Pacific Coast. Late particulars announce that great devastation and loss of life were caused by the tidal wave which swept the Peruvian coast. Six hundred persons are reported to have perished.

IN connection with the above the following is of interest:—A Press despatch from Washington, of May 12, states that Assistant-George Davidson telegraphs to C. P. Patterson, Superintendent of the Coast Survey, in relation to the earthquake waves registered in the tide-gauge at Fort Point, at the entrance of San Francisco Harbour, to the following effect:—"Sharp earthquake waves commenced on Thursday, May 10, at 6.18 a.m., five rises and falls of 9 in. in 80 minutes; then nine *maxima* or crests 48 minutes apart, with secondary *maxima*, the largest of which were 15 in.; then six sharp rises of 14 in. each to irregular broken crests one hour apart; then to 5 A.M. Friday, double *maxima* as at the commencement, the largest rise being 18 in. From 5.20 A.M. Friday, to 1.15 P.M., irregular *maxima*, then a sudden fall of 16 in., and action, continuing until 5 P.M. No well-defined *maximum* of action, but exhibitions of markedly different character. It may be stated that the earthquake wave of the great earthquake in Japan some years ago was twenty-three minutes in traversing the Pacific to San Francisco."

ON Monday afternoon, about 3.30, a tornado partially destroyed Mount Carmel, a flourishing town of 3,000 inhabitants on the Wabash River, South-Eastern Illinois. It struck the town on the southern quarter and passed north, destroying almost everything in its path. Three churches, two newspaper offices, two schools, the Court-house, and 120 buildings were demolished. The ruins caught fire, burning almost till midnight before the flames were extinguished. Many persons were killed or injured.

ON the vote in Parliament last Thursday to complete the sum of 24,569*l*. for the Patent Office, Dr. Playfair remarked that while the Government derived a revenue of 180,000*l*. from the inventors of this country, it made them a very insignificant return. Only about 2,000*l.* was spent upon a Patent Museum. The Patent Museum at South Kensington was very inferior to the corresponding institutions in France and America, and if we had such a museum at all it ought to be a good one, and such as would promote invention. He wished to ask whether the Government had bought or rented a large building for the purposes of an efficient Patent Museum? Mr. W. H. Smith explained that the space intended for the Patent Museum at South Kensington had been taken up by the collection of scientific apparatus, but that the Government still intended to arrange for a suitable exhibition of inventions.

A NOTE has been circulated in German papers, warning parents against the use of india-rubber toys manufactured in France and sold largely on the north of the Rhine. They were said to be poisonous owing to a certain quantity of oxide of zinc which was mixed with vulcanised india-rubber. The French Minister of Agriculture and Commerce ordered an inquiry to be made by the Council for Public Health, and the result is a declaration that these objects are quite harmless.

ACCORDING to recent news received by M. Sidoroff as to the mammoth discovered in Siberia, the carcass was found in the gold-bearing sands of a gold-washing on the river Kundola, at a depth of five mètres. The flesh was very soft and of a light red colour when freshly dug out, but soon hardened, becoming like a white clay. It seems to be much impregnated with lime. The digging out of the whole body will be very difficult because of the access of water. M. Polyakoff has already left St. Petersburg for the excavation of the body.

A TASHKEND telegram, dated May 10, announces that the Russian embassy, sent last spring to Kurl, has made a thorough survey of the route it followed, together with numerous barometrical measurements of heights. Capt. Kuropatkin and the naturalist M. Wilken, have explored the lake Bastan-nor and the route to Karashar, twenty-seven miles east of Kurl, whilst Lieut. Senorguroff has explored and surveyed the route from Aksu to Karakol, following the Babel pass.

AT the last meeting, May 16, of the Russian Geographical Society, it was agreed to postpone the exploration of the Angara river until next year, whilst the exploration of the water-parting between the Obi and the Yenissei will be made during this summer. Besides, M. Sonirnof undertakes a tour in Russia for magnetical measurements.

WE learn from the *Gardeners' Chronicle* that Prof. Boulger, the Professor of Natural History at the Royal Agricultural College, Cirencester, is preparing a "Flora" of Gloucestershire, and will be glad of any information as to rare species, localities, &c.

A COMMITTEE has been formed having for its object the presentation of a testimonial to Mr. F. W. Wilson, whose connection with the Crystal Palace, after twenty-five years' service, has just terminated. During the past quarter of a century Mr. Wilson, by his genial disposition, obliging manners, and thoroughly zealous efforts to promote and successfully carry out the various interesting natural history and other exhibitions held at the Crystal Palace, has earned the respect both of his colleagues and the general public.

OUR Paris correspondent writes: We were present at the Alliance Electric Works to witness an experiment on the new caolin-light, which we had seen in operation at the Physical Society, and which was recently described in NATURE. The Ruhmkorf machine was fed by a more powerful primary current given out by an Alliance electro-motor of three-horse power. The bar of caolin, ignited by induction-spark, was eight centimetres long, and fully equal to eight gas-burners. At the same time three so-called electric candles were in operation, each of them giving about forty gas-burners, and fully equal to five caolin lights. Consequently not less than twenty-five lights could be fed at once in separate parts of the same building or at any distance. The light is admirable for its constancy and duration; the quantity of caolin destroyed is quite insignificant—no more than 1 mm. per hour; the thickness of the caolin plate or bar used is not more than 4 millimetres. The electric candles are formed, as is known, by two graphite bars separated by a caolin bar. It was proved that the agitation of the candlestick produces no interruption of the current. Consequently a single Alliance-work machine can feed the three lights required by the regulations for steamers. The graphite pencils are consumed at the rate of eight centimetres per hour, which is a difficulty; but it is expected that any length required may be supplied by means of a proper clock-work. Experiments were tried on the same day at the Palais de l'Industrie for the purpose of illuminating the exhibition of pictures with electric light. The ordinary Gramme machines were set into operation. The light was found steady, but the effect was not quite favourable artistically. It is expected that with the system of light division, an improvement may be effected. The experiments were made in order to test whether electric light may be utilised on the occasion of the forthcoming International Exhibition.

IN connection with the above we may state that the experiment with Jablochkoff's electric light at the West India Docks on Tuesday night, was not quite successful, owing to some part of the apparatus going wrong. The experiment is, however, we believe, to be repeated.

THE destruction of agricultural and garden produce by the systematic attacks of insects is yearly becoming more and more serious. The gravity of this subject has been felt of late years not only by the growers of food crops themselves, but by scientific men, especially entomologists, and it is therefore with satisfaction we notice that some steps have been taken to obtain accurate records of the habits and conditions most favourable to the development and increase of certain well-known insects that devastate our crops. A little pamphlet called "Notes for Observations of Injurious Insects" has recently been issued by Mr. T. P. Newman, of 32 Botolph Lane, E.C. In these notes the necessity of such observations is pointed out, not only on scientific grounds, but also with a view to diminish the yearly losses of food crops to the country. The "Notes" are illustrated with woodcuts of many well-known insect depredators as a guide to their identity, and a ruled sheet is furnished for entering the records and any remarks that may appear desirable. These, as well as the "Notes" themselves, are to be obtained *free* on application to the printer, as above, and any information required will be furnished by the Rev. T. A. Preston, the Green, Marlborough, or by Mr. E. A. Fitch, Maldon, Essex.

MR. OSCAR BROWING, in lecturing on Friday evening last at the Royal Institution on the history of education, drew attention to the science of teaching being in advance of the art. He urged that, like medicine, pædogogics should be made a science of observation on results obtained. Teachers should interchange experiences on results as well as on theory.

THE additions to the Zoological Society's Gardens during the past week include a Mesopotamian Fallow Deer (*Cervus mesopotamicus*), an African Leopard (*Felis pardus*), a Cheetah (*Felis jubata*) from South Africa, a Humboldt's Lagothrix (*Lagothrix humboldti*) from South America, deposited; a Prevost's Squirrel (*Sciurus prevosti*) from Malacca, purchased; a Black-backed Jackal (*Canis mesomeles*) from South Africa, presented by the Earl of Guildford.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE, May 30.—The Sheepshanks Astronomical Exhibition has been adjudged to John Edward Aloysius Steggall, scholar of Trinity College.

The twenty-second annual report of the Botanic Garden Syndicate has been issued. It is stated that during the past year much attention has been paid to the labelling of the arranged trees, shrubs, and herbaceous plants in the open ground, and it is believed that they all, or nearly all, are correctly and legibly named. There has not been time, without neglecting other important work, to name with similar completeness the plants scattered in the belt. It is believed that very few of the plants in the houses are without names, although a few duplicate specimens may be in that condition. About 1,700 new labels have been written. The culture of the plants is such as to give satisfaction to the syndicate. About 170 species of herbaceous plants have been raised from seed to supply the places of those which have died. Among the presents acknowledged are packets of seeds from the Indian Botanic Gardens, from Baron F. v. Mueller, and five large ferns from Australia from the last-mentioned gentleman.

OXFORD.—At the ensuing commemoration the honorary degree of D.C.L. will be conferred upon Mr. J. Evans, the distinguished antiquary; Dr. Harold Browne, the Bishop of Winchester; and Lord Coleridge. It is probable that degrees will be conferred on certain other distinguished persons, whose names, however, it would be premature to announce at present.

On November 23 next there will be an election to a Brackenbury Natural Science Scholarship at Balliol College, worth 80*l.* a year, tenable during residence for four years, open to all such candidates as shall not have exceeded eight terms from matriculation. Papers will be set in (1) Mechanical Philosophy and Physics, (2) Chemistry, (3) Physiology; but candidates will not be expected to offer in more than two of these.

DURHAM.—The University Mathematical Scholarship has been awarded to Mr. F. W. Sanderson, Hatfield Hall.

THE UNIVERSITIES BILL.—The attempt was made twice on Monday in the House of Commons to get a clause inserted in the Universities Bill abolishing Clerical Fellowships. As might have been expected, the attempt failed, though in the case of Mr. Goschen's motion by a very narrow majority—only 9.

UNIVERSITIES AND NATIONAL LIFE.—The following forcible remarks on universities and national life occur in the address of Prof. Sylvester at the Johns Hopkins University, to which we have already referred:—"The mention of Germany brings to my mind the importance of universities to the maintenance or development of a national spirit in the countries in which they are fostered and carried on with an animus free from local or sectarian prejudices. I think that there can be little doubt that the greatest fact in modern history, the consolidation of the German empire, the resurrection of the German people, is mainly to be attributed to the feeling of brotherhood and the spirit of nationality kept alive in those ganglions of thought, those centres of intellectual activity, the German universities. It is the university professors who have made German unity a possibility, and I cannot but deplore the unpatriotic short-sightedness of those in my own country who, until so late a period, have struggled, and still covertly struggle, to make our universities in England not the representatives of the universal English mind, but the monopoly of a party and the appanage of a sect."

SCIENTIFIC SERIALS

American Journal of Science and Arts, May.—On vortex rings in liquids, by J. Trowbridge.—An account of the discoveries in Vermont Geology of the Rev. Augustus Wing, by J. D. Dana.—Notes on the history of *Helianthus tuberosus*, the so-called Jerusalem artichoke, by J. H. Trumbull and Asa Gray.—A new investigation of one of the laws of friction, by A. S. Kimball.—Examination of American columbic acid minerals, by J. Lawrence Smith.—On the sensitiveness to light of various salts of silver, by M. Carey Lea.

Poggendorff's Annalen der Physik und Chemie, No. 3.—On the cohesion of salt solutions, by G. Quincke.—On the theory of stationary electric flow in curved surfaces, by A. Topler.—On normal magnetisation, by M. Petruscheffsky.—On the temperature in the conducting wire of a galvanic current, by M. Streintz.—Remarks on a statement of F. Kohlrausch on thermo-electricity, by M. Clausius.—On the galvanic resistance of haloid compounds, by M. Lenz.—On the dynamical significance of the quantities occurring in the mechanical theory of heat, by M. Szily.—On a paradox of the mechanical theory of heat, by M. Ritter.—Researches on the movements of radiating and irradiated bodies (concluded), by M. Zöllner.—On the connection between absorption and dispersion, by M. Ketteler.—On the neutral combs of the Holtz machine, by M. Riess.—Galvanic dipping battery for elements with two liquids, by M. Hertz.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. x. fasc. iv. v. vi.—Observations on Borrelly's comet, by M. Schiaparelli.—Ditto by P. Secchi.—On a singular congenital and lipomatous pigmentary alteration, by M. Scarenzio.—Contribution to the study of Addison's disease, by M. Valsuani.—New barometric formula for the measurement of altitudes, and the reduction of barometric heights to the sea-level, by M. Grassi.—General method of obtaining diagrams of the motion of a point, by M. Padelletti.—On algebraic differential equations of the first order and first degree, by M. Pincherle.—On some questions of electrostatics, by M. Beltrami.—On some unpublished letters from Lagrange to Euler, by M. Schiaparelli.—Origin and anatomy of intestinal diverticula, and their application in practical surgery, by M. Sangalli.—On a new species of Dochmius (*Dochmius balsami*), by MM. Parona and Grassi.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 31.—"On the Amplitude of Sound-Waves," by Lord Rayleigh, M.A., F.R.S.

Scarcely any attempts have been made, so far as I am aware, to measure the actual amplitude of sound-bearing waves, and indeed the problem is one of considerable difficulty. Even if the measurement could be effected, the result would have reference only to the waves actually experimented upon, and would be of no great value in the absence of some means of defining the intensity of the corresponding sound. It is bad policy, however, to despise quantitative estimates because they are rough, and in the present case it is for many reasons desirable to have a general idea of the magnitudes of the quantities with which we have to deal. Now it is evident that a superior limit to the amplitude of waves giving an audible sound may be arrived at from a knowledge of the energy which must be expended in a given time in order to generate them, and of the extent of surface over which the waves so generated are spread at the time of hearing. An estimate founded on these data will necessarily be too high, both because sound-waves must suffer some dissipation in their progress, and also because a part, and in some cases a large part, of the energy expended never takes the form of sound-waves at all.

The source of sound in my experiment was a whistle, mounted on a Wolf's bottle, in connection with which was a syphon manometer, for the purpose of measuring the pressure of wind. This apparatus was blown from the lungs through an india-rubber tube, and with a little practice there was no difficulty in maintaining a sufficiently constant blast of the requisite duration. The most suitable pressure was determined by preliminary trials, and was measured by a column of water $9\frac{1}{2}$ centimetres high.

The first point to be determined was the distance from the source to which the sound remained clearly audible. The experiment was tried in the middle of a fine still winter's day, and it was ascertained that the whistle was heard without effort at a distance of 820 metres. In order to guard against any effect of wind, the precaution was taken of repeating the observation with the direction of propagation reversed, but without any difference being observable.

The only remaining datum necessary for the calculation is the quantity of air which passes through the whistle in a given time. This was determined by a laboratory experiment. The india-rubber tube was put into connection with the interior of a rather large bell-glass open at the bottom, and this was pressed gradually down into a large vessel of water in such a manner that the manometer indicated a steady pressure of $9\frac{1}{2}$ centimetres. The capacity of the bell-glass was 5,200 cubic centimetres, and it was found that the supply of air was sufficient to last $26\frac{1}{2}$ seconds of time. The consumption of air was therefore 196 cubic centimetres per second.

In working out the result it will be most convenient to use consistently the C. G. S. system. On this system of measurement the pressure employed was $9\frac{1}{2} \times 981$ degrees per square centimetre, and therefore the work expended per second in generating the waves was $196 \times 9\frac{1}{2} \times 981$ ergs. Now the mechanical value of a series of progressive waves is the same as the kinetic energy of the whole mass of air concerned, supposed to be moving with the maximum velocity of vibration (v); so that, if S denotes the area of the wave-front considered, a be the velocity of sound, and ρ be the density of air, the mechanical value of the waves passing in a unit of time is expressed by $\frac{1}{2}S \, . \, a \, . \, \rho \, . \, v^2$, in which the numerical value of a is about 34,100, and that of ρ about ·0013. In the present application S is the area of the surface of a hemisphere, whose radius is 82,000 centimetres; and thus, if the whole energy of the escaping air were converted into sound, and there were no dissipation on the way, the value of v at the distance of 82,000 centimetres would be given by the equation—

$$v^2 = \frac{2 \times 196 \times 9\frac{1}{2} \times 981}{2\pi(82000)^2 \times 34100 \times {}^{\cdot}0013},$$

whence

$$v = {}^{\cdot}0014 \text{ centimetres per second.}$$

This result does not require a knowledge of the pitch of the sound. If the period be τ, the relation between the maximum excursion x and the maximum velocity v is

$$x = \frac{v\tau}{2\pi}.$$

In the present case the note of the whistle was f^{iv}, with a frequency of about 2730. Hence

$$x = \frac{{}^{\cdot}0014}{2\pi \times 2730} = 10^{-8} \times 8{\cdot}1,$$

or the amplitude of the aerial particles was less than a ten millionth of a centimetre.

I am inclined to think that on a still night a sound of this pitch, whose amplitude is only a hundred millionth of a centimetre, would still be audible.

Linnean Society, May 24.—Annual General Meeting.—Prof. Allman, F.R.S., president, in the chair.—The Senior Secretary (Mr. Currey) read his report, among other items, mentioning that twelve fellows and five foreign members had died during the past year. On the other hand forty-three fellows, three foreign members, and one associate had been elected. Of active scientific workers that had passed away, J. Scott Bowerbank, Edward Newman, and Alfred Smee called for special mention; the labours of the first named, in a previously little-worked department, the sponges, marking an epoch in British natural history. A passing tribute was due to the memory of the foreign members that had died during the year, for von Baer, Braun, De Notaris, Ehrenberg, and Hofmeister in their several departments worthily represented biological science in its broad aspects.—Mr. Gwyn Jeffreys, treasurer, in his financial statement, showed an increased balance in favour of the Society, and this, notwithstanding extra outlay in valuable additions to the library, improvements in the Society's scientific publications, &c. The demise of the late Charles Lambert, F.L.S., had brought the handsome bequest of 500*l*. to the funds of the Society.—The president and officers were re-elected and the following gentlemen, viz., Lieut.-Col. Grant, C.B., W. Carruthers, R. Hudson, Dr. J. Millar, and Dr. R. C. A. Prior were elected into council in lieu of the subjoined, who retired by rotation:—G. Bentham, Gen. Scott, C.B., R. B. Sharpe, H. T. Stainton, and C. Stewart. We elsewhere give an abstract of the presidential address, devoted to a *résumé* on "Recent Researches among the Lower Sarcode Organisms," a subject of daily increasing interest.

Meteorological Society, May 16.—Mr. H. S. Eaton, M.A., president, in the chair.—Messrs. Stephen Bretton, J. Gulson Burgess, David Milne Home, and F. Gartside Tippinge were elected fellows of the Society.—The following papers were read:—An improved form of mercurial barometer, by Mr. R. E. Power, F.M.S. The improvement consists in the use of a double column of mercury, so that in the event of a vacuum being formed by the escape of some mercury into the cistern, the shock is no longer felt by the tube but in the first place is received by the mercury alone and then reflected much diminished into the cistern, where it is modified by the presence of the atmosphere. At the same time, owing to the peculiar construction of the cistern, the probability of any mercury leaving the tubes is much less than in the case of the standard barometers at present in use. It is also believed that the employment of the double tube will do away with the necessity of boiling the mercury.—The relation between the upper and under currents of the atmosphere around areas of barometric depression, by the Rev. W. Clement Ley, F.M.S. This paper gives a description of the mean directions of the movements of cirrus clouds over the different segments of areas of depression. The subject is treated on its observational side, as it is not yet considered ripe for much theoretical discussion, but one or two points seem likely to throw some light on the theory of the movements of the atmosphere. The direction of the upper currents round a depression is found to be most intimately related to the direction in which the depression itself is progressing. In the rear of a depression where the mean direction of the surface winds is nearly parallel to the isobars, or at right angles to the radius, the cirrus current almost coincides with the surface wind, except near the central calm. In the front of the depression on the other hand, where there is the greatest indraught near the earth's surface, the upper currents flow greatly away from the centre. The current in the rear of a depression has therefore the greatest, and that in the front the least vertical depth. The majority of our depressions travel towards some point between N. and E., and so far as the author has been able to calculate, the mean height of our south-easterly winds is not half that of our north-westerly winds, even supposing the latter to extend no higher than the stratum of cirrus. The angle of deviation increases with the increase of friction; and it is possible, therefore, that the great incurvation of the surface winds in the front of a depression is closely related to the greater amount of friction which they encounter, for they are comparatively shallow currents, and experience resistance above as well as at their base. The contrast between the upper currents on the right and those on the left of the trajectory is quite as remarkable as that between the upper currents in the front and in the rear, and it is very constant and well-marked in its general character. As regards the centre, the upper-current, when traceable over this district, commonly coincides, or very nearly, with the wind previously felt at the earth's surface.—Contributions to the meteorology of the Pacific—the Island of Rapa, by Robert H. Scott, F.R.S. Rapa is a small island, eighteen miles in circumference, in the South Pacific, in latitude 27° S. and longitude 144° W. The observations were made by Capt. D. E. Mackellar, on board the depôt ship *Medas*, during the period extending from 1867, December 15, to 1869, May 27. The climate appears to be an equable one.

Physical Society, May 26.—Prof. G. C. Foster, president, in the chair.—The following were elected members of the Society:—Lieut.-Col. A. C. Campbell, Dr. H. Debris, F.R.S., Mr. W. T. Thiselton Dyer, M.A., B.Sc., W. Jack, M.A., and Capt. Sale, R.E.—Lieut.-Colonel Campbell explained and exhibited a double slit which he has employed for measuring the distances between the lines in the spectrum and finds of great service in cases where the illumination is so slight as to preclude the possibility of using the ordinary micrometer. One slit remaining stationary the other can be moved at right angles to its direction by means of a very delicate micrometer screw of 200 threads to the inch, the graduated head of which is capable of distinctly indicating one-five-millionth of an inch in the motion of the slit. If now a reading of the micrometer be taken when the slits are superposed and form one continuous slit, and a second reading when any given line has been superposed upon any other line at a moderate distance from it, the difference between these readings will enable us at once to ascertain the distance between the lines if the micrometer be calibrated in terms of the spectrum as seen in the observing telescope. The author has made several measurements with this apparatus, and finds it to be capable of extreme accuracy, but it is of course essential that the movable slit remains within a moderate distance of the axis of the collimator. He then described a simple arrangement for automatically fixing a prism, when placed on the table of a goniometer at the angle of minimum deviation when different coloured rays are under examination. To the arms which support the telescopes of the goniometer are attached two short links of equal lengths connected at their extremities with a nut sliding freely on an arm which is fixed radially to the centre table of the instrument. The prism is held on this table with its base at right angles to this arm, and it thus remains adjusted for all the rays of the spectrum.—Mr. O. J. Lodge then read two papers by Profs. Ayrton and Perry, jointly, of the Imperial College of Engineering, Japan. The first contains an account of an elaborate series of experiments on ice as an electrolyte. They state as a result of their experiments that the capacity per cub. cent. of ice at – 13°·5 C. is 0·002 micro-farad, and the specific inductive capacity is 22,160 (that of air being called unity), while that of water at 8°·7 C. is about 2,240 times this amount. Commencing with ice at – 13°·6 C the temperature was allowed to rise and the conductivity determined by galvanometer readings. From these a very regular curve was deduced which shows that the conductivity increases regularly, and that there is no sudden rise in passing from the solid to the liquid state. The apparatus was also employed for determining the electromotive force of polarisation currents at different temperatures by replacing the copper by a zinc disc.—The second communication contained suggestions for experiments on the viscosity of water and other liquids. It is accompanied by working drawings of an apparatus which the authors have designed for determining the relation between the viscosity of a liquid and the velocity of a surface moving in contact with it. They have, however, no facilities for making such an apparatus, and therefore place it at the service of any one who may be willing to study the subject.

Victoria Institute, June 4.—At the Annual Meeting of this Institute, the address was delivered by Mr. J. E. Howard, F.R.S.—Capt. F. Petrie (the honorary secretary) read the eleventh annual report; 107 members and associates had joined during the year, and the total number had risen to over 700, two-thirds of whom were country and foreign members.

Institution of Civil Engineers, May 29.—Mr. George Robert Stephenson, president, in the chair.—A paper was read on an economical method of manufacturing charcoal for gunpowder, by Mr. George Haycraft, F.C.S.

PARIS

Academy of Sciences, May 28, M. Peligot in the chair.—Some remarks were made on M. Roudaire's Algerian scheme. M. de Lesseps thought it practicable and useful; M. d'Abbadie desired that study might be given for a whole year to the quantity of evaporation and the *régime* of the winds in that region, &c.—The following papers were read:—Reply to M. Tacchini's note inserted in last *Comptes Rendus*, by M. Janssen. While accepting M. Tacchini's figures in comparison of maximum and minimum years, he yet holds that the numerous and rapid appearance and disappearance of spots witnessed during the past year indicates very violent movements of matter.—On Gay Lussac's law of volumes, reply to M. Saint Claire Deville, by M. Wurtz. The system of chemical equivalents which prevailed about 1840 over the atomic notation of Berzelius, has not taken account of Gay Lussac's discoveries on the combinations of gases, and the maintenance of the principle of equivalence in chemical notation would bring science back to the times of Dalton, Wollaston, and Richter, which would be an anachronism.—Reply to M. Wurtz's note on the law of Avogadro and the atomic theory, by M. Berthelot.—Experimental critique on the glycogenetic function of the liver, by M. Cl. Bernard. He proves the function directly during life, laying bare the liver in a dog, cutting off a piece of it, which is then put into boiling water. The tissue contains a proportion of sugar varying from 1 to 3 per 1,000. The influence of vivisection does not cause variation in the quantity, unless the circulatory and respiratory functions are greatly disturbed. M. Bernard also demonstrates that the saccharine matter continues to be formed in the liver after death.—Observations on the work presented to the Academy by M. Villarceau, entitled the "Nouvelle Navigation," by M. Mouchez. He objects to the title; and the analytic method proposed (application of Taylor's series in place of the old formula) though good in theory, is found impracticable. The graphic method is nearly the same, according to M. Mouchez, as he has himself long practised and recommended. Lastly there is but rare need to use M. Villarceau's new process for determining *the most probable point*.—On an algebraic method for obtaining the *ensemble* of the fundamental invariants and covariants of a binary form, and of any combination of binary forms (continued), by Prof. Sylvester.—Description of new manœuvres executed with the economising apparatus at the dam of Aubois, by M. de Caligny.—Experiments made in order to appreciate the diffusion of the vapours of sulphide of carbon introduced into the ground as an insecticide, by M. Gastine. In permeable soil the diffusion reached a maximum radius of about 1 metre about the hole of injection (which received 20 grammes of the sulphide). The vapours persisted at 30 cm. distance from March 1 to 5, or about 100 hours; nearer the hole, 150 hours. In clayey soil the diffusion was as extensive, and the persistence was about twenty-four hours more.—Historical remarks on the theory of movement of one or several bodies of constant or variable forms, in an incompressible fluid; on the apparent resultant forces and on the experiments relating thereto, by M. Bjerknes.—On Gauss's formula of quadrature, by M. Callandreau.—Thermal researches on the substituted anilines, by M. Longuinine.—Electrolysis of ordinary pyrotartaric acid, by MM. Reboul and Bourgoin. This acid is very stable; it is electrolysed like mineral acids, and is in this respect quite unlike succinic acid, which is decomposed easily.—Researches on the synthesis of acids of the series $C_nH_{2n-2}O_2$ and $C_nH_{2n-4}O_2$; allylic and diallylacetic acids, by M. Reboul.—On the decomposition of carbonic acid in the solar spectrum by the green parts of plants, by M. Timiriazeff. A spectrum was formed with a bisulphide of carbon prism and a trough containing chlorophyll solution was put in the path of the rays. Next were interposed in a row five vessels inverted over mercury, each containing air with about 5 per cent. carbonic acid, and these vessels received green organs of plants (pieces cut from a bamboo leaf). The vessels, being in different parts of the spectrum, were left there six to ten hours on fine days in July, and the gas was afterwards analysed. The maximum of decomposition of CO_2 was always found in the vessel corresponding to the position of the characteristic absorption band of chlorophyll; in orange, yellow, and green, the amount of decomposition showed successive decrease, and in red there was even production of CO_2 through respiration.—On the nature and signification of the small red corpuscles of the blood, by M. Hayem. He concludes that they are young corpuscles incompletely developed.—On the changes of volume, and the delivery of the heart, by M. François-Franck. His method was to connect the cavity of the pericardium (in live dogs) with one of Marey's registering apparatuses. The heart increasing in volume in diastole drove a certain quantity of air into the tambour, while the contraction in systole permitted return of this air.—On the histological alterations of the uterus in their relations to the principal diseases of this organ, by M. Courty.—Statistical Researches on the Sologne, especially with regard to recruiting and movement of the population, by M. Coste.—On a process for recognising the presence of fuchsine in wines, by M. Baudrimont. A drop of fuchsinated wine left a few seconds on the skin of the hand, produces a mark which cannot be washed out with water.

GÖTTINGEN

Royal Academy of Sciences, January 31.—Report on the Botanical Institute of Göttingen University for 1876.

February 21.—A contribution to the theory of reflection-phenomena, by M. Rethy.

March 21. Separation of arsenic from nickel and cobalt.

April 4.—On the electric conductivity of aqueous solutions, especially of salts of the alkalies and alkaline earths, caustic alkalies, and some acids, by M. Kohlrausch.

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THURSDAY, JUNE 14, 1877

THE ENDOWMENT OF RESEARCH

WE are authorised to publish the accompanying list of the sums to be paid by the Government, on the recommendation of the Royal Society, during the present year in aid of Scientific Research.

We might well leave the list to speak for itself, but it would be ungrateful not to point out that the Duke of Richmond and Lord Sandon have by their action, beyond all doubt, inaugurated a new era in the scientific activity of our country, and one which is sure to be fostered by corporate bodies and individuals now that the Government has set so noteworthy an example.

PERSONAL PAYMENTS.

Mr. J. A. Broun.—For Correction of the Errors in the published Observations of the Colonial Magnetic Observatories . . . . 150*l.*

Dr. Joule.—For Experimental Investigations into the Mechanical Equivalent of Heat . . . 200*l.*

Prof. Parker.—For Assistance in Researches on the Morphology of the Vertebrate Skeleton and the Relations of the Nervous to the Skeletal Structure, chiefly in the Head . . . . 300*l.*

Rev. W. H. Dallinger.—For Microscopic Investigations of Monads, Bacteria, and other Low Forms of Life 100*l.*

Rev. F. J. Blake.—For compiling and publishing a "Synopsis of the British Fossil Cephalopoda" . 100*l.*

Prof. A. H. Garrod.—For Aid in preparing for Publication an Exhaustive Treatise on the Anatomy of Birds . . . . 100*l.*

Dr. Murie.—For completing and publishing three Memoirs:—"Anatomy of the Kingfisher," 4to., with five plates; on "Extinct Sirenia," 4to., with six plates; "Osteology of the Birds of Paradise," folio, three plates 150*l.*

Mr. H. Woodward.—For Continuation of Work on the Fossil Crustacea, especially with reference to the Trilobita and other Extinct Forms, and their Publication in the Volumes of the Palæontographical Society. . 100*l.*

Prof. Schorlemmer.—For Continuation of Researches into (1) the Normal Paraffins, (2) Suberone, (3) Aurin . 200*l.*

Dr. H. E. Armstrong.—For Continuation of Researches into the Phenol Series, and into the Effect of Nitric Acid on Metals . . . . . . 300*l.*

Profs. King and Rowney.—For Researches to Determine the Structural, Chemical, and Mineralogical Characters of a Certain Group of Crystalline Rocks represented by Ophite . . . . 60*l.*

Mr. W. J. Harrison.—Towards the Expense of collecting and describing Specimens of the Rocks of Charnwood Forest . . . . 50*l.*

NON-PERSONAL PAYMENTS.

In aid of Apparatus, Materials, and Assistance.

Dr. J. Kerr.—For aid in Electro-Optic and Magneto-Optic Researches . . . . . . 200*l.*

Mr. J. E. H. Gordon.—For Experimental Measurements of the Specific Inductive Capacity of Dielectrics 50*l.*

Prof. Guthrie.—For Apparatus and Assistance in (1) the Determination of the Latent Heats of the Cryohydrates and the Vapour Tensions of Colloids; and (2) the Examination of Heat Spectra and Radiant Heat by means of varying Electrical Resistance in Thin Wires . 150*l.*

Mr. J. T. Bottomley.—To aid in carrying out a Series of Experiments for determining the Conductivity for Heat of Various Liquids and Solutions of Salts . . 100*l.*

Sir William Thomson.—For Assistance and Materials for a Continuation of Experiments on the Effects of Stress in Magnetism . . . . . . 100*l.*

Mr. W. Crookes.—For Assistance in continuing his Researches connected with "Repulsion resulting from Radiation" . . . . 300*l.*

Messrs. Rücker and Thorpe.—For a Comparison of the Air and Mercurial Thermometers . . . 50*l.*

Mr. F. D. Brown.—For an Investigation of the Physical Properties, the Specific Gravity, Expansion by Heat, and Vapour Tension, of the Homologous and Isomeric Liquids of the C_nH_{2n+1} Series . . . 100*l.*

Prof. Roscoe.—For Continuation and Extension of the Experiments on the Self-registering Method of measuring the Chemical Action of Light . . . 100*l.*

Sir William Thomson.—For Investigation and Analysis of Tidal Observations and Periodic Changes of Sea Level . . . . . . 200*l.*

Dr. J. B. Balfour.—For the Expense of Illustrations for a "Monograph of the Pandanaceæ" . . 50*l.*

Mr. H. T. Stainton.—For Aid in publishing the "Zoological Record" . . . . . 100*l.*

Dr. J. G. M'Kendrick.—For Apparatus for a Research into the Respiration of Fishes . . . 75*l.*

Prof. Gamgee.—For a more Complete Survey than has yet been made of the Physiological Action of the Chemical Elements and their more Simple Compounds, with the Object, in the first instance, of establishing a Physiological Classification of the Elementary Bodies . 50*l.*

Dr. Brunton.—For Researches into the Physiological Action of the most important Compounds of Nitrogen, and into the Action of certain Poisons, and for Apparatus . . . . 80*l.*

Mr. E. A. Schäfer.—To pay the Wages of an Assistant to give Mechanical Aid in Histological and Embryological Research . . . . . . 50*l.*

Dr. Burdon Sanderson.—For an Investigation of the Normal Relation between the Activity of the Heat-producing Processes, and the Temperature of the Body 70*l.*

Prof. Schorlemmer.—For continuation of Researches into (1) the Normal Paraffins, (2) Suberone, (3) Aurin . 100*l.*

Mr. W. N. Hartley.—For Researches into the Photographic Spectra of Organic Substances, into the Phosphates of Cerium, the Conditions under which Liquid Carbonic Acid is found in Rocks and Minerals, the Double Salts of Cobalt and Nickel, and for other Investigations, and for Assistance . . . . 100*l.*

Dr. Burghardt.—For a Research into the Origin of the Ores of Copper and (if possible) of Lead, their Mode of Formation, and the Chemical connection (if any) between the Ore and its Matrix. . . . . 50*l.*

Prof. Church.—For a Research into the colouring matters of Colein, of Red Beet, and for the Study of Plant Chemistry . . . . . . 50*l.*

THE "CHALLENGER" COLLECTIONS

THE preliminary steps have been taken for the completion of the great work of the *Challenger*, and the vast collections made during the voyage are now being distributed among experienced workers for determination and description.

The director of the scientific staff has been at great pains in endeavouring to secure the services of men most competent for the task, and we are sorry to see that some of our English naturalists, and notably the president of the Geological Society, have thought it necessary to remonstrate against the course which the director has taken in the selection of the men to whom he is about to entrust the examination of the collections. We have already had occasion to refer to what we felt obliged to characterise as an unwarranted attack on Sir Wyville Thomson, and it is with much regret that we observe an attitude of hostility to the mode of distribution which has been deemed most conducive to the reputation of the expedition and to the interests of science.

It would seem that while almost all the great zoological groups which the *Challenger's* dredges have brought to light have been handed over for examination to naturalists in this country, a few have been placed in the hands of American and German workers; and it is this association of foreign zoologists with the men to whom in this country by far the largest portion of the work has been assigned that has excited the indignation of the individuals referred to.

Now every one who has kept himself *en rapport* with recent zoological research, must know that the foreign zoologists, to whom Sir C. Wyville Thomson has intrusted these collections, stand before all others in the amount and thoroughness of their work in the special departments of zoology for which their aid is asked, and the narrowest nationalism cannot deny that it was the duty of the director to see that the specimens were placed in the hands of men most competent to secure for science the results which have been obtained at the cost of so much labour, skill, and public expenditure.

If this country can be shown to enjoy the unique distinction of possessing in every department of zoological research men at least as good as can be met with elsewhere, the advocates of a national science may find an argument in favour of having the work absolutely confined to Englishmen; but if we cannot assume a position which no other nation in the world would think of claiming, it is plainly for the interests of science that we should supplement from abroad those departments of research in which foreign workers may excel us.

That the naturalists to whom we have referred will not receive much support from their fellow-workers will be evident from the subjoined letter to the Editors of the *Annals* now in process of signature, which has already received the adhesion of the presidents and secretaries of the Royal, Linnean, and Zoological Societies, and of other leading men in this department of knowledge:—

"*Zoology of the 'Challenger' Expedition.*

"As in a letter upon this subject in the number of the *Annals of Natural History* for May last Dr. P. Martin Duncan, writing as president of the Geological Society, has stated that he speaks 'at the instance of a very considerable number of members of learned societies,' we, the undersigned, wish to state that we do not agree in the strictures passed by Dr. Duncan upon the manner in which Sir C. Wyville Thomson has distributed the specimens collected by the *Challenger* Expedition for description. So far as we have had an opportunity of judging we are perfectly satisfied that Sir C. Wyville Thomson, in the arrangements which he has made as regards these collections, has acted consistently with the best interest of science.

"It was, in our opinion, Sir C. Wyville Thomson's duty to secure the aid of the most competent naturalists without regard to their nationality; and, even if it were proper that national jealousies should be imported into science, Sir C. Wyville Thomson can hardly be reproached on this score, when it is considered that two-thirds at least of the naturalists whose aid he has obtained are Englishmen.

"J. D. HOOKER.
T. H. HUXLEY.
CHARLES DARWIN.
ST. GEORGE MIVART.
FRANCIS DAY.
GEO. BUSK.
WILLIAM B. CARPENTER.
W. H. FLOWER.
P. L. SCLATER.
OSBERT SALVIN.
A. H. GARROD.
GEO. A. ALLMAN.
TWEEDDALE."

It is of importance that no misunderstanding should exist as to the real state of the controversy which has arisen on a subject in which zoological science is so deeply interested, and we believe we cannot do better than lay before our readers the correspondence which had taken place between Sir Wyville Thomson and Dr. P. Martin Duncan before a word of hostile criticism had as yet shown itself in print.

"Scientific Club, Savile Row, London, W.
"24th March, 1877

"MY DEAR SIR WYVILLE THOMSON,

"You can hardly imagine the strong feeling of disappointment which has arisen amongst a very large section of the naturalists and palæontologists who study the invertebrates, in consequence of a letter which was published in the *Ann. and Mag. of Nat. Hist.* for March, 1877. In this letter the scientific world is informed by our mutual friend, A. Agassiz, that the Echini, Ophiurans, Radiolaria, and a part of the Spongida collected in the expedition of the *Challenger* have been given to American and German naturalists for description, and that the United States have a 'fair share' of the work. So great is the feeling that English workers should have been thus passed over, that a conference has been held on the subject, and I have been asked to write to you in the friendliest spirit of remonstrance. I need hardly state that I should not have taken this liberty did I not happen to hold a position which entails action in everything relating to the progress of geological science. Writing then on the part of many men whose capabilities as naturalists and palæontologists I am well aware of, I express their and my own opinion that in this distribution your amiability and want of personal acquaintance with English workers have led you astray. We recognise the great merits of those foreign gentlemen to whom you have sent collections and the exceeding liberality of A. Agassiz; but we do not think that you are justified in giving them the results of the greatest natural history expedition which has ever sailed from this country, unless there is a want of that power amongst English workers which will enable them to treat the subjects in the broadest sense, and to compare the recent and geological faunas satisfactorily. There is no such deficiency. I am asked to urge upon you a reconsideration of the matter, and to leave a fair portion of work in the hands of our friends, giving the rest to men of your own country. Assuring you that we appreciate your difficulties, and that we will assist you in every way consonant with the dignity of English science, I remain,

"Yours sincerely,
(Signed) "P. MARTIN DUNCAN

"Sir C. WYVILLE THOMSON"

"MY DEAR DR. MARTIN DUNCAN,

"I must ask you to consider this note as written to yourself personally, for I cannot, of course, in any way recognise this nameless 'Conference.' I may mention, however, at starting, that in this matter I have consulted several of the first English naturalists, and that they entirely approve of my selection.

"I take up my pen rather hopelessly, for your letter does not touch any of the considerations on which I have acted. My duty was to have prepared an official account of the voyage to the best of my power within a certain time. I endeavoured to select to assist me in this (1) those who had most successfully made certain branches their special study and were generally regarded as *authorities*; and (2) those whom I knew by experience to be likely to do the work within the time to which I was tied down, and to return the specimens in good order to be lodged in the British Museum. In all cases where I considered that these conditions were fairly fulfilled by Englishmen I at once and fully recognised the great advantage of avoiding the risk of sending things abroad, but except for this consideration I confess I saw and see no objection, but rather the reverse, to making a great work of this kind somewhat more catholic. The result has, however, been, that by far the greater part of the work will be done in England. I do not mean to go into special cases, but I give a general sketch of the arrangements as they now stand:—

Sea Mammals	Prof. Turner.
Birds	Dr. Sclater.
Fishes	Dr. Günther.
Cephalopoda	Prof. Huxley.
Gastropoda ... ... Lamellibranchiata	Rev. R. B. Watson.

Brachiopoda	Mr. Davidson.
Higher Crustacea	Probably Prof. Claus.
Ostracoda	Prof. G. Brady.
Copepoda	
Isopoda	Mr. Henry Woodward.
Cirripedia	Mr. Darwin.
Annelida	Dr. McIntosh.
Gephyrea	Prof. Ray Lankester.
Bryozoa	Mr. Busk.
Echinoidea	Mr. A. Agassiz.
Ophiuridea	Mr. Lyman.
Crinoidea	Dr. Carpenter and myself.
Hydromedusæ	Prof. Allman.
Corals	Mr. Moseley.
Sponges	Prof. Oscar Schmidt and myself
Rhizopods	Mr. Henry Brady.
Radiolarians	Prof. E. Haeckel.

"Now the only foreigners in this list are Dr. Günther, Prof. Claus, Prof. Agassiz, Mr. Lyman, Prof. Oscar Schmidt, and Prof. Haeckel. If there is a better English authority than Dr. Günther on fishes, I beg his pardon for having overlooked him. The crustaceans were to have been done by the late Dr. v. Willemöes-Suhm and certain considerations come in as to the use of his plates and notes, which I need not discuss. I am not aware that there is any one in this country who can be considered at present an authority on recent Echinoidea. The choice perhaps lay between Agassiz and Lovén, but the reference collection at Cambridge is the best in the world in this department. There is, so far as I know, no English authority on Ophiurids at present. I prefer Oscar Schmidt's mode of treating the sponges to that of any other author. I am not aware that any Englishman knows the Radiolarians so well as Haeckel. There are a good many departments not yet settled, and one or two other foreigners may be added to the list. I should of course have most heartily asked your assistance with the corals had Moseley not undertaken them, but he has the preference as one of our staff, and he has done excellent work.

"I have submitted the principles on which I am working to the best of my ability to the Treasury, and they have received their sanction and that of the Council of the Royal Society. I cannot recognise the importance of the geographical distribution of naturalists, and with all respect for the dignity of British science I must say I think that in this selection, which I considered entirely open, I have done it ample justice.

"Believe me, very truly yours,
"C. WYVILLE THOMSON

"20, Palmerston Place, Edinburgh, March 27"

To this letter no reply has been received, and the subject might well have ended here.

The objectors to the course pursued by Sir Wyville Thomson would hardly advocate our assumption of a spirit more narrow and illiberal than that of any other country, and they will perhaps be interested in knowing how a foreign Government has acted under quite similar circumstances.

The results of the two great recent scientific expeditions fitted out in the United States, that of the "Haslar," and the Exploration of the Gulf Stream, have been distributed among special workers without any regard to nationality. Of this we need no further evidence than that afforded by the arrangements which have been adopted for the examination of the very rich collections made during the Gulf Stream Expedition. These collections have been allocated as follows:—

Halcyonaria	A. Kölliker	Würzburg.
Annelides	E. Ehlers	Göttingen.
Sponges (part)	O. Schmidt	Strassburg.
Sponges (part)	E. Haeckel	Jena.
Holothurians	E. Selenka	Leiden.
Polyzoa	F. A. Smitt	Stockholm.
Mollusca	J. Gwyn Jeffreys	London.
Hydroids	G. J. Allman	London.
Starfishes	E. Perrier	Paris.
Crustacea	Alph. Milne Edwards	Paris.
Fishes	F. Steindachner	Vienna.
Cephalopods	J. P. Steenstrup	Copenhagen.
Brachiopods	W. H. Dall	Washington.
Corals	L. F. Pourtales	Cambridge, U.S.
Ophiurans	T. Lyman	Cambridge, U.S
Echini	A. Agassiz	Cambridge, U.S

It will be thus seen that out of the twenty-two zoologists among whom the collections of the *Challenger* have been distributed *seventeen are English*; while out of the sixteen to whom the American collections have been assigned, *four are American*.

ELEMENTARY PHYSICS

Matter and Motion. By J. Clerk-Maxwell. (Society for Promoting Christian Knowledge. London, 1876.)

THE recent appearance of a swarm of elementary books on physics, some of which at least are written by well-known authors, leads to some very curious inquiries and speculations: for, though treating in the main of the same parts of the same subject as does the work we are specially dealing with, and addressed professedly to the same class of readers, they have comparatively little in common with it. To a certain, even a considerable, extent, this difference is of course due to the idiosyncrasies of the authors; but, after all allowance is made for these, there is still a most notable divergence. It will be both interesting and profitable carefully to consider in what this divergence consists, and what is its probable origin. For it is not too much to say that an intelligent reader of Clerk-Maxwell's book, had he no other source of information, would be utterly unable to answer any one of hundreds of questions which might be framed (without "dodge" or "trap") by a qualified examiner, *directly* from the text of the others. It is true that such questions would be artificial rather than natural—bearing more upon old and cumbrous dogmatic fallacies than upon the actual facts of science. But if the reader of Clerk-Maxwell's book would be at a loss when examined from any of the others, the student who relies merely upon one (or even *all*) of these would hardly even understand the meaning of a question put directly from Clerk-Maxwell's. The main origin of this divergence is to be found in the steady progress of knowledge in all departments of true science; even the most elementary. And, bearing this in mind, we may give an almost complete statement of the case by saying that Clerk-Maxwell's book properly belongs to the second half of the present century, while his rivals give us that of the first half only. These give us again the elementary "*Mechanics*" of our student days (more than a quarter of a century ago) very little changed—though where changed, often changed for the better—the first gives us what is emphatically the science of *to-day*. Possibly enough, in the beginning of the twentieth century even Clerk-Maxwell's book may appear a little antiquated; but it is hardly to be imagined that the text-book of that not very distant future will differ from Clerk-Maxwell's to anything like the extent to which that differs from its competitors. At least if there be anything like so a great difference it will depend upon some wholly new information as to the intimate nature of matter or energy, certainly not upon a mere difference in the mode of treatment.

The immense steps taken by Galileo and Newton (to mention only two of the chief workers) in the simplification and orderly arrangement of the fundamental conceptions and laws of physical science are too often lost sight of in comparison with the vast extensions which these men gave to our knowledge—though their actual *value* is probably little inferior. No doubt it is scarcely possible for any one (except by accident, of which some notable instances will occur to every reader) to make real extensions of our knowledge unless he possesses a clear conception of elementary principles. But great discoverers are generally too much engrossed with their higher studies to bestow much time on the explanation or co-ordination of the more elementary parts of their science. All the more honour, then, to those who, like Galileo and Newton, have made every step of their progress clearly intelligible to the student from its very foundations.

Immediately after Newton's time the progress of physical science was almost arrested in Britain (mainly, it seems, from the want of men of a high order of genius), and the really great foreign Mathematicians and Physicists of the time were entirely absorbed in the rapid development of their subjects. The disastrous consequence was that the elementary parts of science were left almost entirely to the second-rate men, or to the mere sciolists, men whose representations of science, even at the best, were mere caricatures—in the sense in which an orrery mimics the solar system, or an automaton a living animal;—and though, since that time, really first-rate men (*e.g.*, Cavendish and Young) have occasionally appeared in Britain, the pernicious influence of generations of smatterers was not easily shaken off. Thus an absurdly artificial, and unnecessarily complex system, based to a certain extent on Newton, but altogether devoid of his wonderful precision, simplicity, and completeness, came to be generally adopted here. This artificial system may be said to have reached its climax in the works of the late Dr. Whewell, perhaps the only brilliant writer of what is known now as *Paper Science*, by far the most pestilent weed against which the true scientific cultivator has to contend. The omniscient Master of Trinity might quite probably have been able to hold his own with Aristotle, had he lived in days when science had but a scanty development; but it is impossible for any one nowadays to hold relatively to human knowledge any such position. And he who tries to do so, even had he the genius of Newton to start with, will simply *do nothing*.

Clerk-Maxwell (wisely, we think) appears to prefer Newton and Rowan Hamilton to Whewell, on whom or the like of whom his rivals mainly rely. And this alone accounts for a great deal of the extraordinary dissimilarity between the works to which we have alluded. What is Whewell, the universal genius, with all his book-learning, in comparison with Newton, the special genius, with his close and patient study of material phenomena themselves? Nothing. Men consulted Whewell as they would a dictionary or an encyclopædia, simply to save themselves trouble. But when was an encyclopædia ever seen to add a volume of new matter to itself as an appendix? To use an old comparison, Newton, as it were, studied Chinese metaphysics in China itself, at head-quarters; Whewell and those who do like him read all the European works on China and all the European works on metaphysics, and "combine their information." Thus almost all Clerk-Maxwell's rivals—whom therefore we need not specially name—give us the sacred *Three Classes of Levers*, the various *Systems of Pulleys*, the altogether imaginary *gold* shell of the Florentine Academicians, the *Principle of Repulsion*, the *Transmissibility of Fluid Pressure*, and what not. Weight and specific gravity are usually put forward in preference to mass and density—the accidental property before the inherent or essential one! We have the old confusing statements about a co-efficient of *elasticity* in the impact of balls. In one of the most pretentious of these works we are told that the "strict" definition of a *level surface* implies that "at all points of it the force of gravity has the same value, and its direction is at right angles to the surface." That is to say, the author here uses the word "force" in two different senses. It means the "potential energy of a given mass," when its *value* is spoken of; but it means the "weight of a given mass" when its *direction* is spoken of. For it is inconceivable that the author could have meant to state that the weight of a body is the same at all points of a level surface. We could give without practically any limit instances of a similar kind (not mere slips of the pen, from which no man's work can be free), but we will be merciful, and simply extract the following passages, putting a word or two in italics, and leaving the reader to exercise himself in finding what is erroneous:—

> "An arrow shot upwards from a bow reaches to a certain height: show that if the weight of the arrow be doubled, *other circumstances remaining the same*, the height reached will be *one-fourth* of its former value."
>
> "Gravity and distance together represent the *force* employed from the beginning in putting things where they are, and whenever they come together by attraction they develop a corresponding *force or heat*."
>
> "When heat is continually applied to water it is found that if the water is in an open vessel its *heat cannot be raised* beyond a certain point."
>
> "The *forces* of heat, electricity, magnetism and light are now considered to be all *species of motion*, discoverable and measurable only by the amount of *movement* they can produce or counteract."
>
> ". . . water is boiled by placing a lamp beneath the flask so that the upper part of the flask becomes full of steam, the air being expelled. The flask is now stopped with a cork, removed from the lamp, and allowed to cool. . . . By pouring cold water . . the water begins to boil again. *The experiment requires great care to prevent accidents*."
>
> "Matter in motion is FORCE."
>
> "Electrical attraction is *the name given to some cases* resembling magnetic attraction, in which electricity is the agent."
>
> "Liquids possess a *small but very perfect* elasticity, which differs in amount in different liquids."
>
> ". . . gold, which *in the case of a sovereign* falls as fast as anything which we have commonly in view, may be beaten out to a thin leaf which *almost floats on* the air . . ."
>
> "*It is sometimes stated incautiously*, that the weight of a body may always be supposed to be collected at the centre of gravity, but the present case shows that *such a statement is too wide*."

There is no justifying the existence of a new way of doing anything except by showing that it is better than the old one; but, if that can be done, the new way is justified. And, as it is not our interest to become Encyclopædias, when we get a good new way, let us adopt it, and at once drop the old. And we are not without hope that Clerk-Maxwell's book may effect the complete abolition of the older methods, which are already sadly shattered. Perpetual "distinctions without a difference," like the three classes of levers above alluded to, can only confuse and irritate the student—often making him doubt whether he really understands the gist of an explanation or no. And to give rules for calculating results without explaining how to obtain these rules, or what they imply physically, is, in the vernacular, simply CRAM :—call it euphemistically what you will. To learn how to do this is not, in any sense, to acquire knowledge.

Clerk-Maxwell's book is not very easy reading. No genuine scientific book can be. But the peculiar characteristic of it is that (while any one with ordinary abilities can read, understand, and profit by it) it is the more suggestive the more one already knows. We may boldly say that there is no one now living who would not feel his conceptions of physical science at once enlarged, and rendered more definite by the perusal of it. Short and (on the whole) simple as it is, it is one of the most suggestive works we have ever met with. The following extract needs no comment of ours: let us see how the metaphysicians will digest it :—

> "But, as there is nothing to distinguish one portion of time from another except the different events which occur in them, so there is nothing to distinguish one part of space from another except its relation to the place of material bodies. We cannot describe the time of an event except by reference to some other event, or the place of a body except by reference to some other body. All our knowledge, both of time and place, is essentially relative. When a man has acquired the habit of putting words together, without troubling himself to form the thoughts which ought to correspond to them, it is easy for him to frame an antithesis between this relative knowledge and a so-called absolute knowledge, and to point out our ignorance of the absolute position of a point as an instance of the limitation of our faculties. Any one, however, who will try to imagine the state of a mind conscious of knowing the absolute position of a point will ever after be content with our relative knowledge."

We can afford space for only one other quotation ; but, as it is very important, we quote *in extenso* :—

> ARTICLE CXIV.—CENTRIFUGAL FORCE.
>
> "This is the force which must act on the body M in order to keep it in the circle of radius v, in which it is moving with velocity V.
>
> "The direction of this force is towards the centre of the circle.
>
> "If this force is applied by means of a string fastened to the body, the string will be in a state of tension. To a person holding the other end of the string this tension will appear to be directed towards the body M, as if the body M had a tendency to move away from the centre of the circle which it is describing.
>
> "Hence this latter force is often called Centrifugal Force.
>
> "The force which really acts on the body, being directed towards the centre of the circle, is called Centripetal Force, and in some popular treatises the centripetal and centrifugal forces are described as opposing and balancing each other. But they are merely the different aspects of the same stress."

This is one of the few passages in the work to parts of which exception may fairly be taken. Of course, the *physical* statements are correct, and they are very clearly put. But it is hardly fair to a junior reader to begin by telling him that Centrifugal Force means *the force which must act* on a body in order to keep it moving in a circle, and then to say that *the force which really acts* is called Centripetal Force ! That force is required *to produce change of direction* of a body's motion, and that when this is applied by means of a string held in the hand the impression on our "muscular sense" is the same as if the body were *pulling* at the other end of the string, are facts : but they no more justify the use (however guarded) of the word "centrifugal" than the tension of the couplings, just before a train starts, proves that the carriages have a tendency to run *backwards*.

There is one very great blemish in Clerk-Maxwell's book, from which those of his rivals are comparatively free. Some of the woodcuts are simply atrocious. This must be looked to in future editions, for passages of great importance are at present rendered totally unintelligible to the beginner : and from this cause alone.

Clerk-Maxwell's work, then, is simply Nature itself, so far as we understand it. The peaks, precipices, and crevasses are all there in their native majesty and beauty. Whoso wishes to view them more closely is free to roam where he pleases. When he comes to what he may fear will prove a dangerous or impassable place, he will find the requisite steps cut, or the needful rope attached, sufficiently but not obtrusively, by the skilful hand of one who has made his own roads in all directions, and has thus established a claim to show others how to follow.

In the rival elementary works the precipice and the crevasse are not to be seen : there are, however, many pools and ditches ; for the most part shallow, but *very* dirty. You are confined to the more easily accessible portions of the region. In the better class of such books these are trimly levelled—the shrubs and trees are clipped into forms of geometrical (*i.e.*, unnatural) symmetry like a Dutch hedge. Smooth straight walks are laid down leading to old well-known "points of view,"—and, as in Trinity of former days, undergraduates are warned against walking on the grass-plats.

These "royal roads" to knowledge have ever been the main cause of the stagnation of science in a country. He would be a bold man indeed who would venture to assert that the country which, in times all but within the memory of many of us, produced such mighty master-minds as Lagrange, Fourier, Ampère, and Laplace, does not now contain many who might well have rivalled the achievements even of men like these. But they have no chance of doing so ; they are taught, not by their own struggles against natural obstacles, with occasional slight assistance at a point of unexpected difficulty, but by being started off in groups, "eyes front" and in heavy marching order, at hours and at a pace determined for all alike by an Official of the Central Government, along those straight and level (though perhaps sometimes rough) roads which have been laid down for them ! Can we wonder that, whatever their natural fitness, they don't now become mountaineers ?

I still vividly remember the horror with which I watched the the struggles of a former class-fellow of my own, whose friends had just sent him to another school that he might learn geometry a little earlier than was the custom with us. For him there was no longer any play—

all his spare time was devoted to the committing of Euclid to memory! I shuddered as I thought of what was to be my own fate in a few short months, when I too must be subjected to this fearful imposition. But the first hour or two which Dr. Gloag (a name strange, perhaps, to southern ears, but very high indeed on the roll of successful teachers—Clerk-Maxwell, indeed, was one of his pupils) devoted to geometry showed those of us who had any taste for the subject that it was one to be learned by head, not "by heart" (the idiotic phrase in common use)—and that my friend's parents had simply taken him from a good teacher and sent him to an exceedingly bad one—for it came to be discovered after some time that he had really considerable aptitude for geometry.

But if he had been in fact quite unfit for the study, otherwise than in learning to repeat Euclid by rote, what object beyond mere torture would have been attained by forcing it upon him? This leads to another remark of great importance in connection with the mass of elementary text-books.

What sort of students are those who require to be told to *take the square of the velocity, divide it by the radius, and find the proportion of this quotient to* 32 . . . :—without farther explanation or proof? What the better are they of the information? Call you this "teaching science?" Has it improved their minds? Will they be able to make any use of it in after life? I do not see how these questions and many other connected ones can be answered except by a prompt negative. One of two things. The pupil who requires to be taught in this way is either as yet too young, or is one who will never become old enough, to learn even the rudiments of science.

To our metaphor once more. Grass-plats, moss, and flower-beds for the happy sports of children—the bare rock and rough moor for the stern work of men. Your gravel-walks and Macadamised roads are excellent things in their way, but keep them to their legitimate users, the carriage and the perambulator for the invalid and the infant who can neither work nor even play.

My reasons for writing on this subject are very serious ones. I have to consider each year how best to instruct some couple of hundred students in the elements of physics, and have to be constantly on the out-look for a really good text-book of an elementary character. In the higher branches of the subject there is, happily, little difficulty, but that a really good, short, and simple treatise on the merest elements has been (at least till very lately) wholly unprovided is, I think, clear from the ridiculous discussions about *Centrifugal Force*, and other connected ideas, which are even now constantly to be found in our more practical periodicals.

P. G. TAIT

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Nectar-Secreting Glands

MR. FRANCIS DARWIN has made an interesting addition to his important discovery of nectar-bearing glands on the young fronds of *Pteris aquilina*, supplied from the ever-welcome experience of Mr. Fritz Müller. The latter gentleman finds that in Brazil the *Pteris aquilina* is protected from the leaf-cutting ants by those attracted to the nectar, and Mr. Darwin adds some speculations on the origin of the glands and their continued functional activity in Europe where they now appear to be useless. On this part of the question I should like to make the following remarks:—

Prof. Heer has shown that in the Miocene plant-beds at Œningen and Radoboj, ants are the most numerous amongst the fossil insects, and in 1849 as many as sixty-six species had been described from these two localities. In 1865 the number found at Œningen alone is recorded as forty-four. I do not know what the total number of species is that have been recorded from the two places up to the present time, but it probably does not fall short of eighty. Amongst the fossil ants from Radoboj there are species of the Tropical American genera *Atta* and *Ponera*. One of the fossil species of *Atta* resembles in general form and in the venation of the wings the curious *Atta cephalotes* of Tropical America.

As there are only about forty species of ants existing now in the whole of Europe it is evident that in the Miocene epoch they must have played a much more important part in Europe than they do now. Plants may then have been exposed to the attacks of enemies that have become extinct along with the general impoverishment of the fauna and flora of Europe that took place in Post-pliocene times; and the protection afforded by ants attracted to the nectar-bearing glands at the critical stage of the unfolding of the young and tender leaves may have been as important to some plants in Europe, then, as it is to many in Tropical America now.

With regard to the persistency of the nectar-producing glands up to the present time in Europe, it is to be remarked that many plants are identical with those living in the Miocene period and the world-wide distribution of *Pteris aquilina* seems to indicate that it is of very ancient origin. If a plant has not otherwise varied there is no reason apparent why it should do so in this respect so long as the secretion of nectar is not positively injurious to it. I have recently noticed in my garden that the ants that attend the glands at the bases of the leaves of the cherry, the plum, the peach, and the apricot, stroke with their antennæ some of the glands that are not excreting when they arrive at them, just as they do the bodies of the aphides. I have not actually noticed that this promotes a flow of nectar, but ever since I became a disciple of Darwin I have been convinced that the most trivial circumstance is worthy of notice; and it may be that the slight irritation of the glands kept up by the ants is sufficient to ensure the perpetuation of a function of the plant now useless to itself. It is, however, perhaps too soon to assume that the glands are entirely useless to the plants in Europe. Darwin states that there is good evidence that the absence of glands in the leaves of peaches, nectarines, and apricots leads to mildew ("Animals and Plants under Domestication," vol. ii. p. 231).

Darwin refers at the same place to the variation of the glands of the leaves in the above-mentioned fruit trees and I may add that they are extremely variable on the cherry, being sometimes absent, sometimes on the stalk and sometimes on the blade of the leaf. The young leaf in its earliest stage, before it expands, has a complete fringe of them, thus bearing out Mr. Francis Darwin's theory that they are homologous with the serration-glands of Reinke.

May I suggest to some of your correspondents that information as to how far north in Great Britain or in Europe the glands on the above fruit trees are attended by ants and especially if the wild cherry (which I have not had an opportunity of observing) is so attended, would be of great interest.

THOMAS BELT

Cornwall House, Ealing, June 8

On Time

"The fact is, that we have not yet quite cast off the tendency to so-called metaphysics."—Tait, "Rec. Adv. in Phys. Sc.," p. 11.

IN Thomson and Tait's "Natural Philosophy," of which I have only the German edition in my possession, I find, § 246: "Die Zeiten, während welcher irgend ein besonderer Körper, der durch keine Kraft angetrieben wird, die Geschwindigkeit seiner Bewegung zu ändern, gleiche Wege durchläuft, sind einander gleich." And § 247: "Dieser Satz drückt bloss die für die Messung der Zeit allgemein getroffene Uebereinkunft aus."

These quotations quite express what is generally understood.

Yet in the definition of the equality of two lapses of time there is a logical fault. It is not allowed implicitly to introduce in a definition what is to be defined. There is no body of which we know *à priori* that no force tries to alter its velocity; in order to ascertain this, we must find out in consistency with the usual definition of force, given in § 217, whether it moves through equal spaces in equal times.

The definition of § 246, therefore, really says: The times, in which a body that goes through equal spaces in equal times moves through equal spaces are equal. It is evident that we are reasoning in a circle.

I am very well aware of the objection which will be made. We have it in § 245: "Auch werden wir später sehen, dass ein vollkommen glatter sphärischer Körper, welcher aus concentrischen Schalen besteht, deren jede von gleichförmiger Material und überall von derselben Dichtigkeit ist, sich, wenn man ihn in eine Drehung um eine Axe versetzt hat, trotz hinzutretender einwirkender Kräfte mit gleichförmiger Winkelgeschwindigkeit dreht, und seine Rotationsaxe in einer absolut festen Richtung erhällt." Thereupon it is said in § 247 that the earth is a body which fulfils these conditions very nearly, and that therefore its rotation gives us the means to measure time. But this assertion is not at all proved.

I now request my readers to be so good as to follow the exposition of *my* view. I assume that we are able to decide whether two lapses of time are equal. How this is done I shall dwell on afterwards.

When the conception of time is combined with the conception of motion we arrive at the building up of kinematics, in which the ideas of velocity and of acceleration are introduced. In abstract dynamics the idea of force is first introduced, wholly separated from any definite physical sense. As soon as the state of motion of a body (which is determined by the magnitude and the direction of its velocity) undergoes a change, we think of a cause of this change, and call this cause a force. We ascribe to a force magnitude and direction. If a body, which primarily is in rest, acquire a rectilinear motion, the force has constant direction. The magnitude of a force of constant direction is judged by the increase of velocity, which it gives in a definite time to a body primarily in rest. If the increases of velocity in equal times be equal, the force has constant magnitude. Two forces of constant direction and magnitude are in the same proportion as the increases of velocity which they give in equal times to the same body. Unity of force is the force which in unity of time gives to a particular body unity of increase of velocity.

It is conceivable that equal forces acting on different bodies cause different accelerations. Therefore another idea is introduced—the idea of mass. It is settled by definition that the masses of two bodies are in inverse proportion to the accelerations which they receive from equal forces. To a particular body unity of mass is ascribed. Unity of force is the force giving to unity of mass unity of acceleration.

I need not dwell on other ideas which are introduced, *e.g.*, moment, work, energy, &c. The whole building can be constructed, and there is room for every investigation which belongs to so-called theoretical mechanics. So it is demonstrated that a centrobaric body, which has kinetical symmetry in respect to its centre of gravity, and which has been brought in rotation about an axis going through the centre of gravity, retains constant angular velocity, when no forces are acting on the surface, and on the component parts only central forces which are in the same proportion as the masses of the parts.

Before kinematics and abstract dynamics can be applied in interpreting phenomena, we must be enabled to measure time.

What is time? There are mental conceptions which cannot be described by words, and I reckon "time" amongst them. But I shall try to answer the question how the conception of time originates with us.

The formation of the conception of "time" is preceded by the formation of the idea of "lapse of time." The idea of "lapse of time" we arrive at by the simultaneous observation of two phenomena, in conjunction with the observation of two phenomena not occurring simultaneously, in such a manner that we receive the impression of the second phenomenon when the impression of the first one is not yet effaced from our memory.

A lapse of time, from the nature of the idea, is limited. If we abstract the *definite* limits, we have the conception of time.

It is clear that in speaking of the measuring of time we properly mean the measuring of lapses of time.

In order to measure lapses of time we must know when a lapse of time is twice as long as another. We easily come to this on its having been established which lapses of time are equal.

If we wish to compare the length of two bodies we place the one beside the other, or if circumstances prevent us from doing so, we successively place a third object beside each of them.

For the comparison of two lapses of time we lack such means and have to follow another way.

In nature phenomena present themselves that persistently return. *Now we simply settle by definition that the lapses of time between the first occurrence of a particular phenomenon and the second is equal to that between the second and third occurrence.* Which phenomenon is to be chosen? Flux and reflux? Earthquakes? For the application of kinematics and abstract dynamics in interpreting phenomena, the choice is no indifferent matter.

I confine myself to the phenomenon which is still the usual base of the measurement of time. It is settled by definition that the lapses of time between the successive culminations of a definite fixed star in a definite place are equal. To divide these lapses of time themselves into equal parts, it is settled that the apparent motion of the fixed star, and therefore of all fixed stars, is uniform.

The results arrived at in the attempts at interpreting phenomena show that a very good hit has been made. But it is not impossible that after greater development of science we may have to make the measurement of time independent of the rotation of the earth. The application of abstract dynamics to the theory of the motion of the earth round the sun and of the moon round the earth has furnished admirable results. But in comparing the results of calculation with the accounts of eclipses found in ancient chronicles, a difference is met with, and in the opinion of some it is too considerable to be accounted for by the imperfection which may adhere to ancient descriptions. Therefore the theory of the motion of the earth and of the moon is incomplete. But hitherto no omission can be pointed out. For this reason some men of science are inclined to *settle by definition* that the theory of the motion of the earth and of the moon is complete, and to make it the base of the measurement of time. Then, of course, the former definition must be abandoned, and two arbitrary intervals between successive culminations of a fixed star no longer are equal.

Prof. Clerk Maxwell says ("Theory of Heat," second edition, p. 81): "This shows that time, though we conceive it merely as the succession of our states of consciousness, is capable of measurement, independently, not only of our mental states, but of any particular phenomenon whatever." In my opinion this assertion is erroneous. If we reject the rotation of the earth as the base of the measurement of time, we must have recourse to the motion of the earth round the sun or to that of the moon round the earth, or to any other phenomenon. Thomson and Tait, in § 406, already allude to a metal spring oscillating *in vacuo*. It should then be *settled by definition*, for example, that such a spring has a harmonical motion. If we proclaimed the lapses of time between the successive arrivals of flux and reflux at a particular station to be equal, and if we admitted, in order to divide these lapses of time into equal parts, *e.g.*, that the water sinks and rises uniformly, *then the whole of kinematics and abstract dynamics would retain the same form;* even then a centrobaric body with kinetical symmetry in respect to its centre of gravity, would show the peculiarity already mentioned. But it would be seen that our kinematics and abstract dynamics were but a highly deficient aid for the interpretation of phenomena; and the earth would not at all be a body with the same motion round its axis, as if it were a centrobaric body with kinetical symmetry in respect to its centre of gravity.

Of course it is wise to maintain provisionally the definition by which the earth in equal times rotates through equal angles.

In applying abstract dynamics to the interpretation of phenomena, we are led to identify the idea of mass with the idea of quantity of matter, and this has furnished excellently satisfying results. From this, in conjunction with experiment, it follows that two bodies which have equal weight, possess equal quantities of matter; that no matter is annihilated or created, &c.

This article is already too long for me to dwell on other consequences which follow from my view. Only a few words on the conservation of energy. This law threatens to be considered an axiom. Yet I believe it desirable that we should always remember that it is the result of experiment. If the measuring of time were founded on a different basis, it would not hold. Still the experiments do not give perfectly satisfying results. Usually this is ascribed to the imperfection of our methods and instruments, which really may be the cause. But, probably, if in future times it be found by improved methods and instruments that the law does not hold, it would be advantageous to proclaim by definition the conservation of energy and to deduce from it the measurement of time. Then we should have the analogon of the absolute scale of temperature of Thomson.

If any one after the perusal of this article asserts that my views are at variance with the historical development of science, I answer that often in the reasoning of man there are gaps, which by contemporaries are not perceived; but that we must try to find them out and to fill them.

I hope my readers will not be too much annoyed by the defective manner in which I may have expressed myself in English; it is always difficult to make use of a foreign language.

Before closing I am bound to state that I have particularly mentioned the assertions of Sir W. Thomson, Prof. Tait, and Prof. Clerk Maxwell, because in their works I found most emphatically stated what in my opinion is erroneous. These eminent men stand so high that it is unnecessary for me to express my profound respect for them.

V. A. JULIUS

Roermond, Holland

OUR ASTRONOMICAL COLUMN

THE D'ANGOS COMET OF 1784.—Encke's investigation relating to this reported comet appears in Zach's *Correspondance Astronomique*, as an "Imposture Astronomique grossière du Chevalier D'Angos, dévoilée par J. F. Encke, à Gotha." Olbers, in a letter addressed to him, had, as already stated, asked his attention to the subject, saying, "I would invite you to the examination of a doubtful comet of which the result will be either the knowledge of the yet unknown orbit of a very remarkable comet, or the discovery of a most shameful imposture," and adding particulars to which allusion is made in our previous note.

Encke remarks at the outset that, contrary to all general usage amongst astronomers, D'Angos had given the Malta observations with mean times for Paris, and the comet's positions expressed in longitudes and latitudes, which confirmed the suspicion that he had computed from the elements of an imaginary orbit and had not taken the trouble to convert the results into right ascensions and declinations, in which astronomers are accustomed to present them. If it is demonstrable that according to the observations (at least without supposing them erroneous to the amount of many minutes) the comet could only have moved in a very improbable orbit, in fact almost as a satellite of the earth and at a distance less than that of the moon, and if further it can be shown that by a very simple error of calculation D'Angos was misled in deducing the places of the comet from the imaginary elements, then, Encke urged, there remains no longer the smallest doubt that he had invented all these observations. Making use of the positions given for April 15, 22, and 29, Encke assuming arbitrarily a value of the comet's curtate distance from the earth at the first date, finds the corresponding value for the same at the third date in order to represent precisely the longitude on April 22, and compares with the corresponding latitude. Thus if the curtate distance on April 22 be taken as 0·42 (we somewhat contract Encke's figures) the third distance is 0·55, the error on the middle latitude, − 16′, and the resulting conic section is a hyperbola; the same form of orbit is deduced when the comet's distance on April 22 is diminished to 0·25. If this distance be further diminished to 0·146, the orbit becomes an ellipse, but the error on the middle latitude is still −12″·8, and it was found necessary to reduce the curtate distance to 0·00126 in order to represent this latitude with no greater error than −2″·5; the resulting orbit being also an ellipse. Taking the solar parallax at 8″·86, this distance corresponds to 116,000 miles, or about half the moon's distance from the earth, and under the condition named above, on April 29 it would still be less than 160,000 miles. Thus Encke found it was necessary to assume the comet's distance from the earth, almost incredibly small if the errors of calculation are to be brought within the limit assigned by D'Angos to the differences between the places computed from his elements and his observations, or about 1¼′; and, he continues, a celestial body under such circumstances remaining for so long a time in immediate proximity to the earth, would assuredly have been retained within its sphere of activity, and D'Angos if he were the first would certainly not have been the only observer of this second moon. Rejecting then as beyond probability the conclusions necessarily drawn from an investigation in the manner here briefly described, Encke proceeded to examine the calculation of geocentric longitudes and latitudes of the comet from the elements assigned by D'Angos.

Taking for example the observation of April 15, the logarithm of the radius-vector calculated from the orbit of D'Angos is found to be 9·8208333, and continuing the computation the resulting position differs from the observation 47° in longitude and 15° in latitude, but supposing that by an error of the pen D'Angos had used a log. radius-vector ten times greater, or 0·8208333, with the same heliocentric longitude and latitude, the errors are reduced to 56 seconds and 34 seconds respectively, and making the same change in the log. radii-vectores at the other dates of observation, Encke arrived at the extraordinary result that the whole of the reputedly-observed places were represented within about the limit of error mentioned by D'Angos, and he insisted that with such proof there could be no possible doubt that the observations and the orbit of the comet, "ne soient entièrement fausses et controuvées, et que par conséquent il faut les rayer de tous nos catalogues des comètes, comme un astre chimerique qui n'a jamais existé;" while at the same time he acknowledged himself ready to do justice to the accused and to make the most ample satisfaction if he could be opposed by arguments as strong and apparently conclusive as those upon which he had founded an adverse verdict.

THE TOTAL SOLAR ECLIPSE, 1889, DECEMBER 22.—In continuation of notices of future total eclipses of the sun which have appeared in this column, the elements of the eclipse of 1889, December 22, are subjoined:—

G.M.T. of Conjunction in R.A. Dec. 22, at 0h. 24m. 50s.

	°	′	″
R.A.	271	0	10·4
Moon's hourly motion in R.A. ...		41	20·7
Sun's ,, ,, ,, ...		2	46·6
Moon's declination	23	14	1·8 S.
Sun's ,, ...	23	27	10·3 S.
Moon's hourly motion in decl. ...		3	4·2 S.
Sun's ,, ,, ,, ...		0	1·1 N.
Moon's horizontal parallax		61	17·1
Sun's ,, ,, ...			9·1
Moon's semi-diameter		16	42·0
Sun's ,, ...		16	16·1

The central and total eclipse commences in long. 78° 52′ W., lat. 15° 22′ N., and ends in long. 60° 55′ E. and lat. 6° 53′ N., and it occurs with the sun on the meridian in long. 6° 27′ W., lat. 11° 5′ S.

At a point on the coast of Africa in 10° 6′ S. lat., totality commences at 2h. 8m. 55s. local time, and continues 3m. 34s. At Bridgetown, Barbadoes, totality begins at 6h. 47m. 6s. A.M. local time, and continues 1m. 48s., but the sun's altitude is only 6°. The following are points upon the central line, which will show that with a fair duration where the sun is near the meridian, the course of the eclipse is not a favourable one for observation:—

Long.	Lat.	Long.	Lat.
° ′	° ′	° ′	° ′
59 22 W.	13 30 N.	5 5 E.	11 23 S.
47 39	7 37 N.	14 0	9 45
32 28 W.	0 23 S.	18 32 E.	8 27 S.

HOW TO DRAW A STRAIGHT LINE[1]

III.

BEFORE leaving the Peaucellier cell and its modifications, I must point out another important property they possess besides that of furnishing us with exact rectilinear motion. We have seen that our simplest linkwork enables us to describe a circle of any radius, and if we wished to describe one of ten miles radius the proper course would be to have a ten mile link, but as that would be, to say the least, cumbrous, it is satisfactory to know that we can effect our purpose with a much smaller apparatus. When the Peaucellier cell is mounted for the purpose of describing a straight line, as I told you, the distance between the fixed pivots must be the same as the length of the "extra" link. If this distance be not the same we shall not get straight lines described by the pencil, but circles. If the difference be slight the circles described will be of enormous magnitude, decreasing in size as the difference increases. If the distance Q O, Fig. 6, be made greater than Q C, the convexity of the portion of the circle described by the pencil (for if the circles are large it will of course be only a portion which is described) will be towards O, if less the concavity. To a mathematician, who knows that the inverse of a circle is a circle, this will be clear, but it may not be amiss to give here a short proof of the proposition.

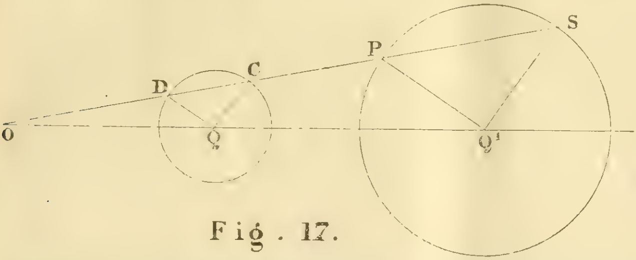

Fig. 17.

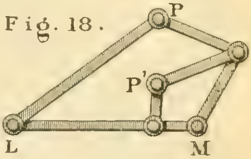

Fig. 18.

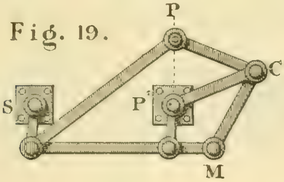

Fig. 19.

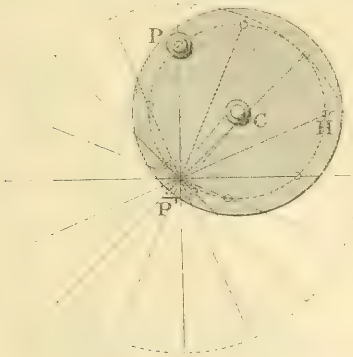

Fig. 20.

In Fig. 17 let the centres Q, Q′ of the two circles be at distances from O proportional to the radii of the circles. If then O D C P S be any straight line through O, D Q will be parallel to P Q′, and C Q to S Q′, and O D will bear the same proportion to O P that O Q does to O Q′. Now considering the proof we gave in connection with Fig. 7, it will be clear that the product O D · O C is constant, and therefore since O P bears a constant ratio to O D, O P · O C is constant. That is, if O C · O P is constant and C describes a circle about Q, P will describe one about Q′. Taking then O, C, and P as the O, C, and

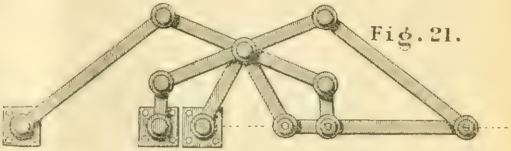

Fig. 21.

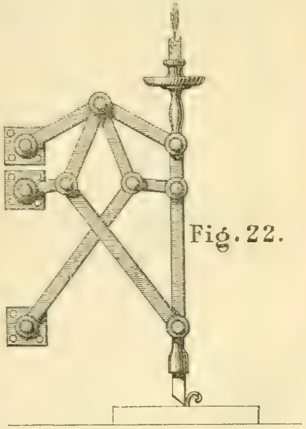

Fig. 22.

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[1] Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A. Continued from p. 89.

P of the Peaucellier cell in Fig. 7, we see how P comes to describe a circle.

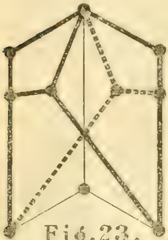
Fig. 23.

It is hardly necessary for me to state the importance of the Peaucellier compass in the mechanical arts for drawing circles of large radius. Of course the various modifications of the "cell" I have described may all be employed for the purpose. The models exhibited by the Conservatoire and M. Breguet are furnished with sliding pivots for the purpose of varying the distance between O and Q, and thus getting circles of any radius.

My attention was first called to these linkworks by the lecture of Prof. Sylvester, to which I have referred. A passage in that lecture in which it was stated that there were probably other forms of seven-link parallel motions besides M. Peaucellier's, then the only one known, led me to investigate the subject, and I succeeded in obtaining some new parallel motions of an entirely different character to that of M. Peaucellier. I shall bring two of these to your notice as the investigation of them will lead us to consider some other linkworks of importance.

If I take two kites, one twice as big as the other, such that the long links of each are twice the length of the short ones, and make one long link of the small kite lie on a short one of the large, and a short one of the small on a long one of the large, and then amalgamate the coincident links, I shall get the linkage shown in Fig. 18.

The important property of this linkage is that, although we can by moving the links about, make the points P and P′ approach to or recede from each other, the imaginary line joining them is always perpendicular to that drawn through the pivots on the bottom link L M. It follows that if either of the pivots P or P′ be fixed, and the link L M be made to move so as always to remain parallel to a fixed line, the other point will describe a straight line perpendicular to the fixed line. Fig. 19 shows you the parallel motion made by fixing P′. It is unnecessary for me to point out how the parallelism of L M is preserved by adding the link S L, it is obvious from the figure. The straight line which is described by the point P is perpendicular to the line joining the two fixed pivots; we can, however, without increasing the number of links make a point on the linkwork describe a straight line inclined to the line S P at any angle, or rather we can, by substituting for the straight link P C a plane piece, get a number of points on that piece moving in every direction.

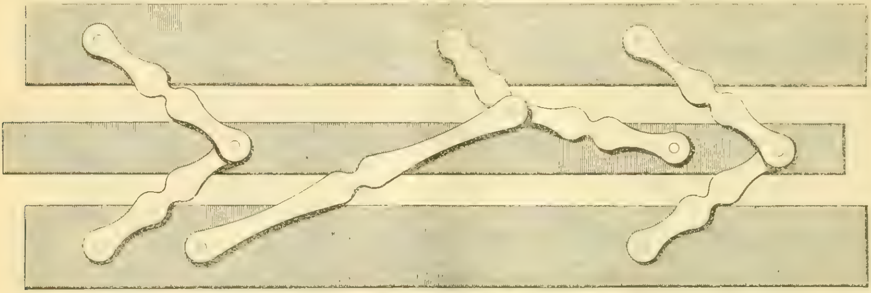
Fig. 24.

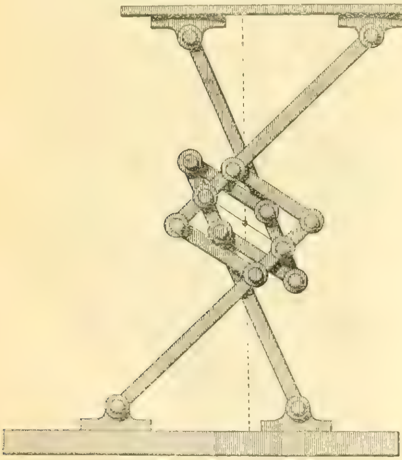
Fig. 25.

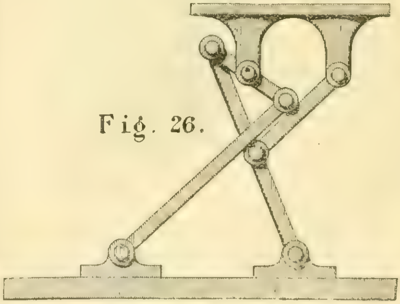
Fig. 26.

In Fig. 20, for simplicity, only the link C P′ and the new piece substituted for the link P C are shown. The new piece is circular and has holes pierced in it all at the same distance—the same as the lengths P C and P′ C—from C. Now we have seen from Fig. 19 that P moves in a vertical straight line, the distance P C in Fig. 20 being the same as it was in Fig. 19; but from a well-known property of a circle, if H be any one of the holes pierced in the piece, the angle H P′ P is constant, thus the straight line H P′ is fixed in position, and H moves along it; similarly all the other holes move along in straight lines passing through the fixed pivot P′, and we get straight line motion distributed in all directions. This species of motion is called by Prof. Sylvester "tram-motion." It is worth noticing that the motion of the circular disc is the same as it would have been if the dotted circle on it rolled inside the large dotted circle; we have, in fact, White's parallel motion reproduced by linkwork. Of course, if we only require motion in one direction, we may cut away all the disc except a portion forming a bent arm containing C, P, and the point which moves in the required direction.

The double kite of Fig. 18 may be employed to form some other useful linkworks. It is often necessary to have, not a single point, but a whole piece moving so that all points on it move in straight lines. I may instance the slide rests in lathes, traversing tables, punches, drills, drawbridges, &c. The double kite enables us to produce linkworks having this property. In the linkwork of Fig. 21, the construction of which will be at once appreciated if you understand the double kite, the horizontal link moves to and fro as if sliding in a fixed horizontal straight tube. This form would possibly be useful as a girder for a drawbridge.

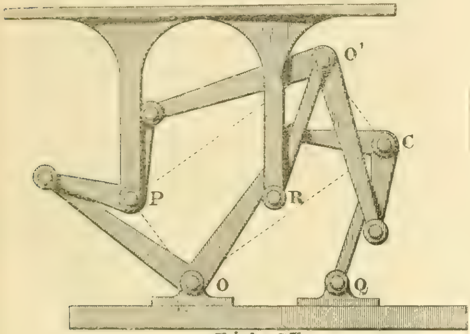

Fig. 27.

In the linkwork of Fig. 22, which is another combination of two double kites, the vertical link moves so that all its points move in horizontal straight lines. There is a modification of this linkwork which will, I think, be found interesting. In the linkage in Fig. 23, which, if the thin links are removed, is a skeleton drawing of Fig. 22, let the dotted links be taken away and the thin ones be inserted; we then get a linkage which has the same property as that in Fig. 22, but it is seen in its new form to be the ordinary double parallel ruler with three added links. Fig. 24 is a figure of a double parallel rule made on this plan with a slight modification. If the bottom ruler be held horizontal the top moves vertically up and down the board, having no lateral movement.

While I am upon this sort of movement I may point out an apparatus exhibited in the Loan Collection by Prof. Tchebicheff which bears a strong likeness to a complicated camp-stool, the seat of which has horizontal motion. The motion is not strictly rectilinear; the apparatus being, as will be seen by observing that the thin line in the figure is of invariable length, and a link might therefore be put where it is, a combination of two of the parallel motions of Prof. Tchebicheff given in Fig. 4, with some links added to keep the seat parallel with the base. The variation of the upper plane, from a strictly horizontal movement is therefore double that of the tracer in the simple parallel motion.

Fig. 26 shows how a similar apparatus of much simpler construction employing the Tchebicheff approximate parallel motion can be made. The lengths of the links forming the parallel motion have been given before (Fig. 4). The distance between the pivots on the moving seat is half that between the fixed pivots, and the length of the remaining link is one-half that of the radial links.

An *exact* motion of the same description is shown in Fig. 27. O, C, O′, P are the four *foci* of the quadriplane shown in the figure in which the links are bent through a right angle, so that O C · O P is constant, and C O P a right angle. The focus O is pivoted to a fixed point, and C is made by means of the extra link Q C to move in a circle of which the radius Q C is equal to the pivot distance O Q. P consequently moves in a straight line parallel to O Q, the five moving pieces thus far described constituting the Sylvester-Kempe parallel motion. To this are added the moving seat and the remaining link R O′, the pivot distances of which, P R and R O′, are equal to O Q. The seat in consequence always remains parallel to Q O, and as P moves accurately in a horizontal straight line, every point on it will do so also. This apparatus might be used with advantage where a very smoothly-working traversing table is required.

(*To be continued.*)

SPONTANEOUS GENERATION[1]

THE investigation embodied in the memoir now submitted to the Society was opened in the summer of 1876 by a series of tentative experiments on turnip-infusions, to which were added varying quantities of bruised or pounded cheese. I was soon, however, drawn away from them to other experiments on infusions of hay. With this substance no difficulty was encountered in my first inquiry. Boiled for five minutes, and exposed to air purified spontaneously or freed from its floating matter by calcination or filtration, hay infusion, though employed in multiplied experiments at various times, never showed the least competence to kindle into life. After months of transparency, I have, in a great number of cases, inoculated this infusion with the smallest specks of animal and vegetable liquids containing *Bacteria*, and observed twenty-four hours afterwards, its colour lightened, and its mass rendered opaque by the multiplication of these organisms.

But in the autumn of 1876, the substance with which I had experimented so easily and successfully a year previously, appeared to have changed its nature. The infusions extracted from it bore in some cases not only five minutes' but fifteen minutes' boiling with impunity. But on changing the hay a different result was often obtained. Many of the infusions extracted from samples of hay purchased in the autumn of 1876, behaved exactly like those extracted from the hay of 1875, being completely sterilized by five minutes' boiling.

To solve these discrepancies, numerous and laborious experiments were executed with hay derived from different localities, and by this means in the earlier days of the inquiry, it was revealed that the infusions which manifested this previously unobserved resistance to sterilization were, one and all, extracted from old hay, while the readily sterilized infusions were extracted from new hay, the germs adhering to which had not been subjected to long-continued desiccation.

I then fell back upon infusions whose deportment had been previously familiar to me, and in the sterilization of which I had never experienced any difficulty. Fish, flesh, and vegetables were re-subjected to trial. Though the precautions taken to avoid contamination were far more stringent than those observed in my first inquiry, and though the interval of boiling was sometimes tripled in duration, these infusions, in almost every instance, broke down. Spontaneously purified air, filtered air, and calcined air,—calcined, I may add, with far greater severity than was found necessary a year previously,—failed, in almost all cases, to protect the infusions from putrefaction.

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[1] "Further Researches on the Deportment and Vital Resistance of Putrefactive and Infective Organisms, from a Physical Point of View." By John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution.—Abstract.

I had the most implicit confidence in the correctness of my earlier experiments; indeed, incorrectness would have led to consequences exactly opposite to those arrived at. Errors of manipulation would have filled my tubes and flasks with organisms instead of leaving them transparent and void of life. By the unsuccessful experiments above referred to a clear issue was therefore raised: Either the infusions of fish, flesh, and vegetable had become endowed in 1876 with an inherent generative energy which they did not possess in 1875, or some new contagium external to the infusions, and of a far more obstinate character than that of 1875, had been brought to bear upon them. The scientific mind will not halt in its decision between these two alternatives.

For my own part the gradual but irresistible interaction of thought and experiment rendered it at first probable, and at last certain that the atmosphere in which I worked had become so virulently infective as to render utterly impotent precautions against contamination, and modes of sterilization, which had been found uniformly successful in a less contagious air. I therefore removed from the laboratory, first to the top, and afterwards to the basement of the Royal Institution, but found that even here, in a multitude of cases, failure was predominant, if not uniform. This hard discipline of defeat was needed to render me acquainted with all the possibilities of infection involved in the construction of my chambers and the treatment of my infusions.

I finally resolved to break away from the Royal Institution, and to seek at a distance from it a less infective atmosphere. In Kew Gardens, thanks to our President, the requisite conditions were found. I chose for exposure in the Jodrell laboratory the special infusions which had proved most intractable in the laboratory of the Royal Institution. The result was that liquids which in Albemarle Street resisted two hundred minutes boiling, becoming fruitful afterwards, were utterly sterilised by five minutes' boiling at Kew.

A second clear issue is thus placed before the Royal Society:—Either the infusions had lost in Kew Gardens an inherent generative energy which they possessed in our laboratory, or the remarkable instances of life development, after long-continued boiling, observed in the laboratory are to be referred to the contagium of its air.

With a view to making nearer home experiments similar to those executed at Kew, I had a shed erected on the roof of the Royal Institution. In this shed infusions were prepared and introduced into new chambers of burnished tin, which had never been permitted to enter our laboratory. After their introduction the liquids were boiled for five minutes in an oil-bath.

The first experiment in this shed resulted in complete failure, the air of the shed proving to be sensibly as infective as the air of the laboratory.

Either of two causes, or both of them combined, might, from my point of view, have produced this result. First, a flue from the laboratory was in free communication with the atmosphere not far from the shed; secondly, and this was the real cause of the infection, my assistants in preparing the infusions, had freely passed from the laboratory to the shed. They had thus carried the contagium by a mode of transfer known to every physician.

The infected shed was disinfected; the infusions were again prepared, and care was taken, by the use of proper clothes, to avoid the former causes of contamination. The result was similar to that obtained at Kew, viz., organic liquids which in the laboratory withstood two hundred minutes' boiling, were rendered permanently barren by five minutes' boiling in the shed.

A third clear issue is thus placed before us, which I should hardly think of formulating before the Royal Society, were it not for the incredible confusion which apparently besets this subject in the public mind. A rod thirty feet in length would stretch from the infusions in the shed to the same infusions in the laboratory. At one end of this rod the infusions were sterilized by five minutes' boiling, at the other end they withstood two hundred minutes' boiling. As before, the choice rests between two inferences:—Either we infer that at one end of the rod animal and vegetable infusions possess a generative power, which at the other end they do not possess; or we are driven to the conclusion that at the one end of the rod we have infected, and at the other end disinfected air.

The second inference is that which will be accepted by the scientific mind. To what, then, is the inferred difference at the two ends of the rod to be ascribed? In one obvious particular the laboratory this year differed from that in which my first experiments were made. On its floor were various bundles of old and desiccated hay, from which, when stirred, clouds of fine dust ascended into the atmosphere. This dust proved to be both fruitful and in the highest degree resistant. Prior to the introduction of the hay which produced the dust, no difficulty as regards sterilization had ever been experienced; subsequent to its introduction my difficulties and defeats began.

In these and numerous other experiments a method was followed which had been substantially employed by Spallanzani and Needham; and more recently by Wyman and Roberts, the method having been greatly refined by the philosopher last named. The flasks containing the infusions were only partially filled, the portions unoccupied by the liquids being taken up with ordinary unfiltered air. Now as regards the death-point of contagia, we know that in air it is higher than in water, the self-same temperature being fatal in the latter and sensibly harmless in the former. Hence my doubt whether, in my recent experiments, the resistance of the contagium did not arise from the fact that it was surrounded, not by water but by air.

I changed the method, and made a long series of experiments with filtered air. They were almost as unsuccessful as those made with ordinary air.

One source of discomfort clung persistently to my mind throughout these experiments. I was by no means certain that the observed development of life was not due to germs entangled in the film of liquid adherent to the necks and higher interior surfaces of the bulbs. This film might have dried, and its germs, surrounded by air and vapour, instead of by water, might on this account have been able to withstand an ordeal to which they would have succumbed if submerged.

A plan was, therefore, resorted to by which the infusions were driven by atmospheric pressure through lateral channels issuing from the centres of the bulbs. As before, each bulb was filled with one-third of an atmosphere of filtered air, and afterwards heated nearly to redness. When fully charged, the infusion rose higher than the central orifice, and no portion of the internal surface was wetted save that against which the liquid permanently rested. The lateral channel was then closed with a lamp without an instant's contact being permitted to occur between any part of the infusion and the external air. It was thus rendered absolutely certain that the contagia exposed subsequently to the action of heat were to be sought, neither in the superjacent air nor on the interior surfaces of the flasks, but in the body of the infusions themselves.

By this method I tested in the first place the substance which, at an early stage of the inquiry, had excited my suspicion—without reference to which the discrepancy between the behaviour of infusions examined in the winter of 1875-76 and those examined in the winter of 1876-77 is inexplicable, but by reference to which the explanation of the observed discrepancy is complete—I mean the old hay which cumbered our laboratory floor.

Four hours' continuous boiling failed to sterilise bulbs charged with infusions of this old hay. In special cases, moreover, germs were found so indurated and resistant, that five, six, and in one case even eight hours' boiling failed to deprive them of life. All the difficulties encountered in this long and laborious inquiry were traced to the germs which exhibited the extraordinary powers of resistance here described. They introduced a plague into our atmosphere—the other infusions, like a smitten population, becoming the victims of a contagium foreign to themselves.[1]

It is a question of obvious interest to the scientific surgeon whether those powerfully resistant germs are amenable to the ordinary processes of disinfection. It is perfectly certain that they resist to an extraordinary extent the action of heat. They have been proved competent to cause infusions, both animal and vegetable, to putrefy. How would they behave in the wards of a hospital? There are, moreover, establishments devoted to the preserving of meats and vegetables. Do they ever experience inexplicable reverses. I think it certain that the mere shaking of a bunch of desiccated hay in the air of an establishment of this character might render the ordinary process of boiling for a few minutes utterly nugatory, thus possibly entailing serious loss. They have, as will subsequently appear, one great safeguard in the complete purgation of their sealed tins of air.

Keeping these germs and the phases through which they pass to reach the developed organism clearly in view, I have been able to sterilise the most obstinate infusions encountered in this inquiry by heating them for a minute fraction of the time above referred to as *insufficient* to sterilise them. The fully developed Bacterium is demonstrably killed by a temperature of 140° F. Fixing the mind's eye upon the germ during its passage from the hard and resistant to the plastic and sensitive state, it will appear in the highest degree probable that the plastic stage will be reached by different germs in different times. Some are more indurated than others and require a longer immersion to soften and germinate. For all known germs there exists a period of incubation during which they prepare themselves for emergence as the finished organisms which have been proved so sensitive to heat. If during this period, and well within it, the infusion be boiled for even the fraction of a minute, the softened germs which are then approaching their phase of final development will be destroyed. Repeating the process of heating every ten or twelve hours, and before the least *sensible* change has occurred in the infusions, each successive heating will destroy the germs then softened and ready for destruction, until after a sufficient number of heatings the last living germ will disappear.

Guided by the principle here laid down, and applying the heat discontinuously, infusions have been sterilised by an aggregate period of heating, which, fifty times multiplied, would fail to sterilise them if applied continuously. Four minutes in the one case can accomplish what four hours fail to accomplish in the other.

If properly followed out the method of sterilisation here described is infallible. A temperature, moreover, far below the boiling point suffices for sterilisation.

Another mode of sterilisation equally certain, and perhaps still more remarkable, was forced upon me, so to speak, in the following way:—In a multitude of cases a thick and folded layer of fatty scum, made up of matted *Bacteria*, gathered upon the surfaces of the infusions, the liquid underneath becoming sometimes cloudy throughout, but frequently maintaining a transparency equal to that of distilled water. The living scum-layer, as Pasteur has shown in other cases, appeared to possess the power of completely intercepting the atmospheric oxygen, appropriating the gas and depriving the germs in the liquid underneath of an element necessary to their development. Above the scum, moreover, the interior surfaces of the bulbs used in my experiments were commonly moistened by the water of condensation. Into it the *Bacteria* sometimes rose, forming a kind of gauzy film to a height of an inch or more above the liquid. In fact, wherever air was to be found, the *Bacteria* followed it. It seemed a necessity of their existence. Hence the question, What will occur when the infusions are deprived of air?

I was by no means entitled to rest satisfied with an inference as an answer to this question; for Pasteur, in his masterly researches, has abundantly demonstrated that the process of alcoholic fermentation depends on the continuance of life without air—other organisms than *Torula* being also shown competent to live without oxygen. Experiment alone could determine the effect of exhaustion upon the particular organisms here under review. Air-pump vacua were first employed, and with a considerable measure of success. Life was demonstrably enfeebled in such vacua.

Sprengel pumps were afterwards used to remove more effectually both the air dissolved in the infusions and that diffused in the spaces above them. The periods of exhaustion varied from one to eight hours, and the results of the experiments may be thus summed up:—Could the air be completely removed from the infusions, there is every reason to believe that sterilisation *without boiling* would in most, if not in all cases, be the result. But, passing from probabilities to certainties, it is a proved fact, that in numerous cases unboiled infusions deprived of air by five or six hours' action of the Sprengel pump are reduced to permanent barrenness. In a great number of cases, moreover, where the unboiled infusion would have become cloudy, exposure to the boiling temperature for a single minute sufficed completely to destroy the life already on the point of being extinguished through defect of air. With a single exception, I am not sure that any infusion escaped sterilisation by five minutes' boiling after it had been deprived of air by the Sprengel pump. These five minutes accomplished what five hours often failed to accomplish in the presence of air.

The inertness of the germs in liquids deprived of air is not due to a mere *suspension* of their powers. They are *killed* by being deprived of oxygen. For when the air which has been removed by the Sprengel pump is, after some time, carefully restored to the infusion, unaccompanied by germs from without, there is no revival of life. By removing the air we stifle the life which the returning air is incompetent to restore.

AGRICULTURAL EXPERIMENTS AT WOBURN

IN the autumn of 1875 Mr. C. Randell proposed to the Council of the Royal Agricultural Society that it be referred to the Chemical Committee to consider the propriety, and the manner, of instituting a series of experiments, to test the accuracy of the estimated value of manure obtained by the consumption of different articles of food, as given in Mr. Lawes' paper, in the Spring Number of the Journal of the Society.

As it was decided that experiments by practical farmers in different districts could not be relied on, the Duke of

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[1] A hard and wiry hay from Guildford, which I have no reason to consider was found very difficult to sterilise.

Bedford very kindly offered to afford facilities for making new experiments at his own cost.

Mr. Lawes and Dr. Voelcker were requested to draw up a scheme for carrying on, at Woburn, such experiments as they, in communication with the Chemical Committee, might determine on. His grace offered to give up for the purpose Crawley Mill Farm, comprising about ninety acres, with the house and buildings. But, on examination, it was found that there was no sufficient area on that farm so even in character, and in condition, of soil, as to render it available for a considerable series of comparative field experiments. Eventually, after inspection of many others, a large field of much more suitable land was selected, on Birchmoor Farm. Crawley Mill Farm is, however, also retained, as a means of providing a residence for the Superintendent of the experiments, the requisite buildings, and the opportunity of having at command the necessary horse and hand labour for the experiments. Mr. P. H. Cathcart, formerly at the Royal Agricultural College, Cirencester, has been appointed the Resident Superintendent of the experiments.

As experiments to determine the value of the manure obtained by the consumption of purchased foods obviously involved the necessity of feeding animals under conditions in which the manure could be collected with as little loss as possible, the Duke of Bedford has erected eight very complete feeding boxes, in which the manure for the experimental barley and root crops recently sown has been made.

The field devoted to the field experiments has an area of twenty-seven acres; the soil has been carefully tested all over, and an account taken of the history of the field since 1874.

It was considered important, especially with reference to valuations under the Agricultural Holdings' Act, to add, if possible, to our knowledge of the manure-value of both artificial manures and consumed feeding stuffs; and it was decided, therefore, both to compare the effects of the manure obtained by the consumption of selected purchased foods, with those obtained by artificial manures estimated to supply the same constituents, and also to determine the effects of dung and artificial manuring substances, applied year after year, on the Woburn soil, and to compare these with the results obtained for so many years, with the same manures, on the very different soil at Rothamsted. Accordingly, 2¾ of the six acres where wheat had been grown in 1876, after tares and turnips, each fed with cake, are devoted to the continuous growth of wheat, and 2¾ acres to the continuous growth of barley. In each case the area is divided into eleven plots, of a quarter of an acre each.

The description and quantities of the manures for these experiments have been so carefully selected that in the end valuable results must be obtained as to the comparative value of various kinds of artificial manure as compared with farmyard manure, the constituents of which are accurately known. Two of the plots are unmanured; seven are manured with artificial manure of more or less complicated composition, and two with farmyard manure estimated to contain different proportions of nitrogen. In connection with the farmyard manure an accurate record is kept of the kinds and quantities of food from which it is produced, as also of the increase in the live-weight of the stock thus fed.

Besides these continuous experiments a series of rotation experiments—seeds, wheat, roots, barley, in successive years from 1877 to 1881—are to be carried out. The stock which is to supply the farmyard manure for these experiments is to be fed with decorticated cotton-cake, which among purchased feeding stuffs has a very high manure value, and maize-meal, which has a very low manure value. The effects of the manures obtained by the consumption of these foods will be compared with those of artificial manures supplying, in one case the same amount of nitrogen, potass, phosphoric acid, &c., as is estimated to be contained in the manure from the cotton-cake consumed, and in another the same as in that from the maize-meal consumed. Accordingly, four feeding experiments have been conducted, in each of which the same amount of litter has been used, and the same amount of roots, and the same amount of wheat-straw chaff consumed. In Experiment 1, 1,000 lbs. decorticated cotton-cake were given in addition; and in Experiment 2, 1,000 lbs. maize-meal. In Experiments 3 and 4 no purchased food was given; but in Experiment 3 artificial manures estimated to contain the same amount of the chief constituents as the manure from 1,000 lbs. of cotton-cake, and in Experiment 4 the same as from 1,000 lbs. maize-meal, will be applied to the land, in addition to the root and chaff manure.

Four areas of four acres each have been devoted to these rotation experiments, eight of them coming into exact experiment this year, and the remaining eight in 1878. Each area of four acres is again divided into four plots, each of the latter sub-divisions bearing the same crop during the rotation of four years, but undergoing different treatment in the way of manure. For example, rotation No. 1, now under seeds, is treated as follows. Each plot is being separately fed by sheep. Plot 1 with cotton-cake; Plot 2 with maize-meal; and Plots 3 and 4 without purchased food. But, for the succeeding wheat, artificial manure estimated to contain nitrogen, and other constituents, in amounts equal to those in the manure from the consumed cotton-cake, will be applied to Plot 3, and artificial manure, equal to that from the consumed maize-meal will be applied to Plot 4. For the roots in 1879 (succeeding the wheat), the 4 acres will be manured as already described, and barley will complete the course in 1880. The other rotations are so treated as at the end of the four years to yield a collection of data that must be of the highest value in agricultural chemistry, and therefore to practical agriculture. In a "Statement" as to the objects and plan of the experiment which lies before us, full details are given on all points, and carefully constructed tables relating to every stage of the experiments, which show that all possible care has been taken to secure accuracy and practical utility in the results. The experiment will no doubt be anxiously watched by all interested in scientific agriculture.

NOTES

PROF. FRANKLAND, D.C.L., F.R.S., has now in the press a volume containing his collected researches in Pure, Applied, and Physical Chemistry, dedicated to Prof. Bunsen, of Heidelberg. The section on Pure Chemistry treats, amongst other matters, of the Isolation of the Organic Radicals, and the Discovery of Organo-Metallic Bodies, and their Application to the Synthetical Production of Organic Compounds. In the section devoted to Applied Chemistry, the author describes his Investigations on the Manufacture of Gas for Illuminating Purposes; on the Qualities of Potable Waters; and on the Treatment of the Sewage of Towns. Physical Chemistry includes his Experiments upon Flames, and upon the Source of Muscular Power, together with those on the Spectra of Gases and Vapours. Each chapter is preceded by introductory remarks, having reference to the scope, object, and future development of the subject treated of. Mr. Van Voorst is the publisher.

A MOVEMENT has been commenced in Spain for the formation of an association similar to the British Association. The Madrid Societies of Natural History, Anthropology, and Geography have appointed a joint commission to consider how best to organise an annual meeting in different parts of the kingdom for the purpose of investigating matters of scientific interest within the domain of these societies, and also to arrange for the publication of the results that may thus be obtained. A movement like this leads one to hope that a fair future is yet in store for Spain.

It is probable that the Sixth Congress of Russian Naturalists will not be held this year, the Government not having granted a sum of money for the expenses of the Congress, and private help being unlikely to be forthcoming on account of the war.

The fifth session of the International Congress of the Medical Sciences will be held at Geneva, from September 9 to 15. In connection with the Congress there will be an exhibition of new apparatus and instruments used in medicine, surgery, physiology, &c. Articles for exhibition should be sent free of all charges to the "Direction de l'Exposition du Congrès Médical; Dr. J. L. Reverdin, place du Lac, Geneva." Intending exhibitors should intimate before August 15 what space they are likely to require.

We hope shortly to give an account of the proceedings which took place in connection with the recent Gauss centenary celebration. We may here state that the festival speech was delivered by Prof. Dr. Sommer, that a sketch for a monument by the Berlin sculptor, Schæfer, was exhibited in the Festival Hall, and that his Majesty, the Emperor of Germany, was a contributor to the Memorial Fund. The following pamphlets have appeared:—"Briefe zwischen A. von Humboldt und Gauss. Herausgegeben von Dr. K. Bruhns;" "Gauss. Ein Umriss seines Lebens und Wirkens. Von F. A. T. Winnecke;" "Über die Anzahl der Ideal-Classen in den verschiedenen Ordnungen eines endlichen Körpers. Von R. Dedekind." The committee also intend to publish an account of Gauss's relations with Brunswick.

France appears to be becoming more and more anxious to do honour to her science worthies by the erection of statues. A statue to Arago is being erected at Perpignan, in the department of Orientales Pyrénées. Another to Niepce de Saint-Victor, a name well known in connection with improvements in photography, will be erected at Chalons, his native place, by public subscription, at the instance of the Municipal Council of the city. It is also stated that a public subscription will be opened at Lyons on behalf of Ampère, the inventor of the electro-magnet, and the precursor of Faraday in the invention of the inductive electricity. Ampère was born in that city in 1775, and his father was guillotined there on the Place des Terraux for having been active in the great royalist rebellion against the Convention, which ended in the famous siege of Lyons and his capture by Dubois-Crancé.

At the usual fortnightly meeting of the Royal Geographical Society on Monday, a paper on "Journeys up the Niger and Notes on the Neighbouring Countries," by Bishop Crowther, was read. The paper, which dealt with the journeys of Bishop Crowther in Western Africa, between 1841 and 1871, described the character of the river Niger, the villages of the natives upon its banks, the tribes scattered about the neighbouring countries, &c. It was remarked that the actual extent of the delta of the river was still uncertain, but the lecturer inclined to the opinion that the affluents of the river, and particularly the Bénuwé, on the south bank, if traced to their source would lead to a rich field of discovery. What might be called the delta of the river was a vast tract of marshy country extending along a coast line of 120 miles, and probably in parts some 150 miles in breadth. In the course of the journey of some 700 miles no less than thirteen tribes, speaking as many different languages, were met with. Ten of the tribes appeared to be of the same family, and might be classed as aboriginal. The Housas were a tribe spread in the widest direction, and the territory in which their language was spoken appeared to be more considerable than any in Africa. It was a beautiful language, and had become to Africa what French is to Europe. The other important language of that part of Africa was the Fulah. The Filanis were a remarkable people who had conquered extensive parts far to the south of the river Bénuwé. Dr. Barth stated that he had been told by natives of the interior that in bygone days an ancient kingdom called Ghanata had existed. The trade routes which meet the Egga on the Niger were important; the chief came from the north, from Tripoli, across the Sahara, with European produce on camels to the Nupe kingdom, where it was distributed in the neighbouring countries. It has been resolved by the Church Missionary Society to send out a small steamer, drawing only three feet of water, to push further into the interior, and afford assistance to Bishop Crowther to carry the missionary work more completely among the natives.

A Museum of Science and Arts has been established at St. Louis, U.S.

We are glad to learn that the experiments with Jablochkoff's Electric Light are to be repeated at the West India Dock tomorrow evening. We hope all will go well and fairly on this occasion so as to allow a real test to be made of the practical utility of the invention.

An Italian optician established in Paris has constructed a very sensitive metallic thermometer on a new principle. The dilations of a small sheet of platinised silver are amplified by means of a system of levers, and the motion is communicated to a needle on a dial, on which degrees are marked. The motion of the needle is almost instantaneous. The apparatus has been tested in the "Ville de Paris," a new balloon sent up on June 3 at Paris.

The St. Petersburg Society of Naturalists has intrusted Professors Fr. Schmidt and Inostrantseff with a geological exploration of the Valley of the Neva, from Schlüsselburg down to the Finnish Gulf. From the interest possessed by the glacial accumulations in this valley, as well as the qualifications of both professors for this special subject, we may expect much new light on the question of the glaciation of Northern Russia.

The *Turkestan Gazette* gives the latest news from M. Prshevalsky, dated from Lob Nor, February 22. After having reached this lake by the valley of the Lower Tarim, M. Prshevalsky advanced 130 miles east of the lake. The survey and the astronomical measurements of latitudes and longitudes he has made give a totally new aspect to the map of the country. The population on the banks of the Tarim and around the Lob Nor is very sparse; the people speak almost the same language as that of Eastern Turkestan. The flora and fauna of the locality are very poor; some vegetation is found only in the Tarim valley, the neighbourhood being a true desert. During February and March M. Prshevalsky was to stay in the Lower Tarim, during May at Yuldus, and during June at Kunghes. About the beginning of July he proposes to return to Kuldsha to begin in August his journey to the Tibet.

The *Gardener's Chronicle* learns that it is proposed to erect at Ootacamund, in the Neilgherry Hills, a statue of the late Mr. MacIvor, to whom the successful cultivation of Cinchona on those Indian slopes is so pre-eminently due.

We see from the Report of the Auckland (New Zealand) Institute for 1876-7, that that society is in a flourishing condition, and that during the year thirteen papers on subjects of scientific importance in connection with the Colony were read. About a year ago a fine new museum was opened, the cost of building having been 4,000*l*., half of which was raised by private subscription and half obtained as a grant from the New Zealand Government.

At a recent meeting of the St. Petersburg Technical Society, M. Chikolef made an interesting communication as to the experiments recently made at St. Petersburg for determining the lighting power of the electrical light at great distances. The power of the light is notably increased by covering the carbon of the lamp with a thin sheet of copper (one-sixteenth of the diameter of the carbon at its upper part, and from one-forty-eighth to one-sixty-fourth in its lower part). It depends also upon the direction given to the carbon, the best being to turn the cup towards the object to be lighted. The great machine of Alteneck, with a carbon of 12 millim. of diameter, gave a *maximum* of light equal to 10,210 candles, and a mean of 5,739 candles; whilst with a carbon of 10 millim., but galvanised, it gave a maximum of 16,255 candles (20,275 when the cup is turned as above) and a mean of 14,039 candles. The light was sufficient to make objects visible (for military purposes) at a distance of 3,080 yards. Of many machines used, the most economical proved to be the great one of Alteneck.

THE Society for Improvement of Public Health in Utrecht, offers a prize of 100 gulden for the best work on the liquid manure of stables, giving (1) an accurate account of the literature of the subject; (2) a description of original experiments on the means of obtaining from horse urine diluted with water, products which, either as manure or as a chemical preparation, may be brought into commerce in comparatively large quantities; (3) full numerical tables on quantitative chemical analyses made; (4) a thorough treatment of the financial side of the question. The memoirs may be written in Dutch, German, French, or English, and are to be sent, with sealed envelope and motto, to Prof. Dr. Th. Mac Gillvory, Director of the Veterinary School in Utrecht, before September, 1878.

THE very interesting discoveries in prehistoric archæology made by M. Kibalchich at Kief, were the subject of his last communication at the Russian Archæological Society. The numerous caves in the limestone on the banks of the Dnieper seem to have been a favourite haunt of men, even during the first ten centuries of our era. Very important objects have been found in these caves dating from the time of the introduction of Christianity in Russia, showing a remarkable mixture of articles used in Pagan and in Christian worship, establishing a link between Christian and Indian religious symbols. We notice especially those caves at Kief which date from the earliest stone period. They are very long, sinuous, but narrow, and contain great quantities of the plainest stone weapons and stone pearls, together with burned bones of various animals. Some facts lead us to infer the existence of lake-dwellings in the vicinity. Close to these oldest dwelling-places there exists a profusion of conic mounds of boulders and *koorganes* (high mounds of earth), or burial-places, coming from times anterior to the introduction of Christianity in Russia. They contain skeletons, often without skulls, which are buried separately, and a variety of weapons and utensils. The number of such burial-places at Kief and in its neighbourhood is very large. One cemetery of that epoch occupies twenty-three acres in the Fundukley Street, without reckoning the numerous "kitchen-mounds." The objects excavated by M. Kibalchich will form, it is hoped, the nucleus of an archæological museum to be opened at Kief. The excavations are to be continued.

THE *Panama Star and Herald* of the 21st ult., states that the destructive tidal wave experienced at Callao and the ports to the north of that place extended as far south as the northern boundary of Chili, but how much further south was not known, as the telegraph communication had been interrupted. The almost complete destruction was reported of Antofagasta, Iquique, Arica, Tambo do Moro, Pabellon de Pica, and Ilo. Severe shocks of earthquake were felt, but they caused little damage. The destruction of life and property was caused by the frightful upheaval and ingress of the sea. At Arica the sea washed over the town to the hill at the back of the church and destroyed much valuable property. The wreck of the United States steamer *Wateree*, carried inland a couple of miles by the tidal wave of 1868, was again floated, and carried a mile or two further up the coast. The sea in some places rose over sixty feet, and the destruction of life and property is believed to have been enormous.

THE final report of the Sub-Wealden Exploration has just been issued by Mr. H. Willett. He reports that the depth attained on December 21 last year was 1,823 feet, and on April 12, 1,905 feet. On the last-mentioned date a letter was sent by the Diamond Boring Company, stating they used the best endeavours to reach a depth of 2,000 feet and had failed, owing to the want of lining permitting the hole to fall in on the rods and jambing them. Mr. Willett writes:—"The Sub-Wealden exploration is, therefore, brought to a close, and has proved conclusively that in the lowest part of the Wealden area no palæozoic rocks exist within 1,900 feet of the surface. That the search was justified, and that the scientific deductions of Prof. Prestwich, F.R.S., were entitled to the highest consideration may be found in the fact that palæozoic rocks of the Devonian period have been discovered (containing spiriferæ) in the boring made at the brewery of Sir Henry Meux and Co., at the corner of Tottenham-Court Road London." Mr. Willett adds: "The search should undoubtedly be further promoted in the valley of the Thames and at other points in the south-east of England."

M. FERDINANDO TOMMASI has recently constructed a "thermodynamic motor," in which work is done by the mere dilatation of a liquid (oil) without change of state.

MR. SAMUEL HIGHLEY writes us:—"In connection with Mr. Atkinson's letter as to the phenomena connected with Japanese mirrors, and the question as to their method of manufacture, a few years ago Prof. Pepper exhibited the reflected figure formed by these specula when illuminated by a beam of oxyhydrogen light upon the screen at the Polytechnic Institution. In his 'Cyclopædic Science' the question of the method of the production of such mirrors is fully discussed. During the time the Japanese mirrors were being exhibited at the Polytechnic, an English brass-worker tried to solve the problem and apparently discovered the secret of the Japanese makers. He found that taking ordinary brass and stamping upon its surface with any suitable die, not once, but three times in succession, upon exactly the same spot, grinding down and polishing between each act of stamping, a molecular difference was established between the stamped and unstamped parts, so that images of the pattern could be reflected from the finally-polished surface, just as with the Japanese specula, though no difference of surface could be detected by the eye. One operation did not produce this result. Mr. James Princeps published an account of his investigations on this subject in the *Journal* of the Asiatic Society, vol. i., p. 242. He gives as the result of his anylysis of the Japanese alloy, copper, 80 parts; tin, 20—100; with no traces of silver or arsenic, but a slight indication of zinc. He supposed that the phenomena resulted from difference of *density* produced by means of stamping,and that the thinnest parts, from being the hardest, should give the stronger reflection."

IN the last number of the *Transactions* of the Institution of Engineers and Shipbuilders in Scotland is a paper read April 24, by Sir Wm. Thomson, on Compass Adjustment on the Clyde, the aim of the paper being to show that the Clyde is pre-eminently suitable for the adjustment of the compasses of ships under way.

THE ninth annual report on the noxious, beneficial, and other insects of the State of Missouri, by Chas. V. Riley, the State Entomologist, contains descriptions (with woodcuts) of the following insects :—The gooseberry span-worm (*Eufitchia ribearia*, Fitch), the imported currant worm (*Nematus ventricosus*, Klug.), the native currant worm (*Pristiphora grossulariæ*, Walsh), the strawberry worm (*Emphytus maculatus*, Norton), Abbot's white pine worm (*Lophyrus abbotii*, Leach), and Le Conte's pine worm (*Lophyrus le contei*, Fitch). There is an account of the progress of the Colorado beetle, the army worm, the wheat-head army worm, and the Rocky Mountain locust.

MUSCULAR contraction, it is known, is always accompanied with electric phenomena; the difference of electric potential between two points of a muscle, undergoes a diminution, which, according to Bernstein, precedes by about $\frac{1}{100}$ of a second, the contraction of the muscle. This *electric variation* has been observed on various muscles, and in particular on the heart (by Du Bois Reymond and Kühne), and recently M. Marey has represented it graphically by photographing the indications of a Lippmann capillary electrometer. We learn from the *Journal de Physique*, that M. De la Roche has tried the experiment on the heart of a living man. Two points of the epidermis of the chest were connected with the poles of a capillary electrometer, by means of electrodes, formed each of a bar of amalgamated zinc, with a plug of muslin at its lower end saturated with sulphate of zinc. Held with insulating handles, the bars were applied, one with its plug opposite the point of the heart, under the left nipple, and the other to another point of the chest. The mercurial column was then seen to execute a series of very distinct periodical pulsations synchronous with the pulse; each pulsation even marked the double movement of the heart (of the auricles and ventricles). The amplitude corresponded to about $\frac{1}{1000}$ Daniell.

WE have received from Perthes of Gotha a special map of Eastern Turkey, by Dr. Petermann, so full of details that for the war operations on and beyond the Danube, should the Russians succeed in crossing, we know of no better.

A RUSSIAN work, by M. Bogolubsky, on Gold and Gold Mining in Russia, is worthy of notice. It contains very interesting information upon that industry in Russia and Siberia. We observe that the area of gold mines occupies in the Russian empire about 2,100,000 square miles, and now yields yearly about 80,000 lbs. of gold, in value upwards of 3,000,000*l.* sterling. The total amount of gold produced in Russia since 1752 has been upwards of 2,500,000 lbs.

A VERY thorough and exhaustive investigation of the Alaska region may now be expected, through the agency of Mr. E. W. Nelson, a well-known naturalist, who has lately proceeded to Norton Sound, by way of Alaska, to relieve Mr. Turner. He has been provided with the necessary outfit by the Smithsonian Institution, and will probably greatly increase the amount of our knowledge of that interesting country.

WE have received from Mr. Stanford "Botanical Tables for the Use of Junior Students," by Miss Arabella B. Buckley. There are two tables—one of some common terms used in describing plants, and the other a table of the chief natural orders of British plants, arranged according to Bentham and Oliver. Both tables are well arranged, and seem to us well calculated to serve the purpose for which they are intended.

M. MEGUIN has lately been making importánt researches on Acarians, and on that strange asexual form called Hypopes, a form which is not absolutely necessary for reproduction, but which seems to occur under certain biological conditions, for the indefinite conservation of the species. In the aërial reservoirs of birds, especially Gallinaceæ, there breeds an inoffensive species, which M. Meguin calls *Kytodites glaber*, which sends colonies even into the bronchial branches, and into the marrowless bones of the limbs in communication with the air vessels in birds. Another harmless acarian is found in the cellular tissue of birds living and dying there, and persisting after death, surrounded by a calcareous tubercle. A third species, which lives normally between the barbs of the feathers, produces at the time of moulting, and in the skin of the birds, especially domestic and wild pigeons, a hypopial vermiform nymph. Without this precaution of nature, the species would be annihilated, by reason of the fall of the feathers in the moulting season.

THE additions to the Zoological Society's Gardens during the past week include a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Mrs. Cleaver; a Common Buzzard (*Buteo vulgaris*), European, presented by Mr. F. Buckland; a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Lord Lilford, F.Z.S.; three Crested Guinea Fowls (*Numida cristata*), two Vulturine Guinea Fowls (*Numida vulturina*) from East Africa, an Imperial Eagle (*Aquila imperialis*) from Turkey, deposited; four Summer Ducks (*Aix sponsa*), bred in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In a Convocation held June 5, the decree authorising the expenditure of 7,000*l.* on the construction and fittings of new chemical laboratories at the University Museum, to which we referred p. 94, was introduced by Prof. H. Smith, and carried on a division by 64 against 42.

A second proposal to grant a sum of 2,400*l.* for additions to the University Observatory was carried on a division by 46 placets to 27 non-placets.

The Trustees of the Johnson Memorial Prize for the encouragement of the study of astronomy propose the following subject for an essay:—"The History of the Successive Stages of our Knowledge of Nebulæ, Nebulous Stars, and Star-Clusters from the Time of Sir Wm. Herschel." The prize is a gold medal of the value of ten guineas, with what remains of the dividends of four years on 338*l.*, reduced annuities, after deducting cost for medals, and other expenses. The essays must be sent to the Registrar of the University on or before March 31, 1879, under the usual conditions.

CAMBRIDGE.—A curatorship in the Department of Zoology at the Museum of the University of Cambridge has just been established by the Senate, to which Mr. J. F. Bullar, B.A., of Trinity College, has been appointed. Mr. Bullar graduated in the first class of the Natural Sciences Tripos of 1875, and has been twice nominated by the University to study at the Zoological Station at Naples, where he is at present working.

The various special examinations for the Ordinary B.A. Degree were held on Friday and Saturday week, when the total number of candidates was 204, while at the corresponding period of 1876 the number was 190. Candidates can select one of the following subjects for this final examination, viz., Theology, Law, Modern History, Natural Sciences, Moral Sciences, Mechanism, and Applied Science. The number in each branch of study is as follows:—Theology, 95, Law, 31, Political Economy, 29; Modern History, 24; Natural Sciences, 21—viz., 13 in Chemistry, 5 in Botany, 2 in Zoology, 1 in Geology. In Mechanism and Applied Science there are four candidates.

Mr. William Napier Shaw, B.A., has been elected a fellow of Emmanuel College. He graduated as 16th Wrangler in the Mathematical Tripos of 1876, and obtained a first-class in the Natural Sciences Tripos, 1877, being distinguished in physics.

LONDON.—The Council of University College have elected Mr. G. D. Thane Professor of Anatomy for two years.

DORPAT.—The Annual Report of the Dorpat University for 1876, gives the number of students at the University as 815, of whom 86 study theology, 173 jurisprudence, 121 history and philology, 363 medicine, and 72 physics and mathematics. The number of professors is 67. The library of the University numbers 138,924 volumes.

RUGBY SCHOOL NATURAL HISTORY SOCIETY.—The Report of this Society for 1876, shows that it is in a "fairly healthy condition," to use the words of the preface. A considerable proportion of the papers are by members of the Society, as are also several of the illustrations. The papers are on very varied subjects and all up to a creditable standard. The preface complains that so few members take an active part in the Society's proceedings, but, in this respect, the Society is no worse than others of much greater pretension. Still it would be to the advantage of the youthful members if the patrons and office-bearers made every effort to increase the number of actual workers. We regret that our space prevents us making special reference to any of the papers. The Botanical Section has issued a list of local plants, by H. W. Trott, the result of many years' observation; this last, we daresay, may be obtained by any one desiring it. The price is only 9*d*.

LONDON SCHOOL-BOARD DISTRICTS.—Mr. Stanford is preparing for the School-Board of London a series of maps of the various School-Board districts of the metropolis, which are likely to possess considerable interest. These maps are on the scale of six inches to a mile, show the various School-Board subdivisions, the positions of the schools which have been erected by the Board, and, in a different colour, of those which are under the Board's inspection. We have seen the sheet of the Hackney district, and no better evidence could be produced of the thoroughly good work done by the Board since its institution.

SCIENTIFIC SERIALS

Memorie della Società degli Spettroscopisti Italiani, January.—Note from Prof. Draper on photographing the spectra of Venus and α Lyræ; a 28-inch reflector and a 12-inch refractor are the instruments used, and an exposure of from ten to twenty minutes. In the photograph of the spectrum of α Lyræ bands or broad lines appear in the ultra-violet region totally different to anything in the solar spectrum.

February.—Letter on the comet Borelly, 1877, Brorsen-Bruhns, 1857, and the eclipse of the moon of February 27, 1877. The spectra of the first appears, according to him, to consist of some carbon compound.—Tables of statistics of protuberances and spots observed at Rome in the months of January and February, 1877.—List of positions on the solar limb in which the vapour of magnesium was observed from February 20, 1876, to July 4 of the same year.—In the appendix to this number appears an article explaining the construction of the several different forms of aneroid barometers.

March.—List of positions on the solar limb in which the vapour of magnesium was observed from July to November, 1876, by Prof. Tacchini, and a table for the year showing the frequency of visibility of the *b*-line and 1,474-line, from which it appears that the latter line is more frequently visible than the former. Table of positions and size of protuberances observed at Rome in 1876, by Father Secchi.—Some observations of the zodiacal light, by Prof. A. Serpieri.—Note by Prof. Tacchini on Mr. Le Verrier's researches on the intra-Mercurial planet.—Drawings of chromosphere for September and October, 1875, made at Rome and Palermo.

April.—Spots and facula observed spectroscopically and directly at Palermo in 1876. This paper consists of the daily notes of observations of the chromosphere for last year.—Table of spots and faculæ observed in February and March, 1877, by Prof. Tacchini.—Drawings of the chromosphere for October, November, and December, 1875, by Secchi, Ferrari, and Tacchini, observed at Rome and Palermo.

Journal de Physique, April.—On the cause of the motion in the radiometer, by M. Gaffie.—On the capillary theory of Gauss and its extension to the capillary properties of liquid lines, by M. Lippmann.—New electric lamp, by M. Jabloschkoff.—On the quadrant electrometer of Sir W. Thomson, by M. Benoit.—Complement to the theory of the microscope and the dark chamber, by M. Neyreneuf.—Experiments of static electricity, by M. Grisson.

May.—On the observation of the infra-red part of the solar spectrum by means of the effects of phosphorescence, by M. Edm. Becquerel.—Determination of the polar distance in magnets, by M. Benoit.—Electric variation produced by contraction of the heart in the living man, by M. De la Roche.—On a new industrial application of heat, called the thermodynamic motor, by M. Ferd. Tommasi.—On the absorbent power of moist air, by M. Hoorweg.—On refrigerating mixtures of snow and sulphuric acid, by M. Pfaundler.

Morphologisches Jahrbuch, vol. iii. Part 1.—Oscar Hertwig, contributions on the formation, fertilisation, and cleavage of the animal ovum, part second (*Hæmopis*, *Nephelis*, *Rana temporaria*, and *R. esculenta*), 86 pages, 5 plates.—A. Rauber, the fixation of long bones in joints, and the form of the bones.—W. Moldenhauer, the development of the middle and outer ear, 56 pages, 4 plates.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. x. Fasc. vii.—Two new mycetes parasitic on vines, by M. Cattaneo.—On a cause little estimated in the pathogenesis of some female diseases, by M. de Giovanni.—The molecular velocity of gas and the corresponding velocity of sound, by M. Brusotti.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, June 7.—Dr. Gladstone in the chair.—The following papers were read:—On the gases inclosed in lignite coal and mineral resin from Bovey Heathfield, by J. W. Thomas. Four samples were examined, two of which contained much hydrated oxide of iron in the cleavages. The gases consisted chiefly of carbonic acid, carbonic oxide, nitrogen, and sulphuretted hydrogen. In one case sulphur sublimed off in yellow crystals; organo-sulphur compounds, mercaptan, sulphide of allyl, &c., were also present in the gases. The lignites resemble cannel coal more than any other of the true coals as regards the occluded gases, but are far less stable, decomposing, *in vacuo*, below 200° C., whilst the true coals resist a temperature of 300° C. It seems probable that the iron pyrites of true coal have derived their sulphur from that existing in organic combination in the plants from which coal is produced.—On apparatus for gas analysis, by Dr. Frankland. The author proposes to substitute for the india-rubber cork, which has several disadvantages, at the bottom of the water-cylinder, a cast-iron base through which the two glass tubes pass, and are firmly clamped by a wooden clamp; the latter is screwed to the cast-iron base. The most important improvement is, however, the removal of the steel clamps which connect the laboratory and measuring tubes. These are replaced by a glass cup at the top of the measuring tube into which fits the drawn-out end of the laboratory tube, covered with thin sheet-india-rubber; this flexible joint, when wetted and covered with mercury, is quite air-tight.—On narcotine, cotarnine, and hydrocotarnine, Part V., by Dr. Wright. The preparation of bromhydrocotarnine hydrobromide, bromocotarnine hydrobromide, and tribromhydrocotarnine hydrobromide is described; the second of these bodies, when heated to 200° splits into a new base, tarconine, and a large amount of an indigo-blue substance; the latter body is very insoluble, but dissolves in strong sulphuric acid, forming a magnificent intense purplish solution. Bromocotarnin crystallises in fine scarlet crystals. Noropianic acid and other substances were also prepared and their properties examined.—On otto of limes, by C. H. Piesse and Dr. Wright. A terpene-like body boiling at 176° C. was obtained which yielded but little cymene. The residue in the retort, after standing two to three months, formed a quantity of crystals. These crystals were investigated and their composition determined.—On primary normal heptyl alcohol and some of its derivatives, by C. F. Cross. Pure œnanthol was prepared with a specific gravity of 0·823 at 16° C. Pure heptyl alcohol is colourless, has an agreeable odour, sp. gr. at 0° 0·833, boils at 175°. Heptyl chloride, bromide, iodide, acetate, and œnanthylate were prepared and examined; their boiling-points closely agree with those calculated by Schorlemmer.—On the transformation of aurin into rosanilin, by Messrs. Dale and Schorlemmer. The authors find the spectra of the hydrochlorides of their new base, and rosaniline quite identical; they have also prepared from their base Hofmann's violet, aniline blue, and aniline green.

Geological Society, May 23.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Richard George Coke, Robert Slater, and William Swanston were elected fellows of the Society.—The president read a letter from Mr. C. J. Lambert, announcing that he had allotted the sum of 500*l.* to the Geological Society out of the 25,000*l.* left by his father for distribution. The president further announced that the sum of 500*l.* had already been paid to the Society, and would be invested for its benefit.—The following communications were read;—Remarks on the coal-bearing deposits near Erekli, the ancient Heraclea, Pontus Bithynia, by Rear-Admiral T. A. B. Spratt, C.B., F.R.S.

—On the structure and affinities of the genus *Siphonia*, by W. J. Sollas, F.G.S. This paper contained, first, a full account of the history of the genus *Siphonia*, including a complete list of its described species, and, next, a description of its general and minute structure. Its skeletal network was shown to consist of spicular elements belonging to the Lithistid type of sponges, and most closely allied in generic details to the recent form *Discodermia polydiscus*. Not only in this character but in every other, *Siphonia* was shown to approach *Discodermia* so closely as to be almost identical with it. The mineral replacements which have affected the siliceous skeleton of *Siphonia* were then considered. The paper concluded with a systematic description of the genus.—On the serpentine and associated rocks of the Lizard district, by Rev. T. G. Bonney, F.G.S., fellow and late tutor of St. John's College, Cambridge. The author stated that considerable doubt appeared still to exist as to the true relations of the lizard serpentine and the associated hornblende schists, and as to the origin of the serpentine. He had carefully examined all the junctions accessible on the Cornish coast (inland they are generally obscured). Some of them are concealed by *débris*, &c., but the majority prove beyond doubt that the serpentine is intrusive. Further, almost everywhere large fragments of hornblende schist are caught up and included in the serpentine. Besides the serpentine there is a large mass of gabbro at Crousa Down, and many dykes and veins along the east coast almost to the extremity of the serpentine region. At Coverack Cove, near the above mass, are gabbros of two ages, the older much resembling a kind of troktolite. On microscopic examination it proves to be chiefly plagioclase felspar, augitic minerals (including diallage), and olivine partially converted into serpentine. There is a red and a green variety. The newer, a coarser variety, appears to be of the same age as the other veins on the coast, and connected with the main mass. Some remarkable changes have taken place in this also. In certain places it exhibits a separation of its mineral constituents, causing it to resemble a foliated rock. This is proved to be due to pressure at right angles to the structure. The minerals also are often changed. The felspar is replaced by a white granular mineral resembling saussurite; the diallage (which occurs sometimes in very large crystals) is often partially, or even wholly, converted into rather minute crystalline hornblende. In these specimens there is no olivine to be distinguished. The great mass, however, is rich in olivine, yet a weathered specimen from it, resembling in aspect the gabbro of the veins, does not show olivine. Hence the author believes that in certain cases the olivine, instead of being converted into serpentine, aids in forming the hornblende. Further, there are dykes and veins over the same area of a dark trap. Some of these are augitic, others hornblendic. The author believes that at any rate in certain of these the hornblende is of secondary formation. On the west coast are veins of granite; those on the east coast, said to be granite, prove, on careful examination, to be altered rock, remarkably like granite veins, but not really such. In discussing the origin of the serpentine the author called attention to a structure commonly seen, which appeared to be a true "fluidal structure." He then described the result of microscopic examination of many specimens of the lizard and some other serpentines. Commencing with slightly altered lherzolite (from the Ariége), he traced the change through the older gabbro of Coverack to the serpentine rock of that place, which contains a large quantity of unaltered olivine; and so to other serpentines in which the olivine is quite replaced by the mineral serpentine. He described also the mode of the change. The other minerals found in the serpentine rock are enstatite, varieties of augite, and occasionally a fair quantity of picotite, with, of course, oxides of iron. Hence he concluded that, as had been already shown as regards some other serpentines, that of the lizard was the result of the hydrous alteration of an olivine rock, such as lherzolite.—On certain ancient devitrified pitchstones and perlites from the Lower Silurian district of Shropshire, by S. Allport, F.G.S.

Physical Society, June 9.—Prof. G. C. Foster, president, in the chair.—The following candidates were elected members of the Society:—Mr. W. H. Northcott and Mr. L. J. Whalley.—Mr. S. P. Thompson read a paper on interference fringes within the Nicol prism. After referring to the original paper by the inventor in 1828, in which this phenomenon was referred to, he gave a general description of it prior to explaining the cause. If the "field" of a Nicol be explored by the eye it will be seen to be bordered on one side by a margin of violet-blue light, and on the other, when the light passes obliquely through the prism, by an orange band within which lie a series of coloured fringes; these latter are very clearly seen with monochromatic light, when a second set, within the blue band, also appears. The author showed that these two sets are due to interference taking place within the film of balsam at the critical angle of total reflexion for ordinary and extraordinary rays respectively; they are therefore analogous to the interference bands in a thin film, placed beneath a prism of a more highly refracting substance and occurring just within the limit of total internal reflection, as first observed by Sir W. Herschel. At the conclusion of the scientific business of the Society, a special general meeting was held.

Royal Microscopical Society, June 6.—Dr. Robt. Braithwaite, vice-president, in the chair.—Six new fellows were elected, and M. L'Abbé Renard was elected an honorary fellow of the Society.—A paper by the Rev. J. Delsaulx on the thermodynamic origin of the Brownian motion was read by the secretary, and described the observations of the author with regard to the motion of fluid in rock cavities and molecular motion generally, with a view to establish the theory that it was due to the action of temperature. The observations had been suggested by the study of Crookes's radiometer.—A letter from Mr. H. C. Sorby on the subject was also read to the meeting, and Mr. Hartley described his experiments which led to the same conclusions. The meeting was then adjourned until October.

EDINBURGH

Royal Society, June 4.—Prof. Kelland in the chair.—Sir C. Wyville Thomson read a paper on the structure and relations of the genus Holypus.—Mr. Alexander Buchan, M.A., secretary to the Scottish Meteorological Society, communicated the second part of his investigations of the diurnal oscillations of the barometer. He stated that the summer months of the northern hemisphere as indicated by the barometer were May, June, and July, the winter months being November, December, and January, both corresponding with the sun's declination. He has now results of the daily barometric readings from upwards of 110 stations at different parts of the earth's surface. His investigations showed that a long-continued series of observations was absolutely necessary to show the peculiarities of the barometric curve. For instance, three years' observations gave in the case of Great Britain only the broadest characteristics. He found that no theory as yet propounded would explain the diurnal oscillations of the barometer, and that as more facts were obtained the difficulty of framing a satisfactory theory was greatly increased.—In his paper on the air dissolved in sea-water, Mr. J. Y. Buchanan stated that the result of the analysis he has as yet made of the specimens of the air dissolved in sea-water which were collected in the recent *Challenger* expedition, tends to show that as regards surface-water least air was dissolved where the temperature was highest, *e.g.*, near the equator, and most where the temperature was least, as in the polar sea. As regards the percentage of oxygen present at different depths it diminishes from the surface to a depth of 300 fathoms and increases from that point to lower depths. Prof. Tait communicated two laboratory notes; (1) Two plates either of the same or different metals were placed very close to one another but insulated and one of them raised in temperature: a difference of potential was produced, which was capable of producing a current measurable by a sensitive galvanometer. (2) He had seen in Dr. Blair's "Scientific Aphorisms" a hypothesis to account for gravitation very like that of Lesage's ultramundane corpuscles, which Blair stated was suggested to him by Newton's works, and Prof. Tait was anxious to ascertain if any part of it was due to Lesage or was entirely original. Prof. Tait laid on the table an algebraic identity which could be used to sum various series.

DUBLIN

Royal Society, May 21.—Prof. J. Emerson Reynolds, M.D., in the chair.—The following papers were read:—On some measurements of the polarisation of light coming from the moon and from the planet Venus, by Earl Rosse, F.R.S. Lord Rosse gave the results at which he had already arrived from a very large number of observations on the polarisation of light from particular parts of the moon's surface made in the years 1872, 1873, 1874, and 1875, and which are still in progress. The observations indicate that the polarisation of the light coming from the plains is greater than that of the light coming from the mountainous regions.—Notes on the crustacea of Ireland, by Mr. William Andrews. An account of the rarer species found on the Irish coasts—On the substitution of an alkaline base in chlorimetry, by Mr. J. Smyth, F.C.S. The author treats bleaching lime with an alkaline carbonate, and thus obtains the chlorine in a more convenient form for tetration.—On a specimen of quartz with a pearly lustre, by Mr. R. J. Moss, F.C.S. The faces of the pyramidal crystals, of which the specimens consist, possess a laminated structure, which causes a very perfect reflection of light. The laminæ consist of quartz exclusively. Mr. Moss concludes that this novel variety of quartz must be regarded as a modification of cup-quartz, the laminated structure being the result of periodic interruptions in the growth of the crystals.—An account of recent attempts to obtain water by deep wells under London, by Prof. Hull, F.R.S. The author referred principally to the scientific results of the recent boring at Messrs. Meux's brewery.—On a remarkable action of light on certain organo-metallic bodies, by Prof. J. Emerson Reynolds, M.D. The author described a number of experiments with mercuric ethide and its homologues, and showed that the pure ethide when sealed up in a tube and exposed to light for some months is wholly decomposed into mercury and nearly pure liquid hydrocarbon.—On the penetration of heat across Crookes's layer, by Mr. G. Johnstone Stoney, F.R.S. The author described the way in which heat is transferred across the vacuous spaces in Crookes's radiometers. He traced the laws under which this transfer of heat takes place, and showed that they are different from the already-known laws of radiation, convection, conduction, and contact. Mr. Stoney suggests that the newly-discovered mode of conveying heat should be called penetration. He showed that a large body of observations were made more than thirty years ago by De La Provostaye and De Sains upon heat conveyed in this way, but without its theory being understood. Unexplained observations made by Dulong and Petit, Grove, and other physicists also admitted of interpretation by the newly-discovered laws. Mr. Geo. M. Fitzgerald was the first to observe that the important results obtained by De La Provostaye and De Sains were due to this cause, and it was by him that they were brought under Mr. Stoney's notice.

Paris

Academy of Sciences, June 4.—M. Peligot in the chair.—The following papers were read:—Preliminary reply to observations by M. Mouchez on the "Nouvelle Navigation," by M. Villarceau.—On the densities of vapour, by M. H. Sainte Claire Deville.—Researches on the law of Avogadro, by M. Wurtz.—On the atomic notation; reply to M. Berthelot by M. Wurtz.—Atoms and equivalents, reply to M. Wurtz, by M. Berthelot.—Reply to M. Fizeau, by M. Berthelot.—On the parallel striæ frequently presented by the surface of fragments of diamonds of the carbonado variety, and on their imitation by means of artificial friction, by M. Daubrée. These striæ seem to show that fragments, now sparsely scattered, must have been at one time, before they were thrust up to the surface, in contact with one another and exerting mutual pressure.—On the regeneration of the red blood corpuscles in frogs after considerable hæmorrhage, by M. Vulpian. The corpuscles result from evolution of small colourless spheroidal nucleated cells, which become discoid, then oval, and a little before they reach the size of red corpusles, they produce hæmoglobin.—M. Gervais announced the receipt of a fresh batch of natural history specimens (chiefly birds and insects) from the Japanese Government. He remarked on some skeletons of Cetaceans.—On the true number of elementary co-variants of a system of two binary biquadratic forms, by Mr. Sylvester.—On the spectrum of Winnecke's comet, by P. Secchi. His experiments lead him to think it similar to that of other comets, and to have for base carbonic oxide.—On some metallic seleniurets and tellurets, by M. Margottet.—Study on oxides of iron, by M. Moissan. He believes the pyrophorus of magnum is formed in great part of pyrophoric protoxide of iron.—On the preparation and composition of emetine, by MM. Lefort and Wurtz. Emetine does not form basic salts, at least in the conditions in which it is ordinarily obtained.—Reproduction of albite, by M. Hautefeuille. Albite may be easily had by heating to a dark red a mixture of tungstic acid and a very alkaline silico-aluminate of soda.—Strontian; its diffusion in mineral and organic nature at the present epoch and in the series of geological times; consequences relative to saliferous mineral waters, by M. Dienlafait. *Inter alia*, of eight hundred springs coming under the designation of saliferous waters, there are only forty-four in which strontian has not been found.—On the affinity of blood corpuscles for carbonic acid, by MM. Mathieu and Urbain. A reply to M. Fredericq, who holds the CO_2 is expanded in the plasma, not fixed by the globules. The authors cite figures to show that the globules of horse blood can fix about twice as much CO_2 as the serum. Some substances (as ether) increase the absorbing power for O and CO_2; others (as alum) diminish it.—Experimental studies on regeneration of cartilaginous and osseous tissues, by M. Peyrand.—Historical remarks on the theory of movement of one or several bodies, of constant or variable forms, in an incompressible fluid (continued), by M. Bjerknes.—Comparative study of experiments by day and by night made by MM. Perrier and Bassot, by M. Perrier. The mean errors of an isolated observation for the day and the night are of the same order of magnitude. The errors arising from the atmosphere are compensated better in night than in day observations.—Siphon barometers whose indications are not influenced by variations of temperature, by M. Gautier.—Observations on a note of M. Yvon on nitrates of bismuth, by M. Ditte.—Combinations of quercite with butyric and acetic acids, by M. Prunier.—Investigation of salicylic acid in wines and urine, by M. Robinet.—Observations on M. Bert's experiments on anthracic disease, by M. Davaine.—Experiments showing that there is not in toxical putrefied blood, liquid or solid virus without organic ferments, by M. Feltz.—On various specimens of Brazilian clay and coal, by M. Guignet.—Female flowers of cordaites, by M. Renault.—Result of geological explorations in 1875–76 in connection with the channel railway, by MM. Potier and De Lapparent.—Researches on the vitality of spermatozoids of trout, by M. Heneguy. They resist the action of alcohol and anæsthetics in such quantity as would kill *e.g.* infusoria.—Determinations of ammonia in the air and the meteoric water of Montsouris, by M. Levy.

Geneva

Physical and Natural History Society, April 5.—Prof. Forel gave an account of his observations on the transparency of the waters of the Lake of Geneva. This transparency, much greater in winter than in summer, is modified very suddenly at the two periods of its annual variation. The changes of temperature are insufficient to account for the phenomenon. On filtering the water of the lake, M. Forel found in suspension fine particles almost exclusively organic, and proved that their proportion increases with the number of strata at different densities.—M. Théod. Turrettini described the discovery which he had made of a freezing mixture of chloride of magnesium and snow, which gave a temperature of − 34° C.—Prof. Wartmann announced a series of researches undertaken by him which enabled him to understand the extra-polar-derivation of currents traversing mercurial conductors.—M. Duby spoke of the capture of certain insects by the *Petunia* and the part played by the sticky glands in this respect.

Rome

R. Accademia dei Lincei, April 8.—Influence of nicotine on the animal organism, by M. Corso. The increase of blood pressure may be obtained without preserving intact the vasomotor centre. It is not due to reflex action. The first effect of nicotine on the pupil is dilatation.—The president (M. Sella) read a necrological notice on Prof. Panceri.—Considerations on specific heat, by M. Cerruti.—The Roman Tuscia and the Tolfa (continued), by M. Ponza.—A memoir by M. Bagnis was presented, containing illustrations of a first hundred cryptogams from the Agro Romano, studied by him.—M. Volpicelli replied to M. Cantoni's recent objections to the electrostatic doctrine of Melloni.

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THURSDAY, JUNE 21, 1877

THE ORGANISATION OF NATURAL HISTORY MUSEUMS

THE two questions connected with museum reform more especially demanding attention relate to their resources and their organisation.

Provincial museums are very generally either the property of societies the funds of which are expended mainly in publishing memoirs, or are carried on in connection with free libraries. With regard to the first, it is impossible that adequate means should be forthcoming for their efficient maintenance, and with regard to the second the library, as a rule, swallows up an inordinate share of the funds provided by a rate. Some are connected with teaching bodies. In nearly all cases they are poverty-stricken and largely dependent on casual benevolence for their support and increase. It is surely only just and fair that the funds necessary for making them efficient should be provided out of the public purse since they are for the public weal, as is the case in all other countries except our own. I do not advocate that the whole burden of this should fall on the public; it should certainly be borne in part by private individuals, who, if experience in other things be worth anything, are ready to subscribe liberally when a clear case has been made out that their liberality will be of practical use. The relation which the museum of natural history at Lyons bears to the Société des Amis des Sciences Naturelles illustrates my meaning, for while supported by the Municipality, it is largely indebted to the Society for the purchase of new and rare specimens. Why should not similar associations for similar ends be formed in connection with every important museum in this country? A well-arranged museum of any size is of necessity costly, and to be well officered must offer inducements other than those which tempt the badly-educated, the mere enthusiast, or the worsted in the struggle for life.

The abnormal connection between museums and libraries should be severed, as it is to be severed in the case of the national collections, because they have no real connection with one another. The officer who has the care of both, if they be of any size, cannot in the nature of things perform satisfactorily what he undertakes. If he give up his mind to the library he will probably neglect the natural history, or *vice versâ*. To this cause the chaotic state of many of our museums is to be assigned. A curator has not merely to catalogue and arrange, but he has to master thoroughly the collections under his charge, and in this respect he differs essentially from a librarian. A museum is one thing, and a library is quite another thing.

The Public Libraries Act of 1855 has certainly failed so far as relates to the establishment of museums, if the rapid development of other means of advancing knowledge during the last twenty-two years be taken into account.

On the very difficult question of museum buildings, it seems to me that the maximum amount of space and of light obtainable at the lowest cost consistent with good work should be the chief end, rather than a building with a fine outside. As a rule we content ourselves with mere externals. Well-supported collections in a big barn or an old cotton-mill are more likely to be useful than a grand building on which the greater part of the funds are likely to be spent to the starving of the museum within. This fact is fully recognised in the United States. In our wealthy centres of population there is no reason why we should not have large collections adequately housed, as is the case very generally on the Continent.

The best internal organisation of a museum which occurs to me is that which preserves as far as possible the continuity that exists both in nature and art, and in which the departments are arranged in the following order:—

1. Mineralogy and crystallography and petrology, including mineral products used in the arts. This should be approached from the point of view offered by chemistry.

2. Palæontology, illustrating the ancient history of life on the earth from the Eozoon to the beginning of the historical period, and consisting of fossils arranged stratigraphically, and divided as far as possible into three divisions within the limits of each group—the fauna and flora of the sea, of the fresh water, and of the land. Under this head prehistoric archæology is included.

3. Botany.

4. Zoology.—In this department it is wholly unnecessary to have every species mounted. The birds may very conveniently be represented by one stuffed specimen of a class, family, or genus, while the species are preserved for reference, and the skins occupy a small space in drawers, as in Dr. Sclater's collection at the Zoological Society.

4A.—Comparative osteology and anatomy are the natural offshoots of zoology, ranking under that head. I cannot, however, agree with those who hold that they should form part of the zoological series, since their chief value consists in their comparison with the like parts of other creatures. Were the collections of the Royal College of Surgeons arranged in the same cases with the zoology of the British Museum—dissections and bones of lions say with stuffed lions—they would at once lose their teaching value.

5. Ethnology, worked out as it is now being done by Mr. Franks in the British Museum, and Dr. Bastian in the Imperial Museum at Berlin.

6. Art.

This scheme is applicable to museums large and small.

In founding provincial museums the fauna, flora, and geognosy of the surrounding districts should receive the first attention, and to these may be added, if possible, a general collection. In all cases they should be connected with teaching. The principle of co-operation should be freely brought into play, and casts of the more important objects in each should be made for exchange and distribution. Each museum should be available for the general public, the local societies, and the teaching bodies of the place, schools, colleges, and the like. Were we to expend as much common sense and energy in this direction as in most others we should soon see our museums important instruments in spreading culture, and in a very different position to that which they now hold in relation to those of other countries, in many ways worse off than our own. The subject is well worth the attention of all who have at heart the higher education of the people.

W. Boyd Dawkins

THE CARBONIFEROUS FLORA OF CENTRAL FRANCE

Flore Carbonifère du Département de la Loire et du Centre de la France. Par Cyrille Grand' Eury, Ingénieur à St. Étienne. (Imprimerie Nationale, Paris.)

THIS work consists of three quarto volumes, the first of which is devoted to the plants, the second to the geology of the districts under consideration, and the third forms an atlas with thirty-four plates of fossil plants and four large "tableaux," in which the author has "restored" the plants he has described according to his own ideas of their morphology.

It is very obvious that the carboniferous plants of one district cannot be received as altogether typical of those occurring at other and distant localities. Hence such publications as those of Dr. Dawson and Prof. Newbury in America, and the volumes of M. Grand' Eury, are extremely valuable to the English palæo-botanists. They tend to preserve him from the one-sided habit of viewing the subject which he is apt to contract when only studying the types occurring in his own coal-fields. But apart from this M. Grand' Eury's work has an independent value, especially in some departments in which he has made important additions to our stock of knowledge. This is especially the case with his investigations amongst the hitherto obscure plants known as Flabellariæ and Cordaites, as well as amongst some remarkable sporange-bearing ferns.

Our knowledge of Cordaites has hitherto been most vague; but M. Grand' Eury has fortunately obtained some beautiful specimens in which not only the leaves are attached to the stems of several species, but in some he also finds what he believes, I think justly, to be male and female organs of reproduction, thus establishing the point that these plants were monœcious Phanerogams. These organs are slender spikes, some of which support small scaly buds lodged in the axils of bracts, and which the author believes to have been antheriferous. Others bear single seeds in each axil. Some of the spikes are affirmed to be those of Antholithes, and the seeds to be identical with Cardiocarpus. The stems which bear these reproductive structures have a Sternbergian pith, surrounded by an exogenous woody zone inclosed within a distinct bark, which latter appears to have consisted of more than one layer. M. Grand' Eury concludes that these plants were Conifers, of which the well-known Dadoxylons were the ligneous axes, and that the type which survived for a time in some of the Ulmanniæ of the Lechstein, and in the Albertia of the Triassic rocks. I see nothing, however, in his figures and descriptions leading me to conclude that they are identical with our British Dadoxylous.

The new ferns described by the author are equally remarkable. They include numerous forms of Pecopteris, with very peculiar sori approaching those of the Marattiaceæ. Some of these fronds he associates unhesitatingly with Psaronius and other stems of tree-ferns. The author's researches on the above subjects have been conducted under most favourable conditions, of which he has availed himself in a praiseworthy manner.

When we come to the debateable subjects of Calamites, Lepidodendron, Sigillariæ, and Asterophyllites. I am obliged to use different language. On these points the author adopts substantially the ideas of Brongniart. Thus he distinguishes between Calamites and Calamodendron, making the former an equisetaceous plant and the latter a gymnospermous one. I cannot understand how any one can do this in the face of our present knowledge of the facts.

In external form the supposed Calamites and Calamodendra exhibit precisely the same appearances. All these appearances are explained in the most exact manner by the internal structure of the many illustrative specimens which we now possess, and which demonstrate that we only have one type of organisation. Further, what are called Calamites by the school to which our author belongs are amongst the most abundant of the plants furnished by our coal-shales, and there is nothing to prevent their being equally common in the Oldham and other beds, in which all the plants retain their internal structure, if they existed as an independent type. But the moment we find a Calamitean plant with organisation it proves to be a Calamodendron. Even M. Grand' Eury is compelled to admit "il est au moins surprenant que l'on n'ait pas mis la main sur un Calamite avec la structure conservée." Very surprising, indeed, considering that we have obtained such numbers of these plants with structure from Oldham, Halifax, and Autun, as well as, though less abundantly, from Burntisland. The conclusion to be drawn is too obvious to need reiteration.

Imbued with these ideas respecting Calamites and Calamodendra, it was inevitable that M. Grand' Eury should fall into error respecting Asterophyllites. These plants are regarded by his school as the branches and leaves of Calamites. Hence he could not recognise as Asterophyllites any plant which had not a Calamitean axis. But I have shown that Asterophyllites has *not* such a structure, but one identical with the very different one of Sphenophyllum. M. Grand' Eury escapes the difficulty by contending that my plants are *not* Asterophyllites, but Sphenophylla. This is certainly not the case. Brongniart has clearly defined the latter genus a possessing 6–8 or 10 *truncate cuneiform* leaves; and after referring to the fructification of Sphenophyllum, he correctly says:—"Ce mode de fructification, malgré l'obscurité qui environne encore sa vraie structure, est trop analogue à celui des Astérophyllites pour qu'on puisse douter de l'affinité de ces deux genres." This conclusion is precisely identical with mine. Instead of 6–8 or 10 leaves in each verticil, my plants have 18 or 20. These leaves are linear, not cuneiform; and as my next memoir will demonstrate even more clearly than I have yet done, each leaf had a single central vascular bundle instead of the two or more invariably seen in Sphenophyllum.

In his views respecting the relations of Sigillaria and Lepidodendron, M. Grand' Eury also clings to the old Brongniartian ideas promulgated in bygone years. M. Brongniart and M. Renault have described the organisation of two Sigillarian fragments, *S. elegans* and *S.*

spinulosa, and have concluded that whilst their Diploxyloid organisation differs from that of the Lepidodendra, it justifies the conclusion that Sigillariæ were not lycopodiaceous but gymnospermous plants. But I have already shown that several indisputable Lepidodendra have precisely the same organisation. Hence I contend that Brongniart's reasons for separating these plants have no existence, and consequently his conclusions must be abandoned; M. Grand' Eury, forgetting this part of my work, and only remembering that I have also described the bark of a true Syringodendroid Sigillaria, and shown that it is identical in every feature with the corresponding tissue in Lepidodendra, says that I have arrived at my conclusion "par des faits isolés d'après l'analogue de l'écorce, et non par des exemples complets réunissant les caractères extérieurs aux caractères intérieurs." How, in the face of my published memoirs, my friend could make so erroneous a statement, I am at a loss to conceive.

I should have felt it necessary to have subjected the volumes under consideration to an incisive criticism on these and some similar points, were it not that he kindly allows me to quote from some letters which I have received from him. In these communications he says:—"Les points sur lesquels nous différons sont précisément ceux que je n'ai pas étudiés." Referring to facts which I have observed, he adds: "Comme je n'avais pas ces faits pour me guider, j'ai conclu d'après ce que je connaissais bien, et je ne suis pas fain de conclure que je me suis trompé; dans ce cas nous aurions dans les Sigillaires et les Lepidodendrons des cryptogames excessivement elevées en organisation; si elevées qu'ils formaient, en quelque façon, une classe intermédiare entre ces plantes et les Gymnospermes." "L'Association presque constante des macrospores avec le débris des Sigillaires est en faveur de vos conclusions." Quite in accordance with the above remarks are the following observations which the author makes in his volumes: "Il est au moins curieux que, à part le corps vasculaire, les autres parties des Sigillaires soient semblables aux parties correspondantes des Lepidodendrons." This is perfectly true with the exception that the vascular portions are less exceptional than M. Grand' Eury's remarks imply. The Sigillarian stem is merely that of Lepidodendron Harcourtii, *with an additional* exogenous vascular zone interposed between that of the Lepidodendron and its investing cortex; *and which I find in other true Lepidodendra.* If all the plants of the coal-measures which possess a similar exogenous zone are to be transferred from the cryptogamic to the phanerogamic group, there will be few cryptogams left in the carboniferous rocks beyond Lepidodendron Harcourtii and the ferns. M. Grand' Eury concludes his notice of the Sigillariæ by a remark which I fully endorse: "Seulement je crains d'avoir tiré des conséquences trop complètes de ces indices insuffisants, dont je n'aurais peut-être alors même dû parler que pour éveiller l'attention des observateurs sur une solution possible du plus important problème de la paléontologie végétale."

Having thus indicated some very important points respecting which I am compelled to differ from M. Grand' Eury, I can with the sincerest truth again express my sense of the value of this new contribution to the study of the carboniferous flora, and of the praiseworthy perseverance with which the author has laboured for many years in collecting his materials. The most prominent fact which the work reveals is the remarkable abundance of the Cordaites in the coal-measures of Central France, compared with what we see in England. In some districts, as M. Grand' Eury informs us, the coal is almost entirely composed of their *débris*. I have met with nothing like this in Great Britain, but it is in strict accordance with what we know of the distribution of living plants, that whilst similar types may be expected to be met with over wide geographical areas, some forms will predominate in one region, whilst in other localities different types will prevail; hence the materials out of which coal has been found must have been widely different at these various spots.

The plates with which the above work is illustrated are extremely beautiful, as is usually the case with the productions of the French lithographers.

W. C. WILLIAMSON

OUR BOOK SHELF

Annæs do Observatorio do Infante D. Luiz. Magnetismo Terrestre. Lisboa, 1876.

THIS part of the *Annals* of the Lisbon Observatory is a continuation of those noticed in NATURE, vol. xiii. p. 301. The results for the magnetic declination are carried forward from 1867 to 1871, while some include the means from 1858 to 1875. This is the case for the secular change and annual variation. Mr. Capello found previously that the north end of the declination magnet approached the north at the rate of 5′·91 yearly (1858-1868). The results he now divides into two series, 1858 to 1866, with a rate of 5′·46, and 1866 to 1875, with a rate of 7′·64 yearly.

The yearly means are deduced from observations at 8 A.M. and 2 P.M. Mr. Capello has also shown that the diurnal law of disturbance appears to be different at Lisbon in different years of the decennial period.[1] In this case, even if two observations daily were otherwise sufficient to give accurate means, or means strictly comparable from year to year, the varying effect of the disturbance on the observations at the two hours mentioned would of itself interfere with this comparability. It is probably for these reasons that the yearly means at Lisbon do not appear to show the small decennial inequality in the secular movement first indicated by me in 1857, and afterwards discovered by Hansteen and Lloyd.

Mr. Capello has repeated discussions for the magnetic disturbances with the increased materials in his possession. He had observed in a preceding number of the *Annals*, that many observations which were considered disturbed (that is to say, which differed from the means for the hours by 2′·26 or more) really belonged to diurnal variations which were regular, only larger than usual; and it was pointed out in NATURE (in the notice cited above) that one cause of these excessive deviations would be found in the superposed lunar actions. Mr. Capello now finds that a great majority of these quasi-solar disturbances are rather to be considered due to the moon. This conclusion induces me to believe that if Mr. Capello had the necessary aid to perform the calculations for the lunar diurnal variations for each month, and for different positions of the moon, as well as for other investigations, the Lisbon observations could not fail to add many important scientific results to those already published.

JOHN ALLAN BROUN

Incidents in the Biography of Dust. By H. P. Malet. (London: Trübner and Co.)

THE first impression one gets of this book is that of a kind of nightmare. It begins by personifying dust and makes "us dusts" utter a great deal of incoherent talk which changes somehow into the voice of the writer himself, who by and by fades into Prof. Tyndall, then into "a weekly paper, *Punch*," then through Hugh Miller and the Holy Scriptures into the familiar tones of Mr. Henry Woodward, F.R.S., who gives way to the dusts again, and so on. The first impression, too, deepens upon further perusal. One never can be quite sure who is speaking; whether the "we" is the editorial pronoun or marks the utterances of the personified dust-motes. Sometimes, indeed, by a kind of feeble and perhaps, unconscious pun, it means both the author and "us dusts;" as where a sentence begins (p. 107), "Of all the authorities we have ever rested on, Sir Charles Lyell has described mountain formation most accurately." Or again: "Mrs. Somerville is a favourite authoress; we seldom find a protracted rest upon her volumes." The writer seems to have made a very hearty meal on all kinds of miscellaneous geological and other scientific and literary food. The variety and amount of the viands have been too much for him. Hence the wild speculations, the grotesque theories, the pell-mell rush of changing subject through the 272 pages of this curious but dreary volume. So completely has the nightmare taken possession of the author that in his frenzy he forgets the composition of the very air he breathes, and sententiously announces that while "the earth consists of air, water, and dust," the "air is composed chiefly of oxygen, hydrogen, and carbonic-acid gases." We would venture to suggest a good application of oxygen and hydrogen in the form of a shower-bath as a corrective. The book closes most appropriately with a spiritualistic *séance*, at which the *dramatis personæ* are a Medium, Spirit of Socrates, and Dust. If the author would discard all this "plain language," as he is facetiously pleased to call it, and tell us in simple straightforward English what it is all about, we should be prepared calmly to listen to him, but no more such "Biographies of Dust!"

Chemical Physics. By N. N. Lubavin. First fascicule. St. Petersburg, 1876, 346 pp., in 8vo. (Russian.)

THE author has given in a handbook a description of the various physical phenomena which, without belonging to the true domain of chemistry, are nevertheless involved in all chemical processes, and which can adequately be described as physico-chemical. These phenomena, of the highest importance for the student of chemistry who is interested in the philosophy of his science, are dealt with at length by the author in a very lucid and plain style. Without discussing advanced theories, M. Lubavin, in this first fascicule (the second being in the press) gives us only facts, and in a condensed form much useful information. He has carefully read what has been published in this department in France and Germany, but is not very familiar with our English works, except through German or French translations.

Enumeracion de los Vertebrados Fósiles de España. Por Don Salvador Calderon. (Madrid: T. Fortanet, 1877.)

THIS is a reprint from the *Anal. de la Soc. Españ. de Hist. Nat.*, tom. v., of Señor Calderon's valuable catalogue of the vertebrate fossils hitherto discovered in Spain, with an introduction and accompanying remarks. As the catalogue and an abstract of the introduction to it have been published in the *Quarterly Journal* of the Geological Society of London during the present year, it will not be necessary for us to do more than to call attention to the appearance of the work in its more complete form. Some interesting questions are opened up by the author concerning the distribution of several interesting Miocene forms such as Sivatherium, Hyænarctos, and Hipparion.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Museum Reform

EVERY one who puts faith in museums as educational engines must be grateful to Prof. Boyd Dawkins for the article on this subject in the number of NATURE for May 31. That reform is pressingly needed in most of our provincial museums is a proposition almost beyond question; but how such reform can be best effected is a subject open to any amount of discussion. The primary difficulty in organising a museum is usually a difficulty of finance. Money, which measures all things, measures the curator's power of procuring glass cases and suitable specimens. Where, then, the resources of a museum are very limited, the greatest amount of good will probably be effected by confining attention to the formation of local collections. Such work, being restricted within a narrow sphere, may be done thoroughly, even in the poorest museum. Yet it is work which will be valued by every true student of science. Prof. Blackie, in his "Self Culture," gives excellent advice when he says: "In order to assist in forming habits of observation in this age of locomotion I should advise young men never to omit visiting the local museums of any district, as often as they may have an opportunity; and when there to confine their attention generally to that one thing which is the most characteristic of the locality." Now it often happens that the things most characteristic of the locality are hardly thought worth exhibiting, and are precisely the things that we do not find in a provincial museum. Only last week I had occasion to visit a museum of thoroughly old-fashioned type, and to my surprise I found that the mineral industries of the neighbourhood, though of great importance, were absolutely unrepresented, whilst unlabelled curiosities collected from every quarter of the globe were heaped together in defiance of all principles of classification. It is true there is great temptation for a curator to display a little of everything, and a specimen from the Antipodes is no doubt regarded as a greater curiosity than a specimen from the neighbouring hills. But if a small museum is to have any educational value worth naming, its aims should be restricted, at least in the early stages of its development. Many museums undoubtedly teach too little by attempting to teach too much.

Perhaps the chief cause of unsatisfactory arrangement in so many museums is to be found in the difficulty of curatorship. Most museums naturally take their complexion from those who have charge of them; if the curator, for example, is a good entomologist, the collection of insects will be good; and so on. A general museum, indeed, needs a curator just a trifle less than omniscient. Even where each department is under charge of some honorary specialist, it by no means follows that the greatest educational value is got out of the collections. It seems to me that it would be an advantage, wherever practicable, to establish some kind of connection between the museum and the nearest college or other educational centre; assuming, of course, that it is a centre of liberal education where science asserts its proper position. Just as lectures teach principally through the ear, so museums teach through the medium of the eye; and those who have had most experience in oral teaching will probably be best qualified to assist in the oversight of an educational museum.

Another direction in which most museums imperatively need reform is in the simple matter of labelling. Too often the visitor leaves without carrying away much information, simply because he is unable to interpret what he has seen. A curator can therefore hardly be too free in the use of descriptive labels. Large labels, no doubt, occupy a good deal of space, and this can be ill spared in a crowded collection. Nevertheless, I believe it is far better to exhibit only half the number of specimens, fully telling their own tales, than to cram the cases with specimens unnamed or only meagrely described. If a museum is to be of real value educationally, it must be made as far as possible its own interpreter. F. W. RUDLER

Scientific Club, Savile Row

I HOPE ventilation of this subject in the columns of NATURE will direct attention to the necessity of more systematic arrange-

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[1] See also Proc. Roy. Soc. (March, 1876.) Vol. xxiv. p. 273.

ment, and that governors will seek to redeem these institutions from the mere curiosity shop style into which too many have developed, and to render them valuable educational instruments.

Interested in geology, I have been pleased, in occasional visits with pupils to our local museums, to note the gain to accurate knowledge as the diagrammatic illustration of the text-book is exchanged for the fuller teaching of fossil and specimen, and where, to chronological and stratigraphical plan, the characteristic fossils are indicated by special labels, and the time range shown by variously coloured mounts, the advantage is considerable. I would further suggest the desirability of numbering important objects, as in a picture gallery, and furnishing the visitor with an attractive catalogue. Where several museums exist in the same town could not the authorities, by mutual agreement, economise space and effort by division of labour, each one becoming to some extent exhaustive in a special direction?

In one of our best arranged museums I recently found, for pure want of room, Cambrian trilobites associated with basic rocks, and a fossil neatly stowed away in a case of Vesuvian products.

Bright, convenient, and well-keyed, our museums ought to increasingly attract students and gather in recruits from time to time from the inquiring public. A good supply should in this, as in some other educational difficulties, create demand, and stimulate public sentiment until our museums become so commodious and well-appointed as to bear comparison with the excellent models Prof. Dawkins refers to as established by our Continental neighbours; and the natural sciences gain in dignity amongst us until they enter into healthy rivalry with the elder and established studies of numbers and letters.

Manchester, June 2 WILLIAM GEE

I SEE with pleasure that Prof. Boyd Dawkins has again raised his voice urging the importance of museums as a means of education, but as there is one point regarding their management to which it may be useful to call attention, I shall be glad if you will allow me to do so through your pages.

Undoubtedly English museums compare most unfavourably with foreign ones, and this partly arises from the idea which is so prevalent that one man ought to be able to arrange and determine anything from New Zealand birds, plants, or fossils to a collection of Egyptian idols. The consequence is that we see such incongruities as were pointed out by Prof. Dawkins, to which I should like to add another, from one of the leading technical institutions of London; there a few years ago (and I suppose there are still), among building materials, some large Nummulites (a genus of fossil foraminifera), marked portions of brick made by the Israelites for the Egyptians when they were allowed no straw.

In a middle-sized foreign town in any of the other civilised countries of Europe there is a museum in charge of men who have given their attention to various branches of science; even in Italy, which is much behind in this matter of museums, we find in such towns as Turin a well-arranged museum with a considerable staff of curators, with the minerals in the hands of one man, the fossils in another, the vertebrates have, I believe, two or three of the staff to work at them, while the invertebrates are in the hands of another, and in the same way the historical and technical portions are no doubt under adequate management.

When we turn back to England we find such a humiliating thing as a town like Manchester with no museum worthy of a fourth-rate town.

It will no doubt be some time before their importance is fully recognised and therefore as museums are likely to be for a long time insufficiently manned, might it not be a great advantage if a number of local museums joined together to employ specialists to determine different groups? Such work might no doubt be done very cheaply, for such men would often be glad of the opportunity of so much material passing through their hands.

A naturalist who was making any group, such as corals or crustacea, his subject, might visit the museums and would in a very short time be able to determine and arrange the greater part of the local collection, and might have those which required further research sent up to London for investigation at leisure upon the completion of his tour. ARTHUR WM. WATERS

Alderley Edge

The Antiquity of Man

HAVING carefully perused the proceedings that took place at the recent "Conference" on the subject of the antiquity of man at the Anthropological Institute, I confess to a feeling of disappointment. I had looked, if not for new geological facts, at least for something novel in the treatment of what was already known, instead of which the geological speakers seem, for the most part, to have merely reiterated opinions with which their names have been for some time identified. Thus my able opponent, Prof. Boyd Dawkins, does no more than restate views and conclusions which have already been controverted more than once, and to which, therefore, I need not reply here, as in so doing I should be only summarising what has been stated at length elsewhere. Mr. Dawkins's "case" and my own are now so fully before our fellow-hammerers that we may be well content to leave them for judgment to the future—a future which is probably not far off. Prof. Prestwich, again, while quite open to conviction that man may have lived in England in pre-glacial times, is yet strongly of opinion that all the human relics hitherto obtained in the south of England are of *post-glacial* age, because they occur in deposits that overlie "the boulder-clay." Now this conclusion would certainly follow if it could be shown that the "chalky boulder-clay" of East Anglia represents, as Prof. Prestwich thinks it does, the glacial period. Unfortunately it only represents one phase of that period. There is an *older* boulder-clay than that "chalky till," and there are two separate boulder-clays which are *younger*, as Mr. S. V. Wood has demonstrated. The East Anglian chalky boulder-clay was laid down, as I believe, during the climax of glacial cold, and is consequently much older than the upper boulder-clays that occupy the surface of Scotland and the North of England. For the evidence which has weighed with me in coming to this conclusion I must refer Prof. Prestwich to the account of the English glacial deposits, which is given in the second edition of my work on the Ice Age. The proofs and argument are too long to recapitulate here. That the East Anglian chalky till belongs to a much more ancient date than the upper boulder clays of Yorkshire and the North, must strike any one who will take the trouble to compare them. The East Anglian deposit has been subjected to long-continued and powerful erosion, and everywhere bears the impress of extreme antiquity, while the younger tills of the North have a comparatively recent appearance. Nor is this by any means all, for between the accumulation of the chalky till and the formation of the most recent boulder-clay or till of the North there certainly intervened one mild inter-glacial period. (There were in reality, as I believe, two such periods.) Now during the "last inter-glacial period"—that, namely, which preceded the deposition of the youngest boulder-clay of Yorkshire and the North—there certainly existed a land-surface in England over which the pleistocene mammalia roamed. The proofs of this are found in certain fresh-water and estuarine deposits which are met with near Hull and elsewhere, and which have yielded mammalian remains, and thousands of *Cyrena fluminalis* and other shells. Prof. Prestwich has himself described these beds and classified them as *post-glacial*, partly because they repose upon boulder-clay and partly on account of their fossil contents. But since the date of Prof. Prestwich's visit to the locality in question, the section (near Burstwick) has been much better opened up, and now one may see *resting upon these so-called post-glacial deposits a thick mass of tumultuous boulder-clay*. This boulder-clay is in my opinion as truly the product of glacier-ice as any ground-moraine or till in Scotland, Norway, or Switzerland, and points to a time when all Scotland and the northern districts of England, down as far as the valley of the Humber, were shrouded in snow and ice.

With reference to the recent discoveries by Mr. Skertchly near Brandon, which Mr. Evans and Prof. Hughes have convinced themselves lend no support to the view that man is other than post-glacial, I would ask geologists to suspend their judgment until they have had an opportunity of hearing the other side. Let them exercise a little of that "caution" which Mr. Evans desiderates, and not too readily acquiesce in his and Prof. Hughes' ruling. Mr. Skertchly, who has mapped the ground about Brandon and Thetford, and whom we may suppose, therefore, to be more intimately acquainted with the geology of that district than either of his opponents, has no doubt that certain implement-bearing brick-earths are covered by boulder-clay *in situ*. I have also carefully examined the sections in question and feel quite sure that Mr. Skertchly is right, and that the overlying accumulation is a true glacial deposit, and an integral portion of the so-called chalky boulder-clay. Prof. Ramsay, who has likewise recently visited Brandon, is, I believe, of the same opinion. But the occurrence of flint implements underneath the chalky till of East Anglia is, after all, no proof that these relics are pre-glacial. The most one can say about them is simply this, that the folk who used them lived in England before the climax of glacial cold. When human relics are got in beds of older date than those at Cromer, we shall then have a demonstration of the pre-glacial age of man in Britain. At the same time the presumption is (as many geologists will admit) that some portion of our ancient river-drifts and cave-deposits with flint implements do really belong to pre-glacial times. In short, after carefully reading the proceedings at the recent Conference, I find nothing to shake me in my present belief that none of the palæolithic deposits belongs to post-glacial times, but that all must be relegated to inter-glacial, and probably pre-glacial ages, and consequently that the palæolithic is separated from the neolithic age by the intervention of the last cold period of the glacial epoch. My opinion, therefore, is still as strong as ever that "until we clearly understand what was the succession of changes during the ice age, it is premature to speculate upon the geological age of those deposits which yield the earliest traces of man in Britain." In concluding, may I be allowed to suggest to the anonymous writer whose communication on the subject of the Antiquity of Man appears in the number of this journal for June 7, that before he again essays to criticise my views he might do well to become better acquainted with them. JAMES GEIKIE

Perth, June 15

BEFORE your readers accept the statements of Messrs. Evans and Hughes respecting my discovery of flint tools beneath the great chalky boulder-clay, as announced in NATURE last year, may I ask them to remember that as yet I have not published the evidence upon which I founded my statement? The delay has arisen from official and other causes; and although my paper is now written, it is, I have just learned, too late for reading during the present session at the Geological Society. Neither of the two gentlemen named is aware of the extent of my evidence, for I have not, as yet, told any one about it, except the two geologists mentioned below. As I shall show, there are now known to me about forty localities in which the brick-earths in question occur, and in most of them their relation to the boulder-clay is very clear; and even in the two or three spots in which that rock is not seen in the actual section, it overlies the implement beds near by on the same outcrop. The brick-earths have naturally suffered much denudation by the boulder-clay, and I have a splendid series of sections showing every phase from almost undisturbed material beneath the boulder-clay to small fragments (boulders, in fact) in that deposit. I wish, also, to state, that instead of four implements from two localities, as originally announced, I know at present nearly 150 from six different spots. The evidence is so clear and overwhelming when seen *en masse*, that it must be convincing to all who carefully weigh it. The boulder-clay which overlies the brick-earths in question is part and parcel of the great mass of the chalky boulder-clay, a formation which I have spent eight years in examining in the field almost daily, of which I have mapped about 2,000 square miles, and upon which I feel quite competent to form an opinion.

Prof. Prestwich is perfectly correct in ascribing the well-known palæolithic implements found in the gravel to a time subsequent to the formation of the chalky boulder-clay; *but that only proves those tools to be newer than the last glaciation of this particular area.* Now inasmuch as Mr. Searles V. Wood, jun., long ago proved, and as everybody who examines the ground must admit, that the "purple" and "hessle" boulder-clays are newer than the one we are dealing with, and as Dr. J. Geikie has shown that gravels bearing the same character and possessing the same peculiar fauna as the well-known palæolithic gravels (overlying, moreover, the chalky boulder-clay), pass under these newer beds, it is, to say the least, a misnomer to call these gravels *post*-glacial. They are post-glacial to this East Anglian area, but not to northern England; and the distribution of surface-beds containing palæolithic implements throughout Europe shows that they are confined exclusively to that area which was free from the erosive action of the newer and less intense ice-sheets of the latter part of the "Great Ice Age." Much of the misapprehension in this matter has arisen from the unfortunate name of "upper" given to the chalky boulder-clay. It *is* "upper" in East Anglia, but it is "lower" in Yorkshire.

My discovery does *not* prove man to have been pre-glacial; it merely shows that he was "pre-chalky-boulder-clay," and I last week obtained evidence to show that the brick-earths in question belong to the "middle glacial" of Mr. Searles V. Wood, jun.; that is to say, they are newer than the Cromer till, but older than the chalky boulder-clay.

Mr. Belt is mistaken respecting the quartzite implements near Brandon. They are found in gravel which is unquestionably above the boulder-clay, as can be seen in hundreds of sections, and the only conceivable source of that material is the boulder-clay. We have, in fact, two horizons of palæolithic implements, one above and one below the boulder-clay, and I am in hopes that the former will be found capable of subdivision, for many facts crop up in the course of my daily work which seem to point in that direction.

Prof. Ramsay and Dr. J. Geikie, who are eminently capable of judging of glacial phenomena, have gone over the area with me, and are perfectly convinced of the accuracy of my determinations. SYDNEY B. J. SKERTCHLY

Brandon

Nicephore Niepce

THERE is an error in one of your "Notes" of last week which you may be glad to have corrected. It is not to Niepce de St. Victor that the citizens of Chalons-sur-Saône (a town, by the way, not to be mistaken for Chalons in the Champagne country) are about to erect a statue, but to his uncle, Joseph Nicephore Niepce, who might well be designated as the first photographer, since he it was who succeeded first of all in fixing an image in the camera. In a "Life of Nicephore Niepce,"[1] recently published by Victor Fouqué, appear letters which leave little doubt that in May, 1816 Niepce had accomplished the feat of fixing shadows in the camera, for in a communication of that date to his brother he incloses four photographs, of which he says: "The pigeon-house is reversed on the pictures, the barn, or rather the roof of the barn, being to the left, instead of the right. The white mass which you perceive to the right of the pigeon-house, and which appears somewhat confused, is the reflection upon the paper of the pear-tree, and the black spot near the summit is an opening between the branches of the trees. The shadow on the right indicates the roof of the bake-house." This, then, is a description of the first camera-picture ever taken, and it was by reason of Niepce's inability to prevent his impressions from fading after a lapse of time that he turned his attention to the bitumen of Judea process, with which he produced photographs as early as 1824, one or two specimens being still among the science treasures of the British Museum.

The name of Nicephore Niepce is little known in England. And yet this should not be. As is well known he came to this country in 1827, and resided at Kew in the hope to receive aid and encouragement, and shortly afterwards, on his return to France, entered into partnership with Daguerre to work out together a more practical process. When Daguerre made known his discovery in 1839, his partner had been dead two years, and no mention was made of Niepce at the time Arago made his famous speech announcing the discovery of Daguerreotype. Specimens of the wonderful process were not long in reaching this country and the first picture was placed in Faraday's hands with the remark that he had never seen anything like it before. But Faraday said he had. A Frenchman, he remembered, had brought him a picture of Kew Church a dozen years ago, with the quaint remark, that "the sun had done it." Faraday was so certain of this that inquiries were at once instituted into the matter, and in the end, a communication was addressed by the Secretary of the Royal Society, Mr. Bauer, to the *Academie* at Paris, a communication which helped materially to substantiate the claim of the Niepce family, and to obtain for the son Isidore, a pension in acknowledgment of the father's services. The deed of partnership between Niepce and Daguerre is still extant, but how much of the latter's published results were due to his dead partner the world will never know. H. BADEN PRITCHARD

June 6

Japanese Mirrors

THERE is still something to be solved about the Japanese mirrors, which show the figures that appear in relief on the back in the disk of light reflected from the face. Not only does it seem impossible (without some indication which I have not yet met with) to tell which mirrors in a series will perform the feat, but it is equally difficult to say why one bunch of leaves will, and another in the same casting, will not appear in the spectrum.

In reference to Mr. Highley's quotation of Mr. Prinseps' conjecture (p. 132), "that the thinnest parts, from being the hardest, should give the strongest reflection, owing to a difference in density produced by stamping," and to the brassworker's solution, not only are the mirrors not stamped, but cast; but it is the thicker parts, those which stand out on the back in highest relief, which reflect the most light. I have one on the back of which there are two large characters as it were laid upon a background of trees. These letters have been carefully ground flat and polished like the front. Their figures not only appear more distinctly than those of other less highly raised adornments, but actually, in the sun, throw off a brilliant white light, which contrasts very markedly with the comparatively subdued spectrum from the rest of the plate.

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[1] "La Verité sur l'Invention de la Photographie."

Is it possible that there may be some difference in molecular arrangement during the consolidation of the melted metal in the thicker (relieved) and other parts of the plate? And yet, the thick rim of the mirror does not reflect a rim of light.

One of my specimens has suffered a little oxidisation, and I observe that this appears on the face to follow certain of the larger masses of relief on the back. This mirror does not "show the pattern through," but there is a curious bright rim reflected from the edge of each scar of injured surface.

Manchester R. D. DARBISHIRE

Colour-Sense in Birds

As the fact of the preference of sparrows for *yellow* crocuses still excites interest and requires explanation, perhaps you will allow me to call attention to the following remarks of Gilbert White in his "Observations on Nature":—

"Birds are much influenced in their choice of food by colour, for though white currants are a much sweeter fruit than red, yet they seldom touch the former till they have devoured every bunch of the latter."

The obvious criticism that the craving for sweets which distinguishes the human biped is not equally predominant among his feathered friends, and consequently, that their selection of the less sweet but more highly coloured fruit may be due to some taste other than the æsthetic, does not detract from the importance of White's generalisation that birds are much influenced in their choice of food by colour—a generalisation which, there is no reason to doubt, was based upon his own keen and repeated observation. PAUL HENRY STOKOE

Beddington Park

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—The following are Greenwich mean times of visible geocentric minima of Algol, for July, August, and September, according to the elements adopted by Prof. Schönfeld:—

	h. m.		h. m.		h. m.
July 11	11 51 ...	Aug. 20	15 11 ...	Sept. 12	13 39
,, 31	13 31 ...	,, 23	11 59 ...	,, 15	10 28
Aug. 3	10 20 ...	Sept. 9	16 51 ...	,, 18	7 16

Minima of S Cancri occur on September 8 at 15h. 1m., and September 27 at 14h. 14m.

A minimum of Mira Ceti will fall on July 23, according to Argelander's formula of sines, the same perturbations being applied as in the case of the maximum of the year, which is computed to occur November 9·7.

Mr. John Tebbutt, writing from Windsor, N.S. Wales, on April 13, states that in consequence of remarks on the probable variability of μ Doradûs, in NATURE, vol. xv. pp. 14 and 281, he examined the star on February 26, and March 14, and found it of the 8th magnitude. There is a star, estimated 9th magnitude, about thirty seconds of time west, and twelve seconds north of it. With such an instrument as was employed by Lacaille at the Cape of Good Hope in 1751, μ Doradûs, with its present brightness, would hardly have been visible. Lacaille calls it a fifth magnitude.

MINOR PLANETS AND COMETS OF SHORT PERIOD.—Dr. von Asten, in the course of his recent researches on the motion of Encke's comet, found that, although in the interval 1819-68 the comet had experienced in each period of revolution an almost exactly equal amount of acceleration, and that this might be attributed to the existence of a resisting medium, yet in order to connect the last two appearances in 1871 and 1875 with the previous ones, it is necessary to have recourse to the hypothesis of an extraordinary perturbation which, in the period 1868-71, counteracted the influence of a resisting medium. For certain reasons Dr. von Asten is led to conjecture that about the middle of the year 1869, when the comet was in the region occupied by the numerous group of small planets (the radius-vector being about 3·2), it made so close an approach to one of these bodies, as yet undiscovered, that a sensible effect on the comet's mean motion was the result.

In connection with this hypothesis it may be interesting to note that the late Prof. Hubbard, whose masterly investigations on the motion of Biela's comet appeared in Gould's *Astronomical Journal*, came to the conclusion that the separation of the comet into two distinct bodies, by whatever cause effected, took place in all probability in a heliocentric position corresponding to about longitude 318°·6, latitude + 12°·0, with radius-vector 4·36, which position the comet occupied in November, 1844 (*Ast. Journ.*, No. 140). It is stated in some works that the comet in 1846 separated under the very eyes of astronomers; nevertheless it is upon record that the companion was first recognised on December 29 by Herrick and Bradley at New Haven, but was not again seen until Maury refound it on January 13; and its not having been remarked when the comet was first glimpsed in the Northumberland and other powerful telescopes may well have been owing to distance and faintness.

A radius vector of 4·36 would, until quite recently, have been considered as placing the comet rather outside the probable superior limit of distance of the minor-planet group, but the discovery of Hilda by M. Palisa in November, 1875, considerably extended the limit, this body in aphelion being distant from the sun 4·6. Although the separation of Biela's comet, if it really took place at the epoch assigned by Prof. Hubbard, could not have been owing to an encounter with this particular planet, yet the position indicated for the occurrence is clearly a possible one for a meeting with an unknown member of the group. In saying this much we are of course aware that the separation may have been owing to a very different cause, indeed it might be supposed that such a *rencontre* would have left a more sensible effect upon the mean motion of the comet.

METEORIC FIRE-BALLS IN AMERICA.—Prof. Daniel Kirkwood in a communication to the American Philosophical Society, on March 16, gives some particulars of meteoric fire-balls which appeared in unusual number in the United States in the latter part of 1876 and beginning of the present year. The circumstances attending the appearance of eight conspicuous meteors are included: the dates were 1876, July 8 (two fire-balls), December 16 and 21, January 3, 20, and 23, and February 8. The train of the larger meteor of July 8 was visible at least forty minutes, the mass having been apparently dissolved or dissipated in the latter part of its track; the motion about the sun was retrograde, but sufficient materials were not forthcoming for determining the orbital velocity or the nature of the orbit. The fire-ball of December 16 had been visible but a few seconds near San Francisco when it apparently plunged into the Pacific at no great distance from the shore, the fall being followed by a loud detonation. The meteor of December 21 was remarkable for the length of its track, between 1,000 and 1,100 miles, one of the longest upon record, and, moreover, the track would appear to have been somewhat curved. When crossing Indiana the principal fire-ball was followed by a train of smaller meteors, many of which exceeded Venus and Jupiter in apparent magnitude; the breadth of the cluster, as seen from Bloomington, was 3°, and the length at least 20°, from which Prof. Kirkwood concludes that the true diameter was five miles; and the length about forty miles; several explosions occurred during the passage of the meteorite over Indiana and

Ohio, and a fragment weighing about twelve ounces, fell upon a farm near Rochester, Indiana, a part of it being secured by Prof. Kirkwood. The body is described as "peculiar in its structure; being pisolitic and remarkably friable." It is inferred that no part of the mass could have escaped out of the atmosphere. The aerolite of January 23, or rather a portion of it, after the final explosion, reached the earth in Kentucky, and is now in the collection of Dr. J. Lawrence Smith, of Louisville. The report is stated to have resembled discharges of heavy ordnance, in such close succession, that the different discharges were barely distinguishable; height at first appearance about seventy miles.

THE TRANSIT OF VENUS, 1882.—In *Astron. Nach.*, Nos. 2133-4, we have another calculation of the elements of this transit from M. Leverrier's Tables; it is by Dr. Deichmüller, of the Observatory at Bonn, who has followed Prof Oppölzer's method for the necessary data for reduction of the observations which are interpolated down to short intervals in Paris time. There are special calculations for certain principal stations.

THE LAND OF HISSAR AND KOLAB

THE eastern part of the dominions of the Emir of Bokhara is the belt of land between 37° 30′ and 39° N.L., and 67° and 71° E.L., bordered on the north by the now Russian province of Samarkand and the Karategin, and on the south by Afghanistan (the Balkh, Kunduz, and the Badakshan districts). It has hitherto been all but totally unknown. M. Maïeff, after having, together with Lieut. Vishnevsky and M. F. Schwarz, thoroughly explored it in 1875, gives us a description of the land of Hissar and partly that of Kolab (*Isvestia* of the Russ. Geog. Soc., 1876, 4th fasc.), with an elaborate map, based on numerous determinations of latitudes and longitudes, surveys, and barometrical measurements of heights.

Two great rivers running east and west, the upper Zerafshan on the north, and the Pandsh, or Upper Oxus, on the south, are the natural boundaries of the country. A third river, the Shehrsebz, running in the same direction under 39° N.L., borders its north-western corner, and a mass of high table-lands, the Pamir, rises to the east of the Kolab district. The whole land is filled with mountains belonging to the Tian Shan system. Two main ridges, which both run north-east to south-west, and are divided by the broad valley of the Surhan,[1] form the backbones of this hilly tract. Secondary ridges, either parallel to the main ones, or spreading out of them, fill the country. But at their western extremities, the mountain ridges are far lower than we know them to be in the east. Thus, the ridge between the Shehrsebz and the Surhan, now called the Hissar Ridge,[2] rising above the snow-line in its eastern parts, is far lower in the western; and its highest pass, Ak-rabat, is but 4,590 feet high, whilst other passes are as low as from 2,200 to 3,600 feet above the sea-level. The second main ridge, lying to the east of the broad Surhan valley and running between it and that of the Vaksh, seems to be higher, but yet far below the highlands of Kokand or of Eastern Turkestan. Besides, the highlands are deeply cut into by large and broad valleys which have in their lower parts a prevailing direction south by west, running thus to the Oxus. The north-western slope of the Hissar ridge is drained by only one river, the Guzar-daria, an affluent of the Shehrsebz; but, instead of being an insignificant stream, as on our present maps, it appears as a mighty river fed by the perpetual snows of the Sengri-dag, and its upper shores are occupied by a numerous population. East of the Hissar ridge we see a series of broad well-peopled valleys. First, that of the Shir-abad, from about 2,500 to 900 feet high, with the towns Derbent, Baisun, and Shir-abad. Next, the valley of the Surhan river which, as well as the Guzar and the Shir-abad, rises in the snow-covered ridge Meshai-Kentely, and receives many affluents. Some time before its annexation to Bokhara, this valley was a centre of the political life of the country, and, going back in its history, we come to a time when—a local tradition says—the population was so dense that a cat could travel upon the roofs of the dwellings from Denau to the Amu. Now, the population is concentrated in the upper, better-watered parts of the valley, where we find the towns Kara-tag, Sary-djui, Yourchi, and Denau. Further east we have the valley of the Kafirnagan (the Ramid of Ibis-Dast), the source of which is about Paldorak, this river being second in size to the Surhan. An enlargement in the upper parts of its valley, running east and west, is well peopled, and contains the towns Hissar, Fyzabad, Kafirnagan, and Doshambe. Then, below Hissar, the river enters a deep ravine, Pavi-Duldul (the foot of the Duldul, the mythic horse of Ali), at the issue of which is the town Kahadian (460 feet high), close to the Amu-daria.

Further east, beyond the second main ridge alluded to above (its local names are—By-katyn, Mazi-melek, Avan-tau, &c.), we have the basins of the Upper Oxus, *i.e.*, of the Vaksh (or Surhab) and the Pandsh, which both, M. Maïeff observes, must be considered as the sources of the Oxus. The former rises in the glaciers of the Alai-ridge and runs, under the name of Kyzyl-soo, on the Pamir table-land. After having received a great affluent, the Muk-soo, the river bears the name of Surhab and enters the Bokhara dominions through an impracticable deep ravine, at the well-known bridge Pooli-sengui. After a short course among mountains it soon emerges on a plain some 500 feet high. Divided into many branches, the main one about 180 yards broad, it runs to its junction with the Pandj near Kurgan-tube. Only one of the affluents of the Pandj, the Kchi-Surhab (little Surhab), was explored by M. Maïeff; it is formed by two rivers, the Baldshoan and Kolab, the valley of the latter being well peopled and cultivated, notwithstanding the extensive marshes which have given their name to the town, Kolab.

The population of the country consists of Usbecks and Tadjicks, the former occupying mostly the lower and better parts of the valleys, having driven the Tadjicks back to the upper parts. The banks of the Amu-daria, and especially the western parts of the country, are mostly peopled with Kungrad-Usbecks, the Tadjicks appearing more numerous to the east. The towns contain, as usual, a very mixed population. The lower parts of the Vaksh and the Kolab valley are mostly peopled by Usbecks of the Katagan tribe. Some Kirgises have begun to found settlements in the lower parts of the Vaksh and Pandsh valleys; and some miserable Turkomans are strewn among the Usbecks on the shores of the Amu. Jews, Hindoos, and Afghans form a very small percentage of the population.

As to the climate of the country, it is easy to perceive that it must be comparatively mild. In the higher parts of the Kafirnagan valley there are occasionally falls of snow about two feet deep, but the lower parts of the valleys have a mild, rainy winter. Figs grow at Shir-abad unsheltered during the winter. All kinds of corn and fruits common to Central Asia are produced in abundance. Cotton, however, is cultivated only in Shir-abad, owing to facility of export to Karshi (on the Shehrsebz). Rock-salt is worked in the neighbourhoods of Guzar and on the Upper Vaksh, but it must be found also elsewhere, the salt springs being numerous. Two gold mines are known on the shores of the Vaksh, and richer ones are reported to exist in the Darvaz.

The country is under the dominion of the Emir of Bokhara, being administered by nine *becks*, vassals to the emir, seven in the Hissar district, and two in that of Kolab.

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[1] Tupalik on Mr. Arrowsmith's map in the *Journ.* Roy. Geog. Soc., 1875. Tupalan is the name of one of the upper affluents of the Surhan.

[2] Its local names are Bash-hurd, Baisun-tau, Meshai-kenteli, &c.

HOW TO DRAW A STRAIGHT LINE[1]

IV.

I NOW come to the second of the parallel motions I said I would show you. If I take a kite and pivot the blunt end to the fixed base and make the sharp end move up and down in a straight line, passing through the fixed pivot, the short links will rotate about the fixed pivot with equal velocities in opposite directions; and, conversely, if the links rotate with equal velocity in opposite directions, the path of the sharp end will be a straight line, and the same will hold good if instead of the short links being pivoted to the same point they are pivoted to different ones.

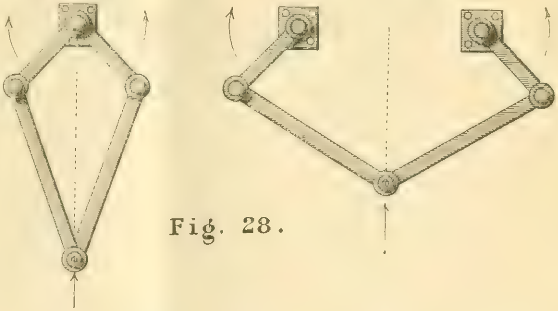

Fig. 28.

To find a linkwork which should make two links rotate with equal velocities in opposite directions was one of the first problems I set myself to solve. There was no difficulty in making two links rotate with equal velocities in the same direction—the ordinary parallelogrammatic linkwork employed in locomotive engines, composed of the engine, the two cranks, and the connecting rod, furnished that; and there was none in making two links rotate in opposite directions with *varying* velocity; the contraparallelogram gave that; but the required linkwork had to be discovered. After some trouble I succeeded in obtaining it by a combination of a large and small contra-parallelogram put together just as the two kites were in the linkage of Fig. 18. One contra-parallelogram is made twice as large as the other, and the long links of each are twice as long as the short.

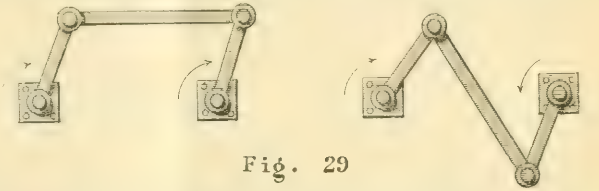

Fig. 29

The linkworks in Figs. 30 and 31 will, by considering the thin line drawn through the fixed pivots in each as a link, be seen to be formed by fixing different links of the same six-link linkage composed of two contra-parallelograms as just stated. The pointed links rotate with equal velocity in opposite directions, and thus, as shown in Fig. 28, at once give parallel motions. They can of course, however, be usefully employed for the mere purpose of reversing angular velocity.

An extension of the linkage employed in these two last figures gives us an apparatus of considerable interest. If I take another linkage contra-parallelogram of half the size of the smaller one and fit it to the smaller exactly as I fitted the smaller to the larger, I get the eight-linkage of Fig. 32. It has, you see, four pointed links radiating from a centre at equal angles; if I open out the two extreme ones to any desired angle, you will see that the two intermediate ones will exactly *trisect the angle*. Thus the power we have had to call into operation in order to effect Euclid's first postulate—linkages—enables us to solve a problem which has no "geometrical" solution. I could of course go on extending my linkage and get others which would divide an angle into any number of equal parts. It is obvious that these same linkages can also be employed as linkworks for doubling, trebling, &c., angular velocity.

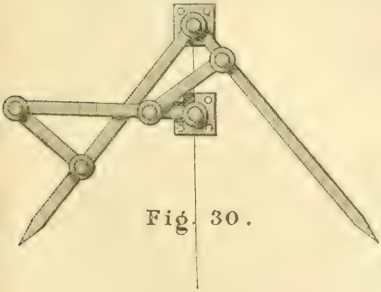

Fig. 30.

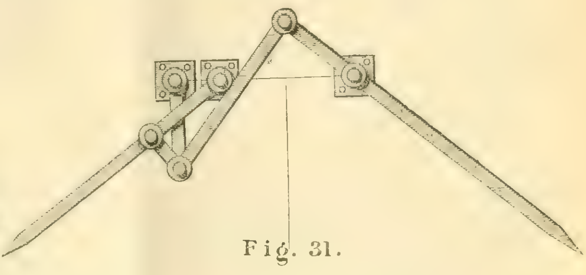

Fig. 31.

Another form of "Isoklinostat," for so the apparatus is termed by Prof. Sylvester, was discovered by him. The construction is apparent from Fig. 33. It has the great advantage of being composed of links having only two pivot distances bearing any proportion to each other, but it has a larger number of links than the other, and as the opening out of the links is limited, it cannot be employed for multiplying angular motion.

Subsequently to the publication of the paper which contained an account of these linkworks of mine of which I have been speaking, I pointed out in a paper read before the Royal Society, that the parallel motions given there were, as well as those of M. Peaucellier and Mr. Hart, all particular cases of linkworks of a very general character, all of which depended on the employment of a linkage composed of two similar figures. I have not sufficient time, and I think the subject would not be sufficiently inviting on account of its mathematical character, to dwell on it here, so I will leave those in whom an interest in the question has been excited to consider the original paper.

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[1] Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A. Concluded from p. 127.

At this point the problem of the production of straight-line motion now stands, and I think you will be of opinion that we hardly, for practical purposes, want to go much farther into the theoretical part of the question. The results that have been obtained must now be left to the mechanician to be dealt with, if they are of any practical value. I have, as far as what I have undertaken to bring before you to-day is concerned, come to the end of my tether. I have shown you that we *can* describe a straight line, and *how* we can, and the consideration of the problem has led us to investigate some important pieces of apparatus.

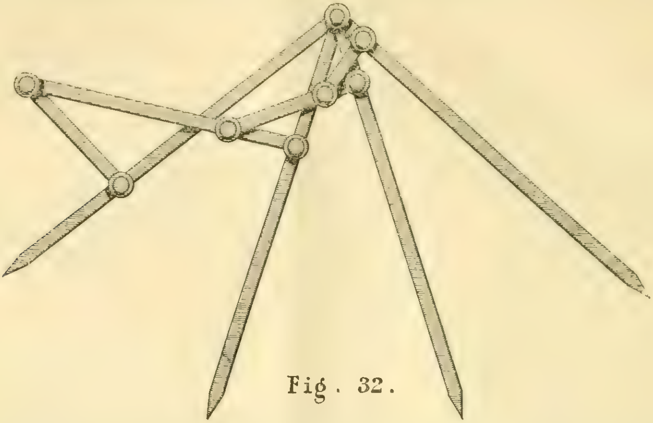

Fig. 32.

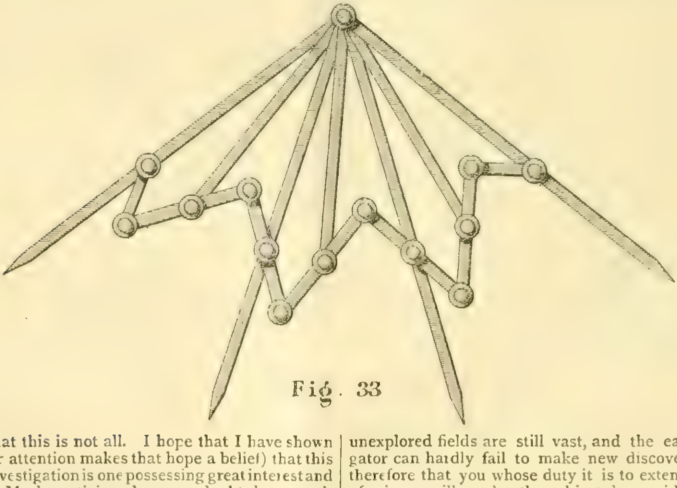

Fig. 33

But I hope that this is not all. I hope that I have shown you (and your attention makes that hope a belief) that this new field of investigation is one possessing great interest and importance. Mathematicians have no doubt done much more than I have been able to show you to day, but the unexplored fields are still vast, and the earnest investigator can hardly fail to make new discoveries. I hope therefore that you whose duty it is to extend the domain of science will not let the subject drop with the close of my lecture.

BIOLOGICAL NOTES

THE TICHORHINE RHINOCEROS.—A number of the *Memoirs* of the Imperial Academy of Sciences of St. Petersburg just issued contains an elaborate article on the Tichorhine Rhinoceroses by the veteran naturalist, Dr. J. F. Brandt. Dr. Brandt treats of two extinct species under this category, which he calls *R. antiquitatis* (i.e., *R. tichorhinus*, auctt.) and *R. merkii.* With the latter he proposes to unite *R. etruscus* of Falconer. Remarks are added upon *R. leptorhinus* of Cuvier and other allied species. When we consider the number of valuable contributions to science recently made by Brandt, Middendorf, Kowalewski, Radde, von Schrenck, and other distinguished names of the Academy of St. Petersburg, it becomes somewhat ridiculous to a naturalist to hear the oft-repeated assertion of the British patriot "that the Russians are as great barbarians as the Turks!"

OSCAR HERTWIG ON THE PHENOMENA OF FERTILISATION.—The last number (Vol. iii., Part 1) of the *Morphologisches Jahrbuch*, contains the second part of Oscar Hertwig's very important researches on the phenomena immediately preceding the cleavage of ova in the Echinoid *Toxopneustes*, in two genera of leeches, and in the amphibia. He has watched most carefully the process of fertilisation and the ova before fertilisation, and has examined them after the action of various reagents. His accounts are accompanied by very valuable figures. From his own observations, and a comparative study of other recorded facts, he appears to have made generalisations worthy of the attention of all biologists. The following is a brief summary of his conclusions. The unripe ovum is characterised by the possession of a germinal vesicle, distinguished from all cell-nuclei by its great relative size, by its definite membrane, its more or less fluid contents, and its possession of one or several nucleoli. The germinal vesicle in this signification does not become the nucleus of the first cleavage-sphere; in many animals it disappears long before fertilisation, in other cases during that process or during the ripening of the ovum. At any rate the germinal vesicle loses all its distinctive characters. The active nuclear substance, or a part of it, remains and forms a new nucleus of much smaller size, lacking a distinct membrane and true nucleoli. From a highly differentiated form is produced a primitive nucleus; instead of a germinal vesicle we have an ovinucleus. In Toxopneustes the retrogression of the germinal vesicle is accompanied by its movement to the outer surface of the yolk, where it disappears, with the exception of the germinal spot; the latter again reaches the centre of the yolk and becomes the nucleus of the ripe ovum. In Hirudineæ there is an accessory prelude to fertilisation, the budding-off of "directive bodies" immediately after oviposition. After this arises a spindle-shaped ovinucleus really derived from the breaking up of the germinal spot. In amphibia the exceedingly large germinal vesicle gets to the surface and disappears. Only a small portion, one or more nucleoli, passes over into the inconsiderable ovinucleus. The parts of the germinal vesicle not contributed to the ovinucleus seem no longer serviceable, and get transformed into the so-called excretory bodies and sphere. In Amphibia a mass of this kind appears as a yellowish covering over the dark pole of the egg. Like Toxopneustes appear to be Medusæ, Siphonophora, Ascidians, some Vermes, Arthropods, &c., possessing in the ripe and unfertilised ova a small homogeneous, membraneless nucleus in the middle of the yolk or on its periphery. The Hirudineæ resemble Gasteropods, Heteropods, Pteropods, and some Vermes. Here the ripe egg has mostly on its periphery a small spindle-shaped nucleus. In fishes and reptiles, as in the frog, there is a germinal vesicle with many nucleoli, some of which form the ovinucleus. After this stage Hirudineæ twice exhibit a budding from the surface of the ovum forming the so-called directive bodies, the ovinucleus contributing to them. The actual occurrences of fertilisation correspond very closely not only in animals but in plants. In Toxopneustes a single spermatozoon reaches the ripe ovum and is transformed into a small corpuscle, the sperm-nucleus, surrounded by a protoplasmic rayed figure. It travels in from ten to fifteen minutes to the central ovinucleus and is fused with it. In *Rana temporaria* the spermatozoon enters at the side of the excretory body and becomes like that of Toxopneustes, travelling to the ovinucleus and fusing with it. In Hirudineæ the spermatozoon enters subsequently to the budding of the first directive body, and after transformation gets to the centre of the ovum and there remains till the budding of the second body. Then the ovinucleus travels to the centre and is apposed to and fuses with the sperm-nucleus, which has swollen considerably. Thus in these cases the cleavage-nucleus is formed by the union of the two sexually-differentiated nuclei.

INDIVIDUAL VARIATIONS IN ANIMALS.—At the last meeting of the St. Petersburg Society of Naturalists, Prof. Wagner made a communication "On the Individual Variations in Animals, their Causes, and Results." Pointing out that the appearance of new races, varieties, and species is rendered possible by the appearance, at all stages, of the development of life of individual variations, which variations give rise afterwards to more or less constant new forms, the Professor sketched the causes of these individual variations, exterior and interior, insisting especially on the importance of these latter. The causes of variability, he said, are not only the physico-chemical influences of the medium inhabited by the individuals, *i.e.*, the exterior causes, but also, to a very important degree, the interior causes, *i.e.* those subjective physiological, and therefore also psychological, individualities which characterise each individual, and which modify to a considerable extent the influence of exterior influences on each separate representative of the species.

A NEW CHEETAH.—At the meeting of the Zoological Society on Tuesday last, Mr. Sclater described a new species of cheetah, from South Africa, differing from *Felis jubata* in the fact that the whole body is covered with spots of a dark yellow instead of black, and at the same time is considerably more thickly covered with hair. Mr. Sclater proposed the name *Felis lanea* for this apparently new species.

NORTH AMERICAN LEPIDOPTERA.—Mr. William H. Edwards has published a catalogue of the diurnal lepidoptera of North America and Northern Mexico, supplementing the well-known work by Dr. Morris, printed some years ago by the Smithsonian Institution. He enumerates no less than 506 species. This is about equal to that of the previous catalogues, the additional new species being balanced by canceling names which were synonyms or not legitimately entitled to introduction in the North American list. The special object of Mr. Edwards is to bring about what he considers a satisfactory nomenclature, dissenting from the radical changes which he insists Mr. Scudder has made in his recent divisions and lists, in few of which he concurs.

A NEW SHELL.—Mr. C. R. Thatcher, the experienced conchological collector, has just returned to this country after a five years' collecting journey through China, Japan, Philippine Islands, and Australia. He has procured several new species of Murex, Cancellaria, and one wonderful specimen of an entirely new genus. This specimen was described at the meeting of the Zoological Society on Tuesday, June 5, by Mr. George French Angas, by whom it is proposed to give the name Thatcheria, in honour of its discoverer. It was the traveller's particular aim to procure specimens of the rare *Cypræa thatcheri* and *Voluta thatcheri*, both of which he found a few years ago, for which purpose he travelled many hundreds of miles into the interior of Japan, often at the risk of his life.

GEOLOGICAL NOTES

RARE MINERALS IN THE NORTH OF SCOTLAND.—The accidental use of a mass of granite for building purposes near Tongue, in Sutherlandshire, has led to the detection of several rare minerals, and of quite a remarkable number of species and varieties associated in the same mass of rock. From among the fragments of the boulder pieces of a bright green stone were sent to the museum of the Duke of Sutherland by Dr. Joass, of Golspie. These were afterwards analysed by Prof. Heddle, of St. Andrews, and found to be the variety of orthoclase felspar, termed amazonstone. For the purpose of more careful examination as to the mode of occurrence of this uncommon substance, Prof. Heddle has recently visited the locality, which is the side of the ridge rising to the east of the village of Tongue. He found the granite mass to be merely a large boulder, and had it thoroughly broken up. It has yielded the following remarkable assemblage of minerals:—amazonstone in simple and twin crystals, radiated cleavlandite, lepidomelane, pinite, fluorite, sphene, zircon, magnetite, ilmenite, allanite, smoky quartz with peculiar faces, and a mineral which a carefully instituted comparison shows to be thorite passing into orangite. The specimens of amazonstone obtained from the boulder are of unparalleled magnificence. One which has been sent to the museum of the Duke of Sutherland exhibited a surface of some three square feet, about a dozen large crystals, of which eight were unbroken and perfect. One crystal, unavoidedly broken in the extraction, showed the following extraordinary dimensions:—viz., a length of 15½ inches, with a breadth and thickness of ten and eight inches respectively. The minute structure of these crystals is peculiar, and has been fully described in a recent paper by Dr. Heddle on Scottish felspars in the *Transactions* of the Royal Society of Edinburgh. The exceedingly rare thorite was found in only a small quantity. From an examination of the granite of this and other boulders on the same hill, it appears that they have probably come from the huge mass of Ben Laoghal, which lies a few miles inland to the south-west. Should this be their origin, we may expect yet to find new sources of amazonstone, and perhaps other rare minerals among the numerous corries and crags of that picturesque mountain.

TERTIARY LEAF-BEDS OF COLORADO.—Mr. E. L. Berthoud, of the Territorial School of Mines, Golden City, Colorado, sends notes of a section near that place which presents some considerable resemblance to the sections in Antrim and Mull, where the miocene leaf-beds and lignites are associated with sheets of basalt and tuff. The order of succession is as follows:—

Basalt	120 feet
Lignite and leaf-bed	2½ ,,
Hard muddy clay and sandstone	13 ,,
Second leaf-bed	3 ,,
Clay, sandstone, conglomerate	40 ,,
Third small leaf-bed in clay	2 ,,
Sandstone and clay, &c.	30 ,,
Basalt	25 ,,

The resemblance is further borne out by Mr. Berthoud's list of plants, which includes *Platanus aceroides*, *Filicites hebridica*, *Populus arctica*, *Corylus McQuarii*, *Fagus macrophylla*, *Quercus chlorophylla*, *Sequoia*, sp. (?), *Gymnogramma Haydeni*, *Cinnamomum*, n. sp., *Ficus*, 2 sp. nov., *Magnolia*, 2 sp, *Juglans*, 2 sp., *Sabal Campbellii*, *S. Grayana*, and *S. goldianus*, *Myrica*, &c.

INFLUENCE OF ANCHOR-ICE UPON FISHING-GROUNDS.—Prof. Hind, to whose late researches in Labrador we recently called attention, has published some remarks on the effects of the formation of ground-ice in retarding the decomposition of fish-offal, and thereby in seriously damaging the value of the Labrador fishing-grounds. He shows that the ice formed on the sea-bottom freezes the offal, and protects it from being devoured by sea-scavengers and from decomposition; that every rise in temperature which prevents the formation of anchor-ice promotes the decomposition of the offal; that when this takes place, as it does every year under a covering of surface-ice, the water, not being aërated, becomes foul with gases and from the removal of its oxygen, and that the result is fatal to the young cod and other fry which then seek the coasts in search of food. He states that vast multitudes of the young fish are, from this cause, destroyed every summer and autumn in the bays and fjords, and he accounts for changes which have taken place in the migratory movements of seals by this wholesale destruction of the food which they used formerly to find in the coast-waters. He recommends the utilisation of the offal, which would not only eventually prove remunerative as a source of artificial manure, but would remove the poisonous gases which are set free on the melting of the anchor-ice at a time when they cannot fail to prove highly destructive.

ORIGIN OF THE TREES AND SHRUBS IN THE SOUTH OF FRANCE.—In a recent memoir presented to the Academy of Sciences of Montpellier, the veteran professor Charles Martins discusses the history of those trees and shrubs in the south of France which suffer from severe cold, such as the carob-tree, oleander, European palm, myrtle, sweet-bay, pomegranate, olive, fig, laurustinus, ilex, vine, and others. He shows that most of these occur among the tertiary and quaternary deposits, that some of them, indeed, like the oleander (*Nerium oleander*), go back even into eocene times. He points to the fact that their remains occur in the geological formations, not only of the countries where the plants are still living, but even of tracts considerably further to the north, both in France and in Switzerland, where their living descendants or analogues could not endure the severity of winter now. The tender trees and shrubs of the Mediterranean seaboard thus serve to prove the former warmer climate of France and its subsequent refrigeration. They are merely the surviving relics of a tertiary vegetation preserved by the exceptional mildness of the climate in which they grow. A single winter of exceptional rigour, or even a single night of extreme cold, like that of January 13, 1826, when the thermometer fell to 9°·7 below zero (Cent.), would suffice to destroy them. It may be presumed, however, that during at least the height of the glacial period these tender plants were driven southwards beyond their present northern limits, and that they have subsequently crept north again.

U.S. NATIONAL ACADEMY OF SCIENCES

ACCORDING to the terms of its charter from Congress, the National Academy of Sciences must hold its annual meeting in April, at Washington. It holds also a semi-annual meeting in the autumn. Its membership has been very slowly increasing, till now it numbers nearly, if not quite, 100. At the last meeting, April 17-20, Prof. Henry presided. The Academy resolved to present a memorial to Congress, in favour of the establishment and maintenance of an International Bureau of Weights and Measures with the object of promoting permanence, precision, and uniformity in the standards, by the joint action of the leading powers of the world, according to the convention submitted to the Senate.

Five new members were elected:—Prof. John W. Draper and Dr. Henry Draper of New York, Dr. Elliot Coues of Washington, Dr. S. H. Scudder of Cambridge, Mass., and Mr. Charles S. Peirce of the U.S. Coast Survey.

The annual report of the president, Prof. Henry, recounts briefly the year's work of the Academy. The Academy reports progress in the work of preparing and publishing the scientific results saved from the wreck of the *Polaris* and in general contributed by the expedition in which that vessel was engaged. This work is in the hands of Dr. Emil Bessels, the scientific director of the expedition, and will be finished in three quarto volumes. The first volume is already published; it is a quarto of 960 pages relating to astronomy, pendulum experiments, winds, solar radiation, and meteorology in general. It is illustrated by fourteen plates, two maps, and forty woodcuts; only 500 copies of this volume were printed. The second and third volumes relate to geology, palæontology, mineralogy, botany, zoology, and ethnology. They will include a monograph on the Eskimo, illustrated by 100 plates and 200 woodcuts. The Academy has divided the income from the Bache fund, so as to cover several distinct researches, as follows :—(1) On sun-spots and chromosphere, conducted by the late Prof. Winlock; the results are published, with plates, in the transactions of the Harvard Observatory. (2) Magnetic survey of the United States, in charge of Prof. J. E. Hilgard; during the year twenty-five new stations in New England and Lower Canada have been occupied; at all the stations the dip, declination, and horizontal intensity are observed. (3) Comparisons of sensations of light; in charge of Mr. Charles S. Peirce. The object is especially to ascertain the mathematical formula connecting the capacity of the eye for light sensations with the physical variations of radiation; two sensations are compared, the one fixed, the other variable; part of the results have been published. (4) Researches on the distribution of heat on the solar surface, the laws of its radiation and absorption, and effects on terrestrial climate; conducted by Prof. S. P. Langley. (5) Researches on the laws of sound and the duration of vibrations of tuning-forks; conducted by Prof. A. M. Mayer. The work of utilising the observations made on the transit of Venus is under direction of a committee of which the president of the Academy is a member; progress has been made in this work, but it is scarcely yet ready for report. The endeavour to obtain an appropriation for a permanent building to display the scientific and other material contributed by the Government of the United States and other nations during the Centennial Exhibition, failed to obtain a two-thirds vote in the House of Representatives after passing the Senate without dissent. The effort will be renewed at the next session of Congress. The collections are large, valuable, and instructive, including the costly gifts of other nations, and the entire exhibit that was in the "Government Building" at Philadelphia.

During the meeting the members of the Academy were formally invited to visit the President of the United States at the White House. They were duly presented, and President Hayes expressed himself in a brief speech as fully appreciating the value of scientific pursuits, and willing and desirous to advance the interests of science. The reception was notably pleasant and cordial.

We give abstracts of the more important communications :—

Prof. Alexander Agassiz gave a brief notice of researches on the young stages of some osseous fishes. The history of previous researches on these points, since those of von Baer at the beginning of this century, was reviewed. Prof. Agassiz concludes that with few exceptions the tail begins to be formed below the dorsal cord. If embryos and very young animals are examined, a lobe is found much developed in some and common to all. The tail fin and the anal fin are probably modifications of the same organ. There is a general uniformity in the plan of construction of the tails of fishes whether osseous or otherwise.

Under the title of "Some Results of Deep-sea Dredging," Prof. Agassiz stated views partly founded upon his general knowledge and study of the products and observations made during the *Challenger* expedition, and partly upon information obtained in conversations with Prof. Sir C. Wyville Thomson and other members of the expedition. The fact of a point of zero temperature being in all oceans but varying in depth with latitudes, had been indicated by previous expeditions and was fully established by the *Challenger* observations. At the equator a depth of 500 fathoms is needed to reach this zero line; as we approach the poles the depth of this line decreases till at last it is at the surface. Sometimes the temperature of the lower water is 2° or 3° below zero, but the conditions are, in general, uniform. Equally uniform is the fauna below this line. Above it, the arctic, temperate, and tropical faunas are clearly distinguishable from each other. There is a remarkable uniformity among the animals of the tropical fauna, such as may have resulted if in a previous era the isthmuses of Darien and Suez were absent, the Sahara was covered by the sea, and an equatorial current swept freely around the world. The deep-sea fauna is so singularly like the cretaceous that its forms would have been at once assigned to that epoch by most palæontologists if they had been fossil. The similarity if not identity of these forms indicates that there has been scarcely any change since that era. This is true of echinoderms, worms, and even of some fishes. It is equally true of some shore animals found both off our coasts and in the chalk. These have been subjected to the most varied conditions of existence as compared with their ancestors, and yet have not altered. There is evidence that natural selection, even under conditions where its forces are extreme, may not bring about any change. The present continents are probably much older than has been supposed. There is an entire want of evidence that great continents existed where oceans now are. The shore mud from our continents is washed down comparatively only a few miles from shore; the depths are not reached by this mud. Other well-known theories need to be modified. At a depth of 2,500 feet crustacea are found having good eyes. These organs have undergone no change during innumerable centuries. There are similar facts established as to starfish. There is no very great number of blind animals in the ocean depths. Those that are blind need not be classed as retrogressions from ancestors that had eyes. Both as to blind animals in the sea and those found in caves, it seems most probable that they were the descendants of eyeless ancestors. In the discussion which followed this communication, Prof. Agassiz said that he had long doubted the theory of geologists respecting an immense miocene continent.

Prof. Joseph Le Conte, of Oakland, California, furnished a paper on critical periods in the history of the earth and their relation to evolution; and on the quaternary as such a period. This paper instanced and enlarged upon the breaks in the geological and palæontological records, and argued that a more rapid rate of evolution had been operative during the intervals, which he designated as "critical periods." The quaternary era he regarded as one of these critical periods during which rapid changes had taken place, but it differed from most of such periods in the fact of its records being preserved.

Prof. G. K. Gilbert described the characteristics and mode of formation of the Henry Mountains.

Dr. F. V. Hayden described the results of boring artesian wells in a locality near Rawlins Springs in Wyoming Territory on the line of the Union Pacific Railroad. The district is on the dividing line of the watershed of the continent, some of the streams on the sides of the district flowing to the Atlantic, and some to the Pacific oceans. The rainfall of the district is very small—not over six to ten inches per year. The wells were bored to depths varying from 300 to over 1,000 feet. The water obtained was from 1,000 to 2,000 gallons per hour; it was lifted by pumps driven by windmills. The result showed the feasibility of thus irrigating very arid regions. The wells were bored under Dr. Hayden's direction, in a sinclinal basin which he regarded as of tertiary formation, probably eocene; but palæontologists who had examined its fossils had pronounced them cretaceous. Dr. Hayden regarded this as an instance where the chasm between the cretaceous and tertiary rocks was bridged over; he stated that the rocks were consecutive from cretaceous to middle tertiary. This opinion was not fully shared by the other geologists present, and the discussion which followed became exceedingly animated, as it brought up questions long at issue between the respective students of the rocks and of the fossils of that region.

Major J. W. Powell presented some remarkable facts respecting the public domain of the United States. Dividing the United States into two portions, the humid, where the rainfall is sufficient for agriculture, and the arid, where it is not, the latter is found nearly to equal the former.

Prof. Elias Loomis, of Yale College, contributed his seventh paper of a series entitled "Contributions to Meteorology," this paper continuing the investigation of rain areas which was begun in the last. Prof. Loomis selected all the cases in the United States during fifteen months—September 1872, to November 1873—in which the reported rainfall amounted to at least eight inches in eight hours. For each of these cases the curves of equal rainfall were drawn on the map and compared. The form of these curves, though occasionally irregular, in general approximates an ellipse, of which the major axis is not quite double the minor. In these cases the area of one-inch rainfall exceeded in length 500 miles; in six cases of one-half-inch rainfall 750 miles; frequently the entire area is an oval of more than 1,000 miles length and exceeding 500 miles breadth. In general the rain area centre is east of the low pressure centre, but in several instances the reverse was the fact, and in some cases the rainfall appeared to have had decided influence on the storm's progress, as to its direction. It was concluded that rainfall is not essential to low barometer areas, nor the chief cause of their formation or progressive motion. Such areas result from a general atmospheric movement toward a central area, and may be caused by unequal barometric pressure, unequal temperature, or unequal amounts of aqueous vapour. The two last-named causes are not comparable to the first of the three in cogency, and only deflect the winds slightly. The progress of areas of low barometer in all latitudes is mainly determined by the same causes as those which determine the general atmospheric circulation; their normal direction is changed by whatever causes may change the direction of the winds.

Prof. William Ferrel, of Washington, delivered a communication on the progressive movements of storms; the object being to show that the movement of great storms is determined by the currents—especially the upper currents—of the atmosphere.

Prof. Pickering presented the results of an investigation made in connection with Prof. W. A. Rogers on systematic errors in star declination. A comparison with the mean both of the earlier and later catalogues rendered probable the existence of systematic errors in the Gesellschaft catalogue.

Prof. Simon Newcomb presented a communication on the secular acceleration of the moon and its increasing deviation from uniformity through many years. He reviewed the existing theory on the subject; the calculation of Laplace according with Halley's estimate of the acceleration as about 10½ seconds of time, to be multiplied by the square of the centuries for a given period; also the Adams theory, which reduces the explanation of Laplace to about 6 seconds, leaving more than 4 seconds to be otherwise accounted for. In ascribing the surplus acceleration to diminished rotation of the earth, we are dealing with a subject where the evidence should be carefully weighed. Much dependence seemed to be placed on the records of ancient eclipses. Prof. Newcomb considered these eclipses separately. The most promising of the Greek solar eclipses was that of Agathocles, tyrant of Syracuse, occurring at the commencement of his voyage to attack Carthage. But we do not know on which side of Sicily he sailed; according to whether he was on one or the other side of the coast, the difference of time for that eclipse may be calculated as justifying the 10 seconds or the 6 seconds acceleration of the moon. The eclipse known as that of Thales has a record open still more to criticism, because it came to its historian by hearsay, and probably through two or three generations after the lapse of a hundred years. It seems curious that if Thales predicted the year (by an estimate of lunar periods) he did not also predict the day. Each of the ancient solar eclipses yielded similar elements of doubt on careful examination. From the records of lunar eclipses if all uncertain features be weeded out, the old estimate of acceleration will be reduced one half. The Arabian records of lunar eclipses were published at Leyden in the early part of this century. The work is very rare. Altitudes of sun and moon are constantly given in it. Calculations from these eclipses give the smaller estimate of acceleration. From all the data he has been able to study Prof. Newcomb concludes that the whole amount of acceleration is about 8·4 seconds. He hopes to make further estimates from modern records, having had the good fortune to pick up in Paris carefully compiled data of occultations going back to 1680.

In introducing his communication on "a Proposed New Method in Spectrum Analysis," Prof. S. P. Langley, of Allegheny Observatory, said that in giving this title to his description of his method, he believed, and, so far as he could ascertain, was justified in believing, that the method in this special application of it, was quite new. The process consists not only in placing in juxtaposition, simultaneously, the spectra of light from two opposite edges of the sun's disc (which had been done before), but also in determining that when these spectra are taken respectively from east and west edges, the atmospheric spectrum lines still coincide, while the solar lines of the two spectra do not coincide. Prof. Langley was anxious to disclaim any intention to abate one jot of the praise due to Prof. C. A. Young for conclusively demonstrating that the difference of wave-lengths from the east and west edges of the sun can be measured and its rotation thereby be proved. The history of this line of discovery was briefly given. Zöllner, Secchi, and Hastings thought they had perceived a change in the refrangibility of the light, and Vogel, using Zöllner's reversion spectroscope, obtained a displacement of from ·08 to 0·15 of one of Angström's units. Finally, Prof. Young, using a Rutherfurd grating, showed a velocity of the sun's equator of 1m. 42s.; and also that independent measurements of solar and atmospheric lines gave different results for these two classes. Prof. Langley's new method has the advantage of great security against instrumental errors, since the two classes of lines under like instrumental conditions, betray their diverse origin. In 1875, while studying the selective absorption of the solar atmosphere, Prof. Langley constructed an apparatus for comparing homogeneous light from different parts of the solar disc; use being made of two pairs of prisms of total reflection, connected with a spectroscope so as to give spectra from different parts of disc side by side. A photometric apparatus was attached to compare the relative intensities of light in different parts of these spectra. The whole apparatus was not intended at first for the comparison of individual lines of the spectrum, a purpose for which somewhat similar arrangements had been used by Lockyer, Hastings, and perhaps others; but Prof. Young's success suggested to Prof. Langley another and cognate method of using the principle of Doppler, to which this apparatus is well adapted. For six months Prof. Langley has been engaged in overcoming the instrumental difficulties of this conception. Only within a few days has he been able to produce complete results. When the apparatus is pointed so as to receive the light from the north and south poles of the sun, the lines are continuous in the two spectra; but when the instrument is rotated so as to take light from the east and west sides, all the solar lines are found discontinuous at the junction of the spectra, while the atmospheric lines remain continuous. If the instrument is rotated 180° the solar lines again appear discontinuous, but the spectrum whose solar lines were before shifted to the right as compared with the other, will after such rotation show them shifted to the left. In order to keep clear of any bias of judgment, Prof. Langley was careful not to know beforehand which way the instrument was pointed; but the displacement in every observation tallied with the theory. Essential aid was given in the construction of the instrument by the use of the choicest of glass gratings, ruled 8,600 to 17,200 lines to the inch, which Mr. Rutherfurd, of New York, sent for the purpose of this investigation. In the higher spectra of these admirable gratings thirty-one lines are discriminated in the E group where Angström and Kirchhoff have a dozen. On actual comparison for the fine lines of that group more have been counted with the grating than with the most powerful spectroscopes consisting of trains of twelve or more prisms. The method of analysis by Prof. Langley's instrument seems less adapted to quantitative work than Prof. Young's, but in this respect it is hoped also to make it useful by employing the micrometer upon the double displacement obtainable in right and left hand spectra of the same order presented simultaneously and in combination. By the observed displacement or fixity of any line we can now discriminate certainly as to its solar or telluric origin. It is hoped that a ready means of mapping atmospheric lines will thus be afforded, since indeed they are already mapped by this process before the eye of the observer.

Gen. J. G. Barnard, U.S.A., contributed a mathematical essay, also in part historical, on the internal structure of the earth as affecting the phenomena of precession and nutation, supplementary to an article under this head in vol. xix. of the "Smithsonian Contributions to Science," being the third of the "Problems of Rotary Motion." The paper shows that Gen. Barnard has coincided in Sir William Thomson's change of view. The formation of a diurnal tide in the fluid earth is called in doubt by this paper. In general it presents work of the kind that Sir William Thomson was longing for in his Glasgow address—a solution, coherently worked out, of the problem above indicated.

Prof. O. N. Rood, of Columbia College, New York, contributed two papers giving details of his researches concerning colours. Prof. Rood used a set of brilliantly coloured circular discs representing the chief spectrum colours, and also purple. By combining in successive proportions with these colours, a white disc, and giving the combined discs rapid rotation, the following results were attained: the lighter shades of vermilion became purplish; of orange, more red; of yellow, more orange; of greenish yellow, unchanged; of yellowish green, more green; of green, blue; of cyanogen blue, less greenish and more bluish; of cobalt blue, a more violet blue; of ultramarine, violet; of violet, unchanged; of purple, less red and more violet. Exactly similar effects were produced when violet instead of white was used to reduce the colours. Hence the mixture with white is the same as if the colours were moved towards the violet end of the spectrum. Prof. Rood thinks his results tend to indicate violet as one of the primary colours, which cannot be said of Maxwell's third fundamental colour, an artificial ultramarine, or Bezold's, a blue violet, careful tests of those colours having been fully carried out. The foregoing results were laid before Mr. Charles S. Peirce. He has reported at considerable length on the mathematical principles involved. He regards the results as in accordance with Fechner's law, that the sensation is proportional to the logarithm of the excitation. When the objective brilliancy of any light is varied, the specific subjective brightness is not changed in the least; but the only effect on the sensation is to add to, or subtract from it a variable amount of a certain constant sensation, which Mr. Peirce designates as the "colour of brightness." This ceases to be true when negative logarithms are involved. The yellow of the spectrum comes very near the colour of brightness. From these considerations a diagram has been constructed showing the colorific effects of mixing white with any part of the spectrum. The results of theory as shown by the diagram, closely accord with those of experiment upon the sensation of colour.

Prof. Rood also presented a paper on Newton's use of the term "Indigo" as a prismatic colour. It was intended to indicate the range of the spectrum between the blue and the blue-violet regions. The order of refrangibility is thus stated: prussian-blue and indigo; cobalt blue; genuine ultramarine blue; artificial ultramarine blue.

Prof. Joseph Le Conte, of California, sent a communication on the structure of the crystalline lens and its relations to periscopism. The discovery of Dr. Hermann, and his deductions therefrom, were first considered. These are that the crystalline lens, by its structure, is endowed with the property of forming distinct images of objects though lying on the extreme margins of the field of view, of forming perfect images on the retinal screen, even to the extreme anterior margins. Thus the eye has an enormous field of view compared with optical instruments. The purpose of the structure is to give periscopism to the eye. Prof. Le Conte believes, however, that as far as periscopism is concerned this structure is of little if any value in man for want of a corresponding suitable retinal structure. The indistinctness of the retinal image is different from the indistinctness of an imperfect perception of the image, the former being due to the properties of the lens, the latter to the organisation of the retina. In proportion as we go upward in the scale of animal life we find the powers of the central spot of the optical apparatus more thoroughly developed for the purposes of binocular vision.

Prof. A. Mayer, of Stevens Institute, presented four communications. He described a "Vernier microscope," which he believes to be new. The object is to substitute an accurate and permanent scale cut on glass for the varying errors of a micrometer screw. The instrument is of small cost and its errors are not varying. On a glass plate a series of lines is cut in tenths of millimetres; the central millimetre is divided into ten parts. This scale slides in carefully wrought guides in front of the objective of a firmly fixed microscope. In the focus is another scale so adjusted that ten of its parts accurately subtend the image of $\frac{9}{100}$ths of the millimetre scale. Thus a Vernier is formed which reads down to the $\frac{1}{1000}$th of a millimetre. The glass slide is so shaped that its rounded conical end abuts against the object to be measured. Readings to the full capacity of the instrument can be quickly obtained.

Prof. Mayer described his apparatus for measuring the expansion of metals and alloys under differences of temperature. It is believed that the coefficients of expansion, now inaccurately known, will be more correctly ascertained by this research.

The vibrations of tuning-forks received further investigation by Prof. Mayer; the cost of inquiry was defrayed by the Bache fund. The probable error in these determinations is the $\frac{1}{100}$ of one vibration, *i.e.*, with 256 vibrations to a second the probable error is $\frac{1}{25600}$ of a second. Differences in amplitude of vibration make no difference in the vibratory period of the fork; pressure applied to the fork also has no effect on the vibratory period, though it shortens the continuance of the note.

Prof. Mayer also described his investigations into the distribution of magnetism in long bar magnets. Some of these bars which were tested were five feet in length. Various methods have been tried for ascertaining the facts of magnetic distribution; Prof. Mayer gave due credit to other workers in this field, and described their experiments.

The Academy will hold its semi-annual meeting next autumn at New York.

NOTES

THE following is a list of the officers of the forty-seventh annual meeting of the British Association which will, as we have intimated, commence at Plymouth on Wednesday, August 15:—President Elect—Prof. Allen Thomson, M.D., LL.D., F.R.S., F.R.S.E. Vice-Presidents Elect—The Right Hon. the Earl of Mount-Edgcumbe, D.C.L., the Right Hon. Lord Blachford, K.C.M.G., Dr. William Spottiswoode, F.R.S., Dr. William Froude, C.E., F.R.S., Mr. Charles Spence Bate, F.R.S. General Secretaries—Capt. Douglas Galton, C.B., F.R.S., Dr. Philip Lutley Sclater, F.R.S. Assistant General Secretary—George Griffith, M.A., F.C.S. General Treasurer—Prof. A. W. Williamson, F.R.S. Local Secretaries—Messrs. William Adams, William Square, F.R.C.S., Hamilton Whitefore. Local Treasurer—Mr. Francis Hicks. The Presidents of the Sections are as follow:—Section A: Mathematical and Physical Science—President, Prof. G. C. Foster, F.R.S. Section B: Chemical Science—President, F. A. Abel, F.R.S. Section C: Geology—President, W. Pengelly, F.R.S. Section D: Biology—President, J. Gwyn Jeffreys, F.R.S., F.L.S. Department of Zoology and Botany, J. Gwyn Jeffreys, F.R.S., F.L.S. (President), will preside. Department of Anatomy and Physiology, Prof. Macalister, M.D. (Vice-President), will preside. Department of Anthropology, Sir Walter Elliot, K.C.S.I., F.L.S. (Vice-President), will preside. Section E: Geography—President, Admiral Ommanney, F.R.S., F.R.G.S. Section F: Economic Science and Statistics—President, the Right Hon. the Earl of Fortescue. Section G: Mechanical Science—President, Edward Woods, C.E. The reception room will be opened on Monday, August 13, at 1 P.M., and on the following days at 8 A.M., for the issue of tickets to members, associates, and ladies, and for supplying information. No tickets will be issued after 6 P.M. The first general meeting will be held on Wednesday, August 15, at 8 P.M., when Prof. Andrews, F.R.S., will resign the chair, and Prof. Allen Thomson, F.R.S., President Elect, will assume the Presidency, and deliver an Address. On Thursday evening, August 16, at 8 A.M., a *soirée;* on Friday evening, August 17, at 8.30 P.M., a Discourse by Prof. Warington Smyth, M.A., F.R.S., on the Physical Phenomena connected with the Mines of Cornwall and Devon; on Monday evening, August 20, at 8.30 P.M., a Discourse, but by whom not yet arranged; on Tuesday evening, August 21, at 8 P.M, a *soirée;* on Wednesday, August 22, the Concluding General Meeting will be held at 2.30 P.M. The local arrangements for the Plymouth meeting are not yet matured, but we believe they will include an exhibition of paintings chiefly by artists of Devon and Cornwall, including magnificent examples of Reynolds, Opie, Eastlake, Northcote, Cooke, Prout, &c. There will be excursions to Torquay or Kent's Hole, &c., and the Dart; up the Tamar or Cotehole; to the Moss Clayworks and over Dartmoor; to the Eddystone Breakwater and Government establishments; to ~~smoked glass.~~the Caradoc mines, the Land's End, &c.

A RECEPTION was held by the President of the Royal Society and Lady Hooker at Burlington House, on Wednesday evening June 13, which was largely attended. The invitations included ladies as well as men of science. The rooms were decorated with plants, and there was a collection of instruments and objects of scientific interest. Among the novelties were new spectroscopic instruments exhibited by Mr. Browning and Mr. Hilger; and Messrs. Tisley and Spiller's harmonograph curves, drawn on smoked glass, two of which held together against a light give some of the effects of polarised light.

A CONGRESS on Domestic Economy, organised by the Society of Arts, is to be held in Birmingham on July 18 and 19. Section A is to include (1) Needlework; (2) Cleanliness; (3) Food and Cookery; (4) Household Expenditure; (5) Thrift. Section B (6), Health; (7) Sickness; (8) The Dwelling; (9) Warming and Ventilation. Section C (10), Teaching the Subjects in Elementary Schools; (11) Text-books; (12) Inspection and Government Grants; (13) Importance of Female Inspectors; (14) Examinations. A number of papers are already promised, among them being papers by Mrs. W. E. Gladstone and Prof. Huxley. The Local Committee includes the Lords-Lieutenant of Staffordshire, Warwickshire, and Worcestershire, the Bishop of Worcester, several noblemen, clergymen, the Mayor of Birmingham, &c.

THE Helvetic Society of Natural Sciences meets this year at Bex, in Canton Vaud, from August 19 to 27. Several interesting excursions have already been arranged for. Prof. Louis Dufour, of Lausanne, will be president. English naturalists will be heartily welcomed.

THE death is announced of Lieut.-Gen. Sir Henry James, director of the Ordnance Survey of Great Britain from 1854, until his appointment to the command of a battalion of Royal Engineers in 1874, since which he has lived in Southampton in failing health. He was seventy-four years old, and from 1844, when he was director of the Geological Survey in Ireland, had written on various scientific subjects.

SOME bones of *Lithornis emuinus*, an enormous bird of the eocene period, have just been discovered in the London clay at Sheppy by Mr. W. H. Shrubsole, of Sheerness-on-Sea. Casts of these fossils will be taken for the British Museum.

THE well-known traveller, Dr. Schweinfurth, has recently returned to Egypt from a two months' journey through the Arabian desert, richly laden with scientific collections. He proceeds to Berlin to complete the arrangement of the large quantities of botanical specimens collected by him in his late expeditions.

No. 4 of the *Quarterly Bulletin* of the Cairo Society of Geography contains a valuable account, by Dr. Nachtigal, of his visit to Wadai, as the country between Darfur and Bagirmi is called, and notes on the country of Harrar, by Mohammed Maktar. The same Society has published separately an obituary notice of the late Marquis de Compiègne, by M. C. Guillemine.

A VALUABLE and somewhat elaborate geographical sketch of Loango and the Loango Coast, by Dr. Pechuel Lösche, has been published in a separate form from the *Mittheilungen* of the Leipzig Geographical Society.

THE Annual Report for 1876-7, of the West London Scientific Association, speaks favourably of the progress of that Association, which now numbers 186 members.

IN the Buenos Ayres *Standard* of May 13 D. Francisco Moreno describes a journey he made up the Santa Cruz river, in Southern Patagonia, in about the 50th deg. of S. lat. Notwithstanding the great rapidity of the current, he succeeded, with three sailors, in ascending the river, taking thirty days to it. The Santa Cruz issues from a fine lake thirty miles long and ten broad, in S. lat. 50° 14′ 22″ and 71° 59′ W. long. D. Moreno was the first to sail along this lake, which he explored and sketched pretty thoroughly, making considerable geological collections in the neighbourhood both of this and of other lakes in the same region. Amongst these was Lake Biedma, in the neighbourhood of the still active volcano, Chalten. A river more than 200 yards wide connects Lake Biedma with Lake Santa Cruz.

MR. G. BROWN GOODE, assistant curator of the National Museum at Washington, has been engaged during the past winter in investigating the natural history of the Bermudas, and has recently returned with a large collection, filling twelve barrels and forty-three boxes, and including over 1,000 bottles of invertebratas in alcohol. His collections embrace the entire marine fauna of the coast—fishes, molluscs, worms, &c., many of which are believed to be entirely new to science.

MR. ROBERT LOWE, M.P., presided on Wednesday, last week, at a meeting of the committee formed for the purpose of organising a testimonial to Mr. John Simon, F.R.S., late medical officer to the Privy Council and Local Government Board, in recognition of the long and valuable labours he has rendered to the State, and of his eminent services to sanitary science. It is proposed that the testimonial shall assume the form of a bust in marble of Mr. Simon for presentation to the Royal College of Surgeons, agreeably to the wishes of the Council of that body. The cost, together with the expenses, will probably amount to 500*l*. We are sure that not only members of the medical profession, but all who are interested in sanitary science, will willingly contribute to a monument to one who has done so much for that science.

A SPECIAL direction of science and arts has been added to the French Ministry of Public Instruction. M. Walteville has been appointed to the new office, and it is supposed that having nothing to do with politics, he may continue in office irrespective of any change in the Ministry. It would be well to insure a continuity of action and professional independence to the head of so useful an administration.

A NEW photographic department has been established in France for the reproduction of scientific or artistic objects. The laboratory will be kept up exclusively by Government. The State photograpers will be required to employ the most advanced methods and to work for the improvement of the art.

ON Friday night a series of interesting experiments with the Jablochkoff electric light took place at the West India Docks, under the direction of M. Denayrouze. The apparatus used for the occasion consisted merely of an electro-magnetic machine worked by a small steam-engine, some insulated wires, and the electric candles, which are the invention of M. Jablochkoff, and composed, as we have already described, of two carbons placed side by side with a slip of insulating substance between them, which burns away with the carbon exactly in the same way as the wax of a wax candle is consumed with the wick. The first experiment in order to show the suitableness of the invention for dock purposes consisted in the lighting of four of the "candles" in a large yard. The light thus obtained, which was shaded by ground glass, brilliantly illuminated the inclosure, it being possible to read small print at a considerable distance from the lights, while at the same time the eyes were not affected by the glare, as is the case with the ordinary electric light. The second experiment was confined to the illumination of the top story of one of the large warehouses, and this, like its predecessor, was equally successful. A large vessel at the quay side was also lighted up, as also was a portion of the quay. The whole of the experiments were very successful, and it was stated that each "candle" gave a light equal to 100 gas lights. The light is said to be much less expensive than gas.

THE Leicester Literary and Philosophical Society have commenced the publication of its *Transactions* from its foundation in 1835, as far as material for these can now be obtained. We have received the first two parts of this publication, extending from 1835 to 1841; they contain much likely to interest not only the members of the Leicester Society, but all who take an interest in the progress of local societies, now becoming so widespread and efficient.

SOME experimental researches on the light refraction of a number of gases are described by M. Mascart in the *Annales Scientifiques*. A beam of light was sent through a collimator to two plates of plate-glass connected together at right angles; the halves of the beam were bent right and left by refraction through the glass. They then went parallel through two copper tubes containing the gases, and after refraction by a second system of glass plates placed in reverse directions, the halves were united again, and the beam passed through a slit to a system of prisms, then to a telescope. If the pressure in one copper tube were as varied, the phases of the two parts of the beam were unlike, and from the number of fringes, the refraction of the gas could be determined. The influence of pressure was examined, then the refractive power for different wave-lengths, then the influence of temperature; and the absolute refractive power deduced from the various factors obtained. The numbers for the latter range from 0·1387 (hydrogen), and 0·2706 (oxygen) to 0·7036 (sulphurous acid), and 0·8216 (cyanogen). The refraction of a gas mixture is equal to the sum of the refractions of the mixed gases. But the refraction of a compound is in general greater than that of a mixture of the simple gases composing it.

As determinations of longitude increase an ever-increasing number of control determinations are obtained. The number of the latter in Germany and Austria is now such that M. Albrecht has considered an attempt at equalisation of the system might prove advantageous, as at least an opinion might be formed regarding the degree of accuracy of the differences of longitude directly measured, and attention would be called to the weak parts of the system. He has accordingly, with M. Sadebeck, attempted an equalisation of the system of longitude determinations between the following stations:—Strassburg, Paris, Mannheim, Bonn, Leiden, Göttingen, Brocken, Leipzic, Berlin, Vienna, Munich, and Bregenz. An account of the investigation appears in *Astronomische Nachrichten*, No. 2,132. A numerical value is assigned to the various determinations, which extend over the last fifteen years; and this was necessarily, of course, somewhat arbitrary in character. In a table M. Albrecht gives for each pair of places the difference of longitude as calculated and the difference observed, and then the difference between the two. The greatest improvements are obtained in the determinations for Leipzig-Vienna and Berlin-Vienna (the difference for the former being + 0·136s., and for the latter 0·102s.) In the former case, there was some uncertainty as to the personal equation, and in the latter two weak currents had been operated with. The improvements are, of course, only approximate, and the certainty of the individual improvements obtained is considerable only when numerous control-determinations are to hand. The result sufficiently shows that a very large number of control-determinations, and an extensive establishment of the system is necessary to remove all doubt with regard to the relative position of the various stations.

As an illustration of the rapid growth of the now celebrated *Eucalyptus globulus*, we may mention that in the more elevated parts of Jamaica trees now exist about sixty feet high, the trunks of which measure a foot in diameter near the ground. These trees have been raised from seed introduced to the island about six years ago. It is proved that in the lowland districts the tree does not thrive, thus upsetting its suitability for regions in which it was at one time specially advocated.

THE new *Journal of Forestry*, the first number of which appeared on May 1, seems to have made a good start, judging from the contents of the two numbers that have now been issued. The contents are sufficiently varied to make the journal welcome to all in any way interested in forests or forest produce, both practically and scientifically, for we find not only articles on forest work for the month, but also a brief *résumé* of Mr. Thiselton Dyer's recent address on "Plant Growth" at the London Institution.

THE additions to the Zoological Society's Gardens during the past week include two Condor Vultures (*Sarcorhamphus gryphus*), a Chilian Sea Eagle (*Geranoaëtus melanolencus*) from South America, presented by Mr. John T. North; two Chaus Cats (*Felis chaus*) from North Africa, presented by Capt. W. Renney; a Crested Guan (*Penelope cristata*) from South America, presented by Mr. Daniel Miron; a Green-winged Trumpeter (*Psophia viridis*) from Brazil, a Common Trumpeter (*Psophia crepitans*), a Demeraran Cock of the Rock (*Rupicola crocea*) from Demerara, a Black-necked Stilt (*Himantopus nigricollis*), a Sun Bittern (*Eurypyga helias*), two Orinoco Geese (*Chenalopex jubata*), a Capybara (*Hydrochærus capybara*) from South America, a Moor Monkey (*Semnopithecus maurus*) from Java, purchased; six Chilian Pintails (*Dafila spinicauda*), seven Summer Ducks (*Aix sponsa*), bred in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—An examination will begin on October 2 for the purpose of electing to a Physical Science Postmastership at Merton College. The postmastership is of the annual value of 80*l*. for five years, to be raised after two years, at the recommendation of the tutors, to 100*l*. The subjects of examination will be chemistry and physics; there will be a practical examination in chemistry, and candidates will have opportunities of giving evidence of a knowledge of biology. Further information from the tutor in physical science.

There will be an examination on October 11 for electing to a Natural Science Scholarship at Exeter College. The scholarship is of the annual value of 80*l*. for four years, without any limit of age. The examination will be in biology, chemistry, and physics, and candidates will be expected to show proficiency in at least two of these subjects. The examination will be to a large extent practical, but special weight will be given to a knowledge of general principles. Further information may be obtained from the Natural Science Lecturer, Prof. E. Ray Lankester.

Mr. M. J. Jackson, of University College, London, has been elected to the vacant (Holmes) Scholarship in Natural Sciences at St. John's College. The scholarship is of the annual value of 100*l*. for five years.

MANCHESTER.—Prof. Boyd Dawkins, F.R.S., has just completed his course of Field Lectures on Geology, at Owens College. Upwards of forty students—the large majority of whom were other than regular students of the College—availed themselves this year of the opportunity offered for acquiring some practical knowledge of geology. Excursions were made to the mountain limestone of Derbyshire, the coal measures near Oldham, the Permian rocks of Alderley Edge, where the copper veins disseminated throughout the sandstone were studied, and where traces of prehistoric man, in the shape of a few flint implements, were discovered. During Whit week Oxford was visited, and the Oolitic beds of the neighbourhood were explored.

During the session which has just closed, 100 students have worked in the chemical laboratories of Owens College, while the number of students attending the various courses of chemical lectures has amounted to about 200. Over a dozen original communications have proceeded from the chemical department during the session.

TAUNTON COLLEGE SCHOOL.—An address of sympathy with, and confidence in, the Rev. W. Tuckwell, head-master of the Taunton College School, is published in the local and educational papers, with the signatures of nearly all the parents. It appears that the school is heavily in debt, and that the numbers, chiefly through an attack of fever, have fallen below the paying point. The panic-stricken officials have selected the head-master as a scapegoat, attribute the loss in numbers to his "unpopularity," and are endeavouring to drive him to resign, a movement against which the parents protest in very animated terms. The part taken by this school and its head-master in working out and popularising the systematic teaching of science in company with the old-fashioned classical curriculum impels us to record these facts, and to look with interest for the result of this latest struggle between Philistinism and culture.

UNIVERSITY COLLEGE OF WALES.—A Welsh gentleman engaged in business in London, in addition to sums of 250*l*., 2,500*l*., and 1,100*l*. (the last sum in conjunction with his brother) previously subscribed, has just placed in the hands of the Council of the University College of Wales, a sum of money to be used in promoting scientific agriculture in Wales. According to a circular just issued, "one of the means proposed to be adopted in furtherance of this object is the delivery of courses of lectures free of charge to persons engaged in tuition in Wales, whereby they may be qualified for giving elementary instruction in the principles of agriculture in their several schools." Prof. Henry Tanner, M.R.A.C., examiner for the section under the Government Department of Science will, on August 7 next, begin a course of twenty lectures, to be continued from day to day, at the College in Aberystwith.

THE UNIVERSITIES' BILL was read a third time in the House of Commons and passed on Monday, and a first time in the Upper House on Tuesday.

ADELAIDE.—We have received a copy of the *Calendar* of Adelaide University for 1877. This University has at present only four professors, who represent very fairly the main branches of literature and science. There is only one professor for Mathematics and Natural Philosophy, and the professor of Natural Science gives instruction in Chemistry, Geology, and Botany. We hope the University will soon be able to carry the principle of sub-division of labour into these two professorships, and thus promote efficient teaching, and at the same time relieve these two professors of a burden they ought not to be made to bear in this advanced age. There are some points in which our home universities might advantageously imitate that of Adelaide. Judging from the programme of the B.A. examination, the Adelaide graduates must be possessed of a more varied amount of knowledge than the ordinary graduates of our universities. Some knowledge of physical science (physics and chemistry), must be possessed by every graduate, and a choice of subjects is given in the second and third stages, whereby a candidate can take his degree either through literature or science. The University possesses a few valuable scholarships, one, of the value of 200*l.* per annum for three years, being awarded after examination in mathematics and natural science, the holder being required to proceed to England, take a degree in science at the London University, and undergo a training in engineering. We cannot but admire the lines on which education is conducted at Adelaide, and we trust the University may soon be able to extend its staff of teaching.

BERLIN.—The report of the Berlin University for the present year shows an attendance of 2,237 students, a decrease of 253 on the past year. The lectures are also attended by 2,080 other persons not connected with the University. The students are divided among the faculties as follows:—Theology, 135; law, 792; medicine, 297; philosophy (philology, history, &c.), 644; mathematics and natural sciences, 369. 194 students are from foreign countries, including nine English and thirty-nine Americans. The professors and privat-docenten number 200—fourteen in the theological, eighteen in the legal, seventy-four in the medical, and ninety-four in the philosophical faculties. The University library contains but 60,000 volumes, the royal library of 700,000 volumes being chiefly used.

UPSALA.—The *Abo Underrattelser* states that the Imperial Academy of Sciences of St. Petersburg will be represented by MM. Gadoline and Grote at the celebration of the 400th anniversary of the foundation of the University of Upsala.

SCIENTIFIC SERIALS

American Journal of Science and Arts, June.—An account of the discoveries in Vermont geology of the Rev. Augustus Wing (continued), by James D. Dana.—On barite crystals from the Last Chance Mines, Morgan County, Missouri, and on Göthite from Adair County, Missouri, by G. C. Broadhead.—Estimation of chromium and aluminium in steel and iron, by Andrew A. Blair.—On the chemical composition of triphylite from Grafton, New Hampshire, by S. L. Penfield.—On a new mode of manipulating hydric sulphide, by Josiah P. Cooke, jun.—On a base derived from a waste product in the aniline manufacture, by C. Loving Jackson.—On an association of gold with Scheelite in Idaho, by B. Silliman.

Verhandlungen der k. k. zoologisch-botanischen Gesellschaft in Wien, vol. xxvi. (Parts I. and II.), 1876.—The following are the principal papers in this volume:—Synopsis Cecidomydarum, by J. v. Bergenstamm and P. Löw.—On the structure and habits of lichens, by Dr. Arthur Minks.—On the ornithological fauna of the Austrio-Hungarian Empire, by A. Pelzeln (fourth paper).—Biology and characteristics of Psyllodæ, with description of two new species of the genus *Psylla*, by Dr. F. Löw.—On the flora of fungi in Hungary, by Fr. Haslinsky.—On the butterfly fauna of Surinam, by H. B. Möschler.—Mycological researches, by Schulzer von Müggenburg.—On the lichen-flora of New Zealand, by Dr. A. von Krempelhuber.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. x., fasc. viii.—On the encystment of the Proteus of Quanzati (*Amphileptus moniliger*, Ehr.), by M. Maggi.—Theory of reticular woodwork combined with an articulated system in modern American suspension bridges, by M. Clericetti.—The silk of the *Bombyx mylitta*, by M. Gabba.

THE *Jahrbuch der k.k. geologischen Reichsanstalt* (1876, vol. xxvi., Oct.-Dec.) contains the following papers:—On the ore deposits of the southern Bukowina, by B. Walter.—On the soda and Szek-soil in the Hungarian Lowlands, by E. von Kvassay.—On some green slate of the Saxon Erzgebirge, by Dr. E. Geinitz.—On the petrographical condition of the tuft-stones occurring in the Devonic formation at Graz, by Joh. Terglav.—On some rocks from the neighbourhood of Rosignano and Castellina Maritima to the south of Pisa, by Dr. Friedrich Berwerth.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 14.—"On the Minute Structure and Relationships of the Lymphatics of the Mammalian Skin, and on the Ultimate Distribution of Nerves to the Epidermis and Subepidermic Lymphatics," by George Hoggan, M.B., and Frances Elizabeth Hoggan, M.D. Communicated by Dr William Farr, F.R.S.

"Refractive Indices of Glass," by J. Hopkinson, D.Sc., M.A. Communicated by Prof. G. G. Stokes, Sec. R.S.

"Electrostatic Capacity of Glass," by J. Hopkinson, D.Sc., M.A. Communicated by Prof. Sir William Thomson, F.R.S.

"On the Difference of Potential produced by the Contact of different Substances," by Prof. R. B. Clifton, F.R.S.

Linnean Society, June 7.—Prof. Allman, F.R.S., president, in the chair.—Dr. Maxwell Masters read an interesting paper on the "Morphology of Primroses." Hitherto much discussion has arisen with reference to the superposition of the stamens to the petals, the free central placenta, and the nature of the ovules in the Primulaceæ. From a lengthened study and comparison of the development of the flower, minute structures, and phenomena of monstrosities, the author arrives at conclusions differing somewhat from those hitherto held. Cultivation is not the reason of the frequent structural variation, for deformed Primulaceæ in the wild state are far from uncommon; indeed the wild primrose itself is very much subject to such changes. Certain genera and species are more frequently found deformed than are others; for instance, the cowslip is less subject to change than is the primrose. Entering into all the more important variations observed by the author and recorded by others, in various parts of the flower, he sums up: (1) That the petals of most Primulaceæ are late outgrowths from the receptacular tube. (2) That the placenta is a direct prolongation of the receptacle or axis, and without apex or side connection with the carpels. (3) The placenta occasionally in monstrous flowers arises from the margin or centre of carpel, but sometimes is detached, the detached placentæ cohering like a solid column. (4) Staminal and carpellary leaves may occasionally be divided or lobed. (5) The ovular coat is essentially foliar, representing blade or undivided leaf, and is not a direct production from the axis. (6) Processes of carpellary leaf may be infolded, thus forming secondary carpels.—The Rev. G. Henslow followed by a "Note on the causes of numerical increase of parts of plants." In this he classified the various methods and causes of the increase of parts of leaves and floral whorls, more especially with the view of limiting each of the various kinds to its proper cause respectively.—The secretary briefly indicated the contents of a paper by Mr. Marcus Hartog, "On the floral development and symmetry in the order Sapotaceæ." From the extracts read of this communication it appears the author, from observation of growing plants in Ceylon has independently arrived at and here brought forward further evidence tending to the same results propounded by the two foregoing home botanists.—"On the nymph stage of the Embidæ, with notes on the habits of the family, &c.," was next read by the author, Mr. R. McLachlan. He stated that in 1837 Prof. Westwood (in *Trans.* Linn. Soc.) instituted the characters of *Embia*, a genus of insects allied to the white ant. Lately (therefore forty years after) Mr. Michael, of Highgate, discovered some orchids partially destroyed by an insect found to belong to the Embidæ, and subsequently the nymph form obtained fills a gap in the insect's history. Mr. McLachlan, in allusion to the habits, recorded by Mr. Lucas and others, mentioned its being carnivorous and spinning a silken web like that of a spider, which, however, Mr. McLachlan believes to be for protection from its enemies, while he doubts its carnivorous propensities, regarding it as probably a vegetable feeder. He then entered into the subject of systematic position, structure, distribution, number of species, concluding with a detailed description and zoological remarks on those forms of the Embidæ now known. He observed that the larvæ of a species of *Embia* has been noticed in fossil amber. The living forms inhabit both hemispheres at spots wide apart. None are known from Australia.—Mr. G. Busk verbally explained the more important points in the succeeding paper, viz., "Observations on British Polyzoa," by the worthy field naturalist, Mr. Charles Peach. The latter has faithfully described and delineated a number of forms of this marine family, some of which he regards as new to science, and of other known genera and species he adds much information regarding their habits and history. For instance, *Scrupocellaria scruposa* he shows has tubulous wool-fibre-like roots armed with spines, and by which it attaches itself to certain sponges, &c., a fact previously unknown.—A notice of the Lichens of the *Challenger* expedition, by the Rev. J. M. Crombie, and on Crustacea inhabiting certain hollow sponges, by Mr. Edward J. Miers, were two papers read in brief extract.

Zoological Society, June 5.—Prof. W. H. Flower, F.R.S., vice-president, in the chair.—A communication was read from Dr. A. B. Meyer, inclosing a paper by the late Dr. Bowerbank, describing five new species of sponges, discovered by Dr. Meyer at the Philippine Islands and New Guinea during his recent travels in the Eastern Archipelago.—A communication was read from Mr. E. L. Layard, F.Z.S., containing some remarks on the exact localities of certain species of Birds of the Islands of the South Pacific.—A second communication from Mr. Layard contained remarks on a paper by Mr. R. B. Sharpe on the Cuculidæ of the Ethiopian Region.—A communication was read from Mr. Arthur G. Butler containing an account of a collection of Lepidoptera, made at Cape York and on the south-east coast of New Guinea by the Rev. J. S. MacFarlane. Of these five butterflies and four moths were described as new to science.—Dr. A. Günther, F.R.S., read a report on a collection of fishes made during the late Arctic Expedition by Mr. Hart, Naturalist on board H.M.S. *Discovery*. Among them was a new species of Charr, from a lake near the winter-quarters of the *Discovery*, which was proposed to be called *Salmo naresi*.—A communication was read from Mr. D. G. Elliot, F.Z.S., containing a review of the genera and species of Ibidinæ or sub-family of Ibises.—A communication was read from Mr. Martin Jacoby, containing the descriptions of some new species of Phytophagous Coleoptera from various parts of the world.—Messrs. P. L. Sclater and O. Salvin read descriptions of six apparently new species of birds from collections lately received from Ecuador and Peru. Amongst these was a remarkable new duck of the genus *Fuligula* from the vicinity of Lima, Peru, proposed to be called *Fuligula nationi* after Prof. Nation, its discoverer.—Mr. A. H. Garrod read the third part of his series of papers on the anatomy of Passerine birds, and treated specially of some modifications of the Tracheophonine larynx which he had lately ascertained to occur in the genera *Pteroptochus* and *Grallaria*.—Mr. George French Angas communicated notes on a collection of land and fresh-water shells from South-west Madagascar; amongst these Mr. Angas pointed out three new species of *Helix*, one of *Bulimus*, and one of *Physa* which he proposed to call *Helix watersi*, *H. balstoni*, *H. ekongoensis*, *Bulimus balstoni*, and *Physa madagascariensis*.—A second communication from Mr. Angas contained the description of a remarkable shell from Japan, which he named *Thatchera mirabilis*, also the description of a new species of *Leiodomus* from Kurrachi, Scinde, proposed to be called *L. kurrachensis*.

Entomological Society, June 6.—J. W. Dunning, vice-president, in the chair.—Mons. René Oberthür, Rennes, France, was elected a foreign member.—Mr. J. W. Douglas exhibited sixteen species of *Psyllidæ* (four of them being new to Britain), which he had taken during the latter half of 1876. Mr. Douglas took the opportunity of calling the attention of entomologists to the wide field for investigation offered by these insects, the economy of many of the species being still quite unknown.—Mr. F. Grut exhibited a white downy nest from Jamaica supposed to be the work of some insect.—Mr. H. Goss exhibited a dark variety of *Cleora glabraria*.—Mr. C. O. Waterhouse exhibited a magnificent dragon-fly from Borneo. This insect, which is new to science, he has proposed to name *Gynacantha plagiata*. The specimen, a female, measured more than six inches in expanse.—The Secretary read a circular from Dr. Buchanan White, of Perth, soliciting specimens of *Hemiptera* (especially exotic) from entomologists, as he was engaged in working out that order of insects.—Dr. Sharp communicated a note on some species of Rhyncophorous beetles from New Zealand, which had been sent to Dr. Leconte for examination.—Mr. Pascoe made some remarks upon the foregoing note.—Mr. J. W. Slater communicated a paper on the food of gaily-coloured caterpillars, in which he attempted to show that brightly-coloured larvæ generally fed upon poisonous plants.—A discussion ensued, in which Messrs. Dunning, McLachlan, Waterhouse, and Meldola took part. Mr. Meldola called the attention of the Society to the explanation of the subject given by Wallace in 1867, and exhibited some butterflies which were the sole survivors of an old Indian collection, the greater part of which had been demolished by mites. The surviving specimens all belonged to protected genera (*Euplœa*, *Danais*, and *Papilio*), proving that the quality which rendered these insects distasteful was, to a certain extent, retained after death.

Anthropological Institute, June 12.—Col. A. Lane Fox, F.R.S., vice-president, in the chair.—Mr. W. J. Knowles, of Ballycully, read a paper on some recent discoveries of flint implements, worked bones, and other objects in a kitchen midden at Ballintoy, co. Antrim.—The director then read some notes on customs of the Caledonia women of Stuart's Lake and Fraser Lake Indians, and two legends of the Langley Fort Indians, by Mr. Gavin Hamilton, of the Hudson Bay Company (communicated by Dr. John Rae, F.R.G.S.)—Staff-Surgeon Messer, R.N., M.D., then made some interesting observations on the subject of poisoned arrows, as used by the South Sea Islanders, and the effects, moral and physical, of them on Europeans and blacks.—Mr. G. M. Atkinson exhibited for the Rev. J. C. Roger, Rubbings from a Runic inscription found on a stone in Cunningsburgh churchyard, Shetland Isles, and of a stone with Oghams, found five feet below the surface at Lunnasting, Shetland Isles.

Victoria (Philosophical) Institute.—The Rev. Isaac Taylor read a paper on the history of alphabets. De Rougé's great discovery has proved that the alphabet is the oldest existing monument of human civilisation—older than the pyramids. There were three stages in its invention:—1. Ideograms—pictures of things. 2. Phonograms—symbols of words and syllables. 3. The letters of the alphabet. After giving a brief account of the syllabic writing which was developed by the Japanese out of the Chinese, and by the Cypriotes out of the Cuneiform, he went on to explain De Rougé's discovery of the mode in which the Semites had selected twenty-two letters out of the 400 Egyptian hieroglyphics, and thus formed that first alphabet which had been the parent of all alphabets in the world. He showed how all the alphabets of the world were to be traced, by means of the Moabite stone, to their source in the Egyptian hieroglyphics. He went on to explain the causes of alphabetic change:—1. Those due to nature of writing materials—clay, stone, papyrus, parchment, palm-leaves. 2. Indolence in the writing. 3. Need of legibility.

CAMBRIDGE

Philosophical Society, May 21.—Mr. Pearson read a paper on one passage in Hesiod and three in Ovid's Fasti, which he said he considered might be properly tested and illustrated from modern astronomy. Admitting, as is often averred, that many allusions of this nature in the classical authors are inaccurate or wrong, some he thought might be still found to have the stamp of truth about them. Hesiod says (Op. et Di. 564-67) that sixty days after the winter solstice Arcturus rose during twilight in the evening. Arcturus's position for January 1, 1875, is given in the *Nautical Almanac* as R.A 14h. 9m. 55s., Dec. 19° 50′ 22½″ N. If we convert these data into latitude and longitude, reduce the star's longitude by about 36° 10′, which, at the annual rate of 50″·1 for *precession* will bring us to about 730 B.C., and reconvert the star's new *longitude* and *latitude* into R.A. and Dec., we shall find that the position of the star in the early part of the eighth century B.C., which may be fairly taken to represent the era of Hesiod, was something about 12h. 6m. R.A. and 33° 30′ north dec. On Feb. 20, at that time, in lat. 38½° N., about the situation of Ascra and Helicon, the sun would set about 5.40 P.M., while Arcturus would rise above the horizon about 5.53 P.M., a relative position of the two luminaries which fairly answers to the words of the poet. And while investigating the position of the star, Mr. Pearson said he found he had unintentionally explained, as he believed, the epithet "late-setting," applied to Arcturus in Hom. Od. E′ 272. Arcturus at that epoch would first have been visible at the time of its morning setting about May 24, and would set June 1 at 3.30 A.M., July 1 at 1.32 A.M., Aug. 1 at 11.30 P.M. During the early summer, therefore, when the Greek seaman or agriculturist was often spending the nights out of doors, the late time at which this brilliant star would set must have been quite unmistakable, and Ulysses is naturally described as keeping his eye fixed on it while sailing eastwards, as carefully as he kept the Bear on his left. Again in the "Fasti" of Ovid, i. 654, ii. 76, we are told that Lyra, or *Vega*, was last visible when setting in the evening, about February 1. "Ubi est hodie, quae Lyra fulsit heri?" Employing again the method of calculation indicated above, we find on that day at Rome the sun would set about 5.10 P.M., and Lyra about 5.44. As the days at that time of the year are rapidly lengthening, while the star would set earlier every day, it is obvious that the date assigned for the last appearance of the latter is nearly exact. Ovid's references to Arcturus are not at first sight so satisfactory. May 26th and June 6th ("Fasti v. 733, vi. 235) are both assigned as the first days on which he is visible, probably by a mistaken reference to two different modes of calculating the time of a star's rising. On May 26 the star would rise at 4.25 A.M., on June 6 at 3.43 A.M. The sun on the former day rises at Rome about 4.35, and on the latter at 4.30 A.M. If we consider Ovid to have consulted two different authorities, one of which gave the true and the other the visible heliacal rising of the star, no reasonable exception can be taken to the value of his statements. He makes, however, a remark about Capella which seems really erroneous. He says ("Fasti" v. 113) that she rises on May 1st, *i.e*, is then first visible in the morning. But at the time when Ovid lived she would, according to the mode of computation used in the previous examples, have risen about 3.0 A.M., while the sun would not have risen until after 5.0. We have a similar apparent mistake in Pliny and Columella, nearly contemporaries, who fix Arcturus' rising for the 23rd or 21st of February. On that day the sun would set at Rome about 5.35 P.M., whereas the star would not pass the horizon before 6.30 P.M. They seem to have copied from Hesiod without any thought. The late Mr. F. Baily, in his edition of "Ancient Star Catalogues," published in vol. xiii. of the *Memoirs* of the Royal Astronomical Society, does not seem to have actually compared the positions there given to any of the principal stars with those which in the present day we must suppose them to have then occupied. As, however, the present rate of change in the obliquity of the ecliptic would have made it in the time of Eratosthenes (230 B.C.) about 23° 43′, whereas that astronomer fixes it roughly at 23° 51′, it is to be hoped that, making allowance for inaccuracies in the MSS., such a process of verification may be attempted with some prospect of success; and possibly some explanation found of Ptolemy's idea that in his time (A.D. 140) the amount of annual *precession* was only 36″.—Mr. J. W. L. Glaisher communicated to the society a ten-figure table of the values of e^x and e^{-x}, with their logarithms from $x = 1$ to $x = 500$ at unit intervals. The table was intended to accompany Prof. F. W. Newman's table of e^{-x}, and will appear with it in the *Transactions* of the Society.

PARIS

Academy of Sciences, June 11.—M. Peligot in the chair.—The following papers were read:—On the densities of vapour; reply to M. H. Sainte-Claire Deville, by M. Wurtz.—On the atomic notation; reply to M. Berthelot, by M. Wurtz.—Second note on the *Nouvelle Navigation* of M. Villarceau, *apropos* of the interior sea of the Algerian Sahara, by M. Naudin. He urges that the result would very probably be an immense pestilential focus. The slope would be slight, and the depth of water in the border of the lake small. A large portion of land would thus be alternately covered with water in the rainy season, and left dry in the summer; and with the mixture of salt and fresh water, bright solar light, and tropical heat during two-thirds of the year, there would be active generation of organisms, the putrefaction of which must corrupt the air all round.—Theory for finding the number of co-variants and contra-variants of order and degree, given, linearly independent of any system of simultaneous forms containing any number of variables, by Mr. Sylvester.—On the rotatory polarisation of quartz, by MM. Soret and Sarasin. They have extended their researches to the ultra-violet radiations, using the light had from induction sparks between cadmium points and applying the spectroscope with fluorescent eyepiece.—Observations on the ovigerous tubes of the Phylloxera, by M. Boiteau.—Results obtained at Cognac since 1875, by the use of alkaline sulpho-carbonates, by M. Monillefert.—On the use of sulpho-carbonates, by M. De Georges.—On a new electric lamp with oblique circular rheophores, by M. Regnier. He was led to this arrangement from having observed that with rheophores meeting angularly the most of the light was emitted at the summit of the angle. The occultations—hitherto inseparable from carbon discs—are suppressed. Each rheophore has its own clockwork movement, and the motors, pivoted, can oscillate with their respective rheophores. One is manœuvred by the operator, who puts the carbons in position; the other, commanded by a solenoid in the circuit, oscillates automatically, bringing the carbons in contact, or separating or approximating them at the proper time.—M. Cance presented a new system of electro-magnets with multiple cores, in which M. Camacho's tubular cores are replaced by small soft iron rods juxtaposed, and enveloping, two by two, the different layers of spiral; this gives certain advantages.—M. Trouvé presented an improved sound for wounds caused by fire-arms.—On the infinitely small displacement of a dihedron of invariable size, by M. Mannheim.—Historical remarks on the theory of motion of one or several bodies, of constant or variable forms, in an incompressible fluid (continued), by M. Bjerknes.—On certain functions, similar to circular functions, by M. Appell.—Comparative study of observations by day and by night; second note, by M. Perrier. He finds that azimuthal observations by night have a degree of precision at least equal, if not superior to that of observations by day, and thinks they should forthwith be introduced into the practice of geodesy.—On the determination of the zenith of a ship or point observed at sea by means of straight lines of height; insufficiency of the zenith or place of the ship called *the most probable*; determination of a point nearest the true zenith, by M. Bertot.—Researches on the use of magneto-electric machines with continuous currents, by M. Gramme. With baths coupled in tension, M. Wohlhill, of Hamburg, got a deposit corresponding to 43 kilogr. of silver per hour, while expending 15-horse power on the machine. M. Gramme describes several experiments by himself of this nature.—Influence of a mechanical action on the production of various hydrates in supersaturated saline solutions, by M. Gernez.—On the new general method of synthesis of hydrocarbides, acetones, &c., by MM. Friedel and Crafts.—Researches on normal propylene, by MM. Reboult and Bourgoin.—Composition of a substance formed on an iron rod altered by a Siemens gas furnace, by M. Terreil. Under the simultaneous action of the oxidising and reducing gases of the furnace, the iron was transformed almost wholly into anhydrous protoxide of iron. M. Daubrée made some remarks on this.—On the asparagine of amygdalene; hypothesis on its physiological *rôle* by M. Portes.

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THURSDAY, JUNE 28, 1877

SOLDIERS' RATIONS

THAT Soldiers' Rations are not without influence upon a campaign no one will dispute. It is not enough to have murderous weapons and big battalions to insure conquest nowadays, and in the last two little wars in which this country engaged these were indeed of secondary importance. The Abyssinian campaign, when our troops marched nearly four hundred miles across a rugged and unknown country, has been justly termed a victory of engineering, while the Gold Coast expedition, by reason of the efficient sanitary arrangements which reduced to a minimum the deadly effects of a terrible climate, may be fitly called a doctor's war. No doubt in the case of European struggles, far more depends upon the purely military element; but if the Prussian needle-gun contributed in a great measure to the defeat of the Austrians at Sadowa, it is none the less true that the famous *Erbswurst*, or pea-sausage, of the Germans had much to do with their maintaining the siege of Paris during the long cold winter months of that capital's investment.

It is a little difficult to institute comparison between the nutritive qualities of the rations served out to soldiers in various countries. A soldier in the field, whether marching or fighting, must put forth more muscular energy than in times of peace, and according to Dr. Parkes and other authorities, it is the nitrogen in his food, more than anything else, that is necessary to the activity of the muscle, and this is required in greater quantity in proportion to the increase of work. That hard labour can be performed for some time without any increase of nitrogenous diet is true no doubt, but in this case it is at the expense of the nitrogenous constituents of other parts of the body, in the neighbourhood of the muscle, and it would be impossible for a man to continue such labour for any length of time. Whether the nitrogenous matter he assimilates is contained in meat or bread seems to be a matter of little import. An English soldier who gets a three quarters of a pound ration of meat daily is said to be no better off, as regards the nutritive character of his diet, than a German soldier, whose staple food is rye bread, and this one can well believe, looking at the constituents of the two food-stuffs. Meat from a lean animal, contains but 12·8 per cent. of nitrogenous matter, whereas samples of rye which have been analysed, have been found to contain as much actually as 15·8 of the same body. Moreover, the amount of water in a pound of meat and a pound of bread is a matter that must not be overlooked, for while in the former it amounts to 57 per cent., in the latter case it is only about 40. As, too, a loaf of bread constitutes of itself a very perfect diet, the starch and fat it contains supplying the calorifiant or heat-producing matter necessary in animal food, we may assume that troops fed upon good bread are as well off as those supplied with more costly rations. At the same time it cannot be denied that different climates and different conditions have a vast influence upon dietary, and while British soldiers require a goodly allowance of meat to sustain their energy, the Turk rarely tastes such food from one week to another. In fact, in the Moslem soldier we have the most easily satisfied of beings, so far as the commissariat is concerned. He does not even require bread, but will fight for weeks and months together upon rations of meal or bruised Indian corn, which serves him indifferently for breakfast, dinner, and supper. The Russian has rather better food, although from our point of view his fare may appear frugal enough. Two pounds of black bread and a quarter of a pound of fresh meat, or bacon in lieu thereof, with garlic, salt, and plenty of tea, seem to be the daily rations of the Czar's soldiers, though a coarse sweet bean, known in this country as the locust bean (Johannisbrod), is occasionally, also employed as food. There is no knowing what the composition of Russian bread is, but assuming it to be for the most part of rye or Indian corn, there should be little difference between the nutritive qualities of the rations of the Turks and Russians, supposing, that is, the soldiers in both cases receive pretty well as much as they can eat. There is enough nitrogenous matter to make muscle and bone, as well as sugar and starch, or non-nitrogenous bodies to supply animal heat and to support the respiratory organs. Taking milk as the most perfect food we have for our standard, which may be said to be made up of nitrogenous matter, oil, and sugar, we find that the proportion of nutritive, to heat-producing, or calorifiant, matter, is as one to two. Beans and peas come next in order to milk, the proportion here being as one to three, while in oatmeal it is as one to five, and in rye, wheat, Indian corn, &c., as one to seven or eight. Thus the Turk and the Russian being fed mainly upon rye and Indian corn derive equal benefit from their rations, although the Muscovite soldier gets additional energy, no doubt, from the small ration of meat allowed him.

The highly nutritive character of pea-flour at once points to the *raison d'être* of the pea-sausage of the scientific German soldier. This newly-invented food-stuff consists, as our readers probably know, of peamea and bacon fat, suitably seasoned, and pressed into skins and boiled. The ordinary daily ration of a German soldier is 2 lb. of rye bread and a dinner of soup, which sometimes has a piece of meat floating in it, but generally does not; this, together with a scanty stipend, which barely suffices to buy him a cup of coffee in the morning and a herring, or salted cucumber, to eke out his bread with, constitutes the whole of his allowances. In the last European war, these comestibles were replaced during some portion of the campaign by the *Erbswurst*, and there cannot be a doubt that the health of the Teuton army was improved by a regular and sufficient supply of this suitable food, while at the same time it greatly simplified the commissariat service of the invaders. Butchers, bakers, army ovens, and cooking pontoons were for a while dispensed with, and thus it was possible for corps and regiments to move, when necessary, without a great deal of impedimenta. Moreover, as we have seen, the pea-flour gave that extra nutrition which troops subject to unusual exertion, coupled with exposure to cold and frost required. To the English palate the pea-sausage had an unmistakable taste of tallow, and there is no doubt that all kinds of fat and grease were employed in its production when the supplies of bacon run short. Animal fat of some kind was, however, absolutely necessary to supply the system with heat, and combining the former in this way with pea-flour was a most happy idea. The pea-sausage might either be eaten cold in the condition in which it was issued to the soldier, or made into a sort of soup with boiling water.

And here we may mention a circumstance of especial interest to scientific men, in connection with the manufacture of this new food. The *Erbswurst* was produced in such huge quantities, that it was found to be absolutely impossible to procure a sufficient number of skins and bladders to contain the preparation. All sorts of substitutes were tried. Oiled fabric and vegetable parchment, as well as other waterproof materials were essayed in vain, for an envelope was required which was elastic and unaffected by boiling water. At last a chemist stepped in and solved the problem. He proposed the use of gelatine mixed with bichromate of potash, or in other words the process employed by photographers now-a-days in producing what are termed carbon-prints. It is well known that if a solution of gelatine and bichromate of potash is spread upon paper and exposed to light, the gelatine becomes insoluble in a very short time, and will effectually resist the action of cold or hot water to dissolve it, this principle being in fact that upon which photographic prints are produced, the portions of a surface which refuse to wash away, constituting a picture. This same mixture was used for treating the sausages. The food was pressed into proper shapes and then dipped into the bichromated gelatine solution, after which it was exposed to daylight for a couple of hours, when the gelatine formed a tough skin around it, capable of being boiled with impunity.

Turning to the British soldier we find in him the most daintily fed of all warriors, unless it was the Servian in last year's war. If we are to believe special correspondents, the rations of the Servian soldiers were almost unlimited, and furnished a striking contrast to the fare of the frugal Turks. An oka, or 2½ lbs. of brown bread, half an oka of fresh meat, together with a modicum of rice, meal, and paprika was the daily ration, the last-named comestible being employed for making soup; the *pot-au-feu*, so we were assured, was to be found simmering in camp from early morn till noon, and then only came off to make room for the coffee kettle. The Servian soldiery, too, usually had a ration of spirits called slivovitch, or plum brandy, allowed them, and yet withal they had no such powers of endurance as the maize-fed Turks. In this country a soldier's ration is three quarters of a pound of meat and one pound of bread, which is supplemented in war time by a quarter of a pound of cheese, together with cocoa or tea, sugar, &c. In the Crimea there was a standing order that hot tea should always be kept ready when practicable, so that the men might partake of it at any time, and in the Abyssinian and Ashantee campaigns the camps were never broken up of a morning before the troops had been supplied with a cup of warm coffee for breakfast. Tea and coffee exercise the same effect upon the system as wine and spirits, but their stimulative action is less marked, and our commanding officers are enjoined never to issue a ration of spirit except under extraordinary circumstances, as in the case of distressing marches, or when troops are engaged in the trenches or up at the front. And yet, as we have said, with this apparently liberal feeding, our men do not receive so much actual nourishment or nitrogenous matter as the German soldier, whose mainstay is the 2lb. loaf of black bread he receives daily. The meat, bread, sugar, &c., received by our soldiers in the Crimea yielded, we are told by the Royal Commissioners, but 23·52 oz. of nutritive principle, while Germany gives her soldiers 32·96 oz., which is still further increased when the latter are fed on such highly nitrogenous diet as the pea-sausage. The Turks, poor as their food may seem to us, probably derive as much nutriment from it as English troops from their bread, meat, and cocoa, for weight for weight, the Turkish rations contain more nitrogenous matter. If, too, their meal is what is termed "whole flour" it will, since it includes the husk, contain more nitrogen still, and, like oatmeal, be one of the most generous foods known. Our Scotch troops, we fancy, would be little the worse if fed solely on porridge for a time. The reader may remember Lord Elibank's retort on Dr. Johnson's definition of oats as the food of horses in England and of men in Scotland: "Yes," said he, "and where else will you find such horses and such men?" A growing soldier, hard at work all day at gun-drill, or other laborious work, does not buy extra meat when he is hungry, but foregoes his beer at the canteen for another pound loaf, thus approaching his diet very nearly to that of the German warrior, whom we have shown lives almost entirely on bread and enjoys the most nutritive fare. At the same time it is necessary to bear in mind that the conditions under which a man lives must guide the nature of his food. A man inhabiting a cold climate such as ours, requires more animal food than would be the case if he lived in a country nearer the equator, and British troops, we fear, would loose much of their energy if fed altogether on farinaceous food. But as we have striven to show, it is not always a so-called liberal diet which affords the soldier the greatest quantity of nutriment.

H. BADEN PRITCHARD

GEIKIE'S "PHYSICAL GEOGRAPHY"

Elementary Lessons in Physical Geography. By Archibal Geikie, LL.D., F.R.S., Murchison Professor of Geology and Mineralogy in the University of Edinburgh, and Director of the Geological Survey of Scotland. (London: Macmillan and Co., 1877.)

AS our knowledge of natural phenomena widens and our insight into the character and mode of operation of the forces which give rise to these phenomena becomes more profound, we are called upon from time to time to take a new survey of the fields of inquiry and to reconsider the principles on which the useful, but necessarily more or less arbitrary, classification of the natural history sciences is made to depend. To instance a notable example, the time-honoured division of the "three kingdoms in nature" has now, by almost universal consent, been abandoned in favour of a more logical grouping of the objects of natural history science depending on the presence or absence in them of the principle of life, and hence has arisen the term biology to include botany and zoology, while mineralogy, released from an unnatural bond, seeks and finds new alliances with those branches of knowledge, crystallography, chemistry, and petrography, with which it has so many and such intimate relations. Etymological purists have indeed cavilled at the term "biology," and the opponents of change have disputed its *raison d'être*, but it is impossible to deny that its invention was the natural consequence of the growth of juster views concerning the relations of living beings to one another, or that, on account of its fitness, it bids fair to survive all hostile criticism.

Now in the same way that the development of our knowledge of the lowest forms of life has led to the breaking down of the unnatural barriers between the animal and vegetable kingdoms, and the union of all the anatomical, physiological, systematic, and ætiological branches of our knowledge of living beings into the federal republic of biology, so the growth and establishment of a juster geological philosophy has greatly modified, and indeed almost revolutionised, our conception and treatment of certain branches of geographical science.

For more than half a century the principle which demands that the geologist shall interpret the past history of the globe by means of a constant reference to the operations now going on upon its surface, has been steadily gaining ground; and this postulate may now be said to have taken its place as the very cornerstone in all geological reasoning. But if geology has thus to own her dependence on geographical knowledge, she has more than requited her obligations by the new vitality which she has infused into her sister science. It is not too much to assert that the growing conviction of the necessity for a more systematic, a more searching, and a more accurate investigation of the phenomena of the globe and of the forces by which they are produced—a conviction which has prompted the despatch of expeditions for carrying out carefully organised researches both on sea and land—has been to a very great extent created and fostered by the revelations of glaring imperfections in our knowledge of the earth's existing economy which are continually being made by geology.

The work before us is an example of the treatment of geographical questions from the point of view of a geologist, and we are not surprised to find that its author is evidently strongly actuated by the conviction of the necessity for a broader and more vivid presentation of the action and reaction upon one another of the various forces operating upon the surface of the globe, than is usually found in works on physical geography, in order to convey a just idea of the character and significance of the features which it presents. Thus, in the introductory chapter, after referring to that complex interplay of agencies by which the fluid envelopes of the globe are maintained in constant circulation, and the elements of its solid crust made to pass through ever-varying cycles of change—a series of phenomena which has suggested to the profounder thinkers of all ages an analogy between our planet and a living being—the author goes on to say:—

> "Now this life of the earth is the central thought which runs through all that branch of science termed physical geography. The word geography, as ordinarily used, means a description of the surface of the earth, including its natural subdivisions, such as continents and oceans, together with its artificial or political sub-divisions, such as countries and kingdoms. But physical geography is not a mere description of the parts of the earth. It takes little heed of the political boundaries except in so far as they mark the limits of different races of men. Nor does it confine itself to a mere enumeration of the different features of the surface. It tries to gather together what is known regarding the earth as a heavenly body, its constitution, and probable history. In describing the parts of the earth—air, land, and sea—it ever seeks to place them before our minds as to make us realise not only what they are in themselves, but how they affect each other, and what part each plays in the general system of our globe. Thus physical geography endeavours to present a vivid picture of the mechanism of that wonderfully complex and harmonious world in which we live."

In that easy and graceful style, of which he possesses so perfect a mastery, the author proceeds in subsequent chapters to give a sketch of those vast fields of knowledge which are opened up to us by this method of looking at the phenomena of the globe. The book is exactly what it professes to be—a series of elementary lessons; but, while it may be read with profit and delight by any fairly-taught schoolboy, it will not be found wanting in instruction and suggestiveness for more advanced students. On some questions, as for example that of the nature and causes of the great movements of the atmosphere, the author has been particularly successful in embodying within a very small compass a mass of information which the student could otherwise gain only by the perusal of a number of special treatises To teachers of elementary science who desire a model on which to frame their lessons for beginners, so as to secure their attention and interest and to arouse the enthusiasm of such among them as are capable of that sentiment, we very heartily commend this admirable little book.

The author points out in a note that the subject of physical geography, as here treated of, is conterminous with that division of science for which the name of physiography has been suggested. The advances made in recent years in the study of physical astronomy and the relations which have been established between celestial and terrestrial objects by the development of spectrum analysis and the study of meteorites, taken in connection with that strongly-felt necessity for a deeper insight into the mode of operation of the forces operating upon the surface of the globe, both from within and without, which geological research has awakened, have independently suggested to many thinkers the desirability of permitting certain portions of natural knowledge to crystallise around a new centre. The importance of this new science thus growing up on the confines of geography, geology, astronomy, and biology, and linking them all together, a science the study of which would form the most fitting preparation for the detailed pursuit of all and each of the natural sciences, was long ago pointed out by Prof. Huxley; and in a course of lectures delivered in 1870 he sought to illustrate the objects and methods of this latest-born member of the family of the natural sciences. In that most excellent of geological text-books, Prof. Dana's "Manual of Geology," the term "physiography" is also employed, in the same sense as advocated by Prof. Huxley. Nor is the use of the term confined to English writers, for in several of the best German manuals of geology, such as Dr. Hermann Credner's "Elemente der Geologie" and Dr. F. von Hochstetter's "Die Erde nach ihrer Zusammensetzung, ihrem Bau, und ihrer Bildung," the necessity of this term physiography is admitted and its use justified. Like the term "biology," that of "physiography" may not improbably meet with some opposition on its first introduction, but as the importance and connection of the branches of knowledge which it embraces become more widely appreciated, the necessity and convenience of the name will doubtless make themselves very generally felt. In conclusion, we cannot part from the little book which has prompted these remarks without taking the opportunity of congratulating the author on his success in presenting to the public, in a form at once compendious and popular, the outlines of this very important branch of science. J. W. J.

THE LABORATORY GUIDE

A Manual of Practical Chemistry for Colleges and Schools. Specially Arranged for Agricultural Students. By Arthur Herbert Church, M.A., Professor of Chemistry in the Agricultural College, Cirencester. Fourth Edition, revised. (London: John Van Voorst, 1877.)

THE fact that Prof. Church's "Laboratory Guide" has reached a fourth edition is a proof that the work has been found useful by that class of students for whom it is specially arranged. Notwithstanding this fact we cannot regard the book as occupying other than a second-rate position in the literature of applied chemistry. The aim of the "Guide" is (1) to place before the student a series of lessons in chemical manipulation in working through which he shall obtain a practical knowledge of "some of the chief truths learnt during the course of lectures on inorganic or mineral chemistry;" (2) to instruct the student in qualitative analysis with especial reference to the analysis of agricultural products; (3) to lay before the more advanced student a number of processes for the quantitative analysis of agricultural substances, food stuffs, manures, &c. The first part of the work comprises a number of fairly well chosen examples in chemical manipulation, preparation of gases, and examination of solid substances. What we should most object to in this portion of the "Guide" is want of method. A few blowpipe experiments are introduced here and there, followed, perhaps, by a short description of one or two rough experiments illustrative of the manufacture of superphosphates; these are succeeded by desultory tests for sugar in milk, by casual semi-quantitative experiments on bread, and so on. To a student without any knowledge of chemistry such a course as that sketched in the first part of the "Guide" may be of use, although we think more care would require to be shown in the selection of experiments; but the book assumes that the student accompanies his practical work by attendance on lectures; surely then the practical course ought, from its very commencement, to be systematic and progressive. The directions given in each lesson are, as a rule, too meagre; without the constant superintendence of a teacher we doubt whether the beginner in practical work could make much progress. In some cases the directions are so vague and inexact as to be positively misleading: witness the method for detecting alum in bread (p. 43). Part II. treating of qualitative analysis has the same failings as Part I.; it is not exact and definite. The author, in his introduction, especially announces that the work is limited in its aim, so that we cannot find fault with him for not including tests for all the metals; but so far as it goes the information given, and the system of teaching pursued, should have been definite, condensed, and such as would train the student in habits of accuracy. No doubt the reactions detailed are true so far as they go; the schemes of analysis are tolerably good, yet there is about it all a slipshod appearance which stamps the work with an unsatisfactory character.

The processes of quantitative analysis are chiefly such as are required in the examination of agricultural products, and substances used in manufacturing manures, of a few leading food stuffs, of soils, and of waters. As the author has not wished to produce a large work, he has limited himself to a description of methods of analysis "intended only for the particular case mentioned;" these processes "may fail if . . . . other substances be present than those here supposed." We cannot help thinking that this is exactly what he ought not to have done; if the book is to be a guide to the student, if it does not pretend to the place of an encyclopædic reference book, then processes of *general* applicability, should have been selected, processes which would illustrate the application of the general principles of analysis, not processes which the student is to learn by rote, and which he will therefore come to regard in much the same light as that in which the cook views her book of receipts. Many of the processes, regarded simply as prescriptions, are faulty or very meagre. Who would apply the volumetric Uranium method for determining phosphates in the manner described on pp. 157, 158? Aided only by the description of the volumetric method for determining chlorine given on pp. 159, 160, who could ever hope to perform an exact estimation of that element? From what is said on p. 150 one would suppose that "reduced phosphates" can be readily determined with something like accuracy. Th report of the British Association Committee has shown that no method for even approximately determining these phosphates has as yet been introduced.

The processes for the analysis of milk, cheese, and butter are extremely meagre. Now that we are possessed of really good and reliable methods for analysing these food stuffs, the introduction into a manual of vague and sketchy methods is almost worse than the omission of all methods, whether good or bad.

One point there is in which Prof. Church deserves all praise, namely, the employment of a systematic nomenclature. The system adopted is that first employed in the works of Roscoe, and of Harcourt and Madan, and now adopted in the *Journal* of the Chemical Society, in Watts's *Dictionary*, and in most of the modern treatises. This system, although not slavishly bound down by rule—although it allows one to say *sulphate of zinc* as well as *zincic sulphate*—is founded on certain definite ideas, and has, at the same time, shown itself capable of expansion with the needs of an increasing science.

The system is, moreover, nearly identical with that employed by the German chemists. Prof. Church has done well in making use of it.

OUR BOOK SHELF

Researches on the Glacial Period. By P. Kropotkin. First fascicule. 827 pages in 8vo. With Maps and Woodcuts in a separate brochure. (*Memoirs* of the Russian Geographical Society, vol. vii., 1876.)

THE book consists of two parts. The first is a detailed account of a journey in Finland and a short visit to Sweden, both made in 1871 under the auspices of the Russian Geographical Society, for the special purpose of studying the glacial formations and the *ösar* (eskers or kames). The second part is an inquiry into the meaning and value of various evidences of the glacial period—the striation of rocks, the forms of rocks and mountains, the boulders, the loose deposits, and the moraines and *ösar*. Out of the seven chapters into which this part is divided only the three first (sketch of the development of the glacial theory, striation, and forms of mountains) appear in this fascicule, and the two last (loose deposits and their classification, moraines, and *ösar*) are summarised at length in an Appendix.

The first fascicule is illustrated by a hypsometric map of Finland (southern half) with all known *ösar* shown upon it; by a map of the most interesting, esker Pungaharju, five miles long; by some other maps and sections of less importance; by a section on a large scale of the loose deposits along the Tavastehus-Helsingfors Railway, and by ninety woodcuts, a large part of which are sections of *ösar*.

The main conclusions as to the glaciation of Finland are in accordance with those arrived at by Messrs. Erdmann, Wiik, Helmersen, and Schmidt, viz., that this low table-land, continuous along its north-western and southern borders with two low and flat border-ridges, was covered with an immense ice-sheet which, creeping from Scandinavia, crossed the Gulf of Bothnia, traversed Southern Finland in a direction south by east, crossed the Gulf of Finland and crept further on in the Baltic provinces. The numberless striæ, the positions and directions of which exclude any suspicion of their having been traced by floating ice, the striation on the islands of the shallow gulfs, together with that of the Onega basin, the Neva valley, and the Baltic provinces, the uninterrupted sheet of till, *i.e.*, of a true unstratified and unwashed morainic deposit covering Finland, the numberless moraines parallel to the glacial striæ, and hundreds of other evidences, settle the existence of such an ice-sheet beyond any doubt. As to traces of marine formations, there are none above a level of about 100 to 120 feet; only local lacustrine deposits cover the till above this level.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Indian Rainfall and Sun-spots

I HAVE observed no notice in NATURE[1] of an important discussion which took place a month ago at one of the Royal Society's meetings on Dr. W. W. Hunter's report on the cycle of rainfall in India, and its coincidence with the periods of eleven years disclosed by sun-spot observations. As one interested in solar research I have carefully considered that report, and I think the author has made out a case within the limits which he assigns to himself. The application of the mathematical law of errors has not altered this opinion in my mind, and from a consideration of the whole subject I have been led to the following conclusions:—In the first place I would remark that in certain meteorological elements, of which the rainfall throughout the world is probably one, and the barometer *in these latitudes* is another, oscillations which we regard as non-periodic, are very large compared with periodic variations. The consequence will be that in dealing with a series of barometric observations in these latitudes, the mean difference of individual observations from the mean of the whole series, or in other words, the mean irregularity, will not be materially modified by the introduction of the comparatively small semi-diurnal variation. But this is no argument against the existence of such a variation, nor is the fact that at Madras the mean rainfall irregularity is not *greatly* reduced by the introduction of an eleven-yearly cycle any argument against the existence of such a cycle. As a matter of fact, this mean irregularity *is reduced, although perhaps not very markedly*, by the introduction of this cycle. The true test of a physical cycle is its repetition, and, since in the present important aspect of this question we cannot, perhaps, calmly wait for other sixty-four years' observations before venturing a conclusion, let us now endeavour to break these sixty-four years up into periods, and see whether we obtain any traces of physical persistence from this method. Grouping, as Dr. Hunter has done, the sixty-four years' Madras rainfall into series of eleven years, beginning with the first in 1813, we obtain the following table:—

Years employed.	Year of Series.										
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
A. 1813–23 ...	45·11	32·41	56·00	41·16	63·56	76·25	36·33	70·01	47·13	59·61	26·62
B. 1824–34 ...	33·72	56·05	60·73	88·41	37·89	36·87	32·43	44·35	18·45	37·11	39·00
C. 1835–45 ...	41·47	44·76	49·26	52·33	53·07	58·65	58·32	36·48	50·28	65·36	38·05
D. 1846–56 ...	79·81	80·99	54·76	39·81	36·88	64·32	72·69	35·82	43·20	32·32	46·99
E. 1857–67 ...	52·95	48·50	55·14	27·64	37·19	38·18	54·61	47·23	41·64	51·39	24·37
F. 1868, end.	41·43	32·31	74·10	56·35	73·67	51·83	62·90	37·12	21·49		
Whole period.	49·1	49·2	58·3	50·9	50·4	54·4	52·9	45·2	37·0	49·2	35·0

In this table 3, 4, 5, 6, 7 embrace the maximum rainfall group, and 8, 9, 10, 11 the minimum rainfall group, and the sun-spot maximum occurs generally about the beginning of 3, and the sun-spot minimum a little before 11.

We have, therefore, taking the means of the five maximum rainfall years a result = 53·4 for the whole six series, and also, taking the means of the four minimum rainfall years, a result of 41·6 for the whole six series.

But we can obtain similar results for each individual series as under:—

		Max. Group.		Min. Group.
Series A	...	54·7	...	50·8
,, B	...	51·3	...	34·7
,, C	...	54·3	...	47·5
,, D	...	53·7	...	39·6
,, E	...	42·6	...	41·2
,, F	...	63·8	...	29·3 (incomplete.)

We have thus considerable evidence of repetition. In connection with this it will be interesting to see if there is any other physical difference indicated between years of maximum and minimum spots besides mere difference of rainfall. Now a very interesting additional peculiarity has been indicated by General Strachey, who has observed that the conception of a cycle of eleven years introduces a decidedly diminished mean cyclical deviation for the minimum period. General Strachey has, no doubt, likewise remarked that this is not chiefly due to those particular years that are nearest the sun-spot minimum. I do not, however, see that we have any right in tracing a connection between solar epochs and rainfall values to insist that the minimum of the one shall correspond absolutely with the minimum of the other, and the maximum of the one with the maximum of the other. In conclusion, the fact that the introduction of a solar cycle diminishes considerably the deviation for minimum years is one of very great interest, since it is these very years that have become so practically important. I trust, therefore, that further attention will be devoted to this very interesting inquiry.

BALFOUR STEWART

Natural History Museums

I AM sure that many readers of NATURE will heartily thank Prof. Boyd Dawkins for his valuable articles just published in your journal on the need of establishing natural history museums in the principal towns of our country. The ideas set forth cannot fail to be reciprocated by a largely increasing number of students who, like myself, are suffering under the disadvantages of not having local museums for reference and in which to compare specimens and examine the various natural history objects which I wish to study. In addition to a museum, I think such buildings should contain lecture-rooms specially fitted up for scientific lectures, as the value of able discourses is frequently lost for want of clearness in illustration.

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[1] See abstract of Gen. Strachey's paper on another page.

The professor cannot over-estimate the value of museums, as every lover of natural history cannot be a collector; but every one in full possession of his faculties can observe so far as he has the power of seeing, and if he cannot examine the wide field of nature for facts he will at least examine the proofs of them in the museums, if at hand.

A few personal observations may serve to show the difficulties under which the so-called working classes have to labour in the pursuit of knowledge.

Some years ago I began to study the works of Sir C. Lyell and other authors on geology, and while so engaged I many times travelled eighteen miles after a hard day's work to compare specimens in the old museum, St. Peter's Street, Manchester. I had tabular views of the characteristic British fossils at hand, but as perfect specimens only are figured, I experienced a doubt and uncertainty pretty nearly in everything I wanted to compare, while in the museum I could find the actual specimen sought after with which to correlate those of my own. The flash of satisfaction experienced by a collector on comparing his objects with those in a well-arranged museum is indeed very great, and there are few things more likely to stir him up to renewed efforts. But the interest of museums is not confined to the collectors of natural history objects; it extends to every man who reads and cares to master the objects about which he reads. In this way his knowledge of things becomes real and he expresses himself with confidence, and in many cases has decided while others are thinking. To show further the need of museums I may state a fact perhaps not generally known, that in one place in the north of England a large number of science students have formed themselves into an itinerant society moving from place to place to suit the convenience of the various members who reside apart. The meetings are generally held at a respectable inn on Sunday evenings, at which papers are read by the more ambitious members, and any interesting objects named, which some of the party never fail to bring up, and their habitat declared.

If the corporate bodies or the educational department of the State would only undertake to provide museums in the principal towns of our country I feel sure that the cry of continental superiority would soon vanish. At home we have the materials out of which the philosopher and the artisan can spin the fibre of future greatness by rightly directing the forces of nature, but the isolated fragments want collecting and receptacles providing in which to store them. Many lives like that of the Banff naturalist could be written if only known, and Prof. Dawkins could not have fixed on a centre of operation more favourable from which to begin than that of Oldham. Men more selfishly removed above praise, working for science for its own sake, he cannot find, and it is a pity that they have not a common repository in which to store their invaluable collections beyond their own full cabinets. I hope the professor's articles will be a means of calling attention to the desirability of establishing museums for the better diffusion of scientific knowledge.

I write from the point of view obtained by my own experience as a working man who has done his best to educate himself.

WM. WATTS

Corporation Waterworks, Oldham, June 16

Koenig's Tuning Forks

ON vient d'attaquer en Angleterre l'exactitude du diapason officiel français. Mr. Alexander J. Ellis ayant trouvé que les notes d'un tonomètre, composé de 65 anches d'harmonium et construit par Mr. Appunn, ne s'accordaient pas avec ce diapason, a cru devoir déclarer dans un mémoire publié par le *Journal of the Society of Arts* (25 Mai, 1877), et dans votre journal (31 Mai, 1877), que le La_3 normal français donnait non pas 870 vibrations simples, comme on l'avait cru jusqu'à présent, mais bien 878 vibrations simples.

Mr. Ellis ayant constaté de plus que les diapasons de ma construction s'accordaient parfaitement avec le La_3 français, n'a pas hésité à affirmer que tous ces diapasons, y compris ceux de mon grand tonomètre, qu'il n'a probablement jamais vus, et en tout cas jamais pu examiner, étaient nécessairement inexacts. N'ayant pas à ma disposition l'instrument dont s'est servi Mr. Ellis, j'avoue que je me serais trouvé assez embarrassé pour dire immédiatement, par où péche cet instrument au point d'avoir donné entre les mains de Mr. Ellis des résultats si extraordinaires; heureusement je me suis rappelé une lettre de M. Helmholtz à Mr. Appunn et publiée par ce dernier lui-même dans une brochure sur les théories acoustiques de M. Helmholtz; cette lettre concerne justement un instrument de même nature du même constructeur et explique suffisamment les surprenantes découvertes de Mr. Ellis. "J'ai examiné à plusieurs reprises votre tonomètre," écrit M. Helmholtz à Mr. Appunn, "et je suis étonné de la constance de ses indications. Je n'aurais pas cru que les anches pussent donner des sons aussi constants que ceux que donne l'appareil, grace à votre méthode pour régler le vent. L'instrument varie un peu, il est vrai, avec la température, comme feraient aussi des diapasons; on ne peut donc s'en servir pour la détermination des nombres absolus de vibrations que lorsqu'on peut travailler dans une pièce qui n'est pas chauffée par un poêle. J'ai compté les battements à l'aide d'un chronomètre astronomique, et je crois que votre pendule à secondes a été légèrement inexact, car, si les nombres de battements s'accordent très bien entre eux, le nombre absolu en a été non pas de 240, mais de 237 à la minute. La température, qui était assez basse pendant mes expériences, a pu y être pour quelque chose, mais on peut éliminer cette influence en comptant jusqu'au bout les battements d'une tierce majeure, ce qui m'a pris un quart d'heure. J'ai trouvé ainsi pour mon diapason de Paris 435·01 vibrations, ce qui l'accorde à $\frac{1}{40,000}$ pris avec le nombre officiel de 435·00 vibrations."

Cette lettre prouve que le nombre entier des battements de l'octave du tonomètre essayé par M. Helmholtz était de $\frac{237}{60} \cdot 64 = 252{\cdot}8$, et sa note fondamentale de 505·6 vibrations simples au lieu de 512 vibrations simples. En comparant cette note de 505·6 vibrations simples avec un diapason donnant réellement 512 vibrations simples, Mr. Ellis eût trouvé ce dernier de 6·4 vibrations simples plus aigu, et l'eût sans doute considéré comme donnant 518·4 vibrations simples. Or il a trouvé 516·7 seulement pour mes diapasons de 512 vibrations simples avec le tonomètre dont il s'est servi; on voit donc que la note fondamentale de ce dernier était déjà plus exacte que celle du tonomètre examiné par M. Helmholtz puisqu'elle donnait 507·3 vibrations simples mais qu'elle restait encore assez loin de la véritable valeur.

Le fait que M. Helmholtz a pu trouver le nombre de vibrations exact du diapason officiel français avec un instrument de cette nature (et même encore moins parfait que celui dont s'est servi Mr. Ellis), en déterminant d'abord la correction de cet instrument, montre à l'evidence que Mr. Ellis a négligé de déterminer la correction du sien; il s'est donc beaucoup trop hâté de déclarer que ces petits tonomètres à anches d'harmonium sont les plus parfaits et les plus exacts qui existent, et de contester si légèrement les résultats obtenus par les Lissajous, les Despretz, les Helmholtz, les Mayer, etc., etc. RUDOLPH KOENIG

Paris, le 5 Juin

Antiquity of Man

MR. SKERTCHLY is absolute that I am mistaken; to me it appears that he has missed the point of my letter, and misinterpreted my views. His important discoveries of flint implements in early glacial beds are, I think, strongly corroborative of the opinions I expressed in my paper on the "Drift of Devon and Cornwall" (*Quar. Journ. Geol. Soc.*, vol. xxii. p. 88), and in that on the "Geological Age of the Deposits containing Flint Implements at Hoxne" (*Quar. Journ. Science*, July, 1876); but I willingly admit that in the present stage of the inquiry Mr. James Geikie has as much right to claim that they support his theory, and I agree with the latter that it is premature to discuss the relation of man to the glacial period, before we have settled what was the succession of events that occurred at tha time.

Mr. Geikie contends that there were two or more glacial periods with inter-glacial warm or mild ones; I, that there was

only one glacial period and that the disappearance of palæolithic man from Northern Europe was principally due to the submersion of the greater part of the land beneath the water of an immense freshwater lake or sea, at or a little before the culmination of the ice age. If Mr. Geikie's views should be ultimately accepted, the term "inter-glacial" will be most appropriate; but should, as I hope and believe, mine be proved to be nearer the truth, I should prefer to use the term "pre-diluvial" instead of "pre-glacial," as heretofore, to express the age of palæolithic man.
THOMAS BELT

The Cedars, Ealing, June 22

WILL you kindly allow me to correct an apparent breach of official etiquette and act of discourtesy in my last week's letter? I should have said that only two geologists prominently interested in the question at issue had seen my evidence; for, of course, Mr. H. W. Bristow, F.R.S., Director of the Geological Survey of England and Wales, has been kept fully *en rapport* with my work, and has several times visited me at Brandon. I am anxious that no statement of mine should appear to slight so eminent a geologist and so considerate a friend.

Brandon
SYDNEY B. J. SKERTCHLY

Colour-Sense in Birds—Blue and Yellow Crocuses

UNLESS your readers are quite tired of the subject, may I add a fact which will be subversive of a good deal that has been written about yellow crocuses and sparrows. I dislike yellow crocuses, and four seasons since planted some hundreds of blue and white in the garden underneath my windows. For two seasons they flowered in beautiful profusion. In 1876 the sparrows for the first time destroyed these flowers completely. I allowed the roots to remain for another year—viz., 1877—but they suffered the same usage, hardly a single flower being left uninjured. So complete was their destruction that I have had the roots dug up.

I regard the proceeding as an imitative one; blue and white crocuses, not being common in the vicinity, were new to the sparrows, and until one more experimental than the rest attacked them they were safe.

A similar result will occur with domestic pigeons; if reared exclusively with small grain, as wheat and barley, they will starve before eating beans. But where they are thus hungry, put a bean-eating pigeon amongst them, and the habit is immediately propagated.

I have seen fowls refuse maize at first, but on seeing others eat it, they follow suit, and become excessively fond of it.
W. B. TEGETMEIER

Purple Verbenas

HAVING now read for the first time the letters in NATURE regarding the preference that sparrows show for the yellow crocus, it might perhaps help to elucidate the problem were it known that the choice of colour is not only confined to birds, as a few years ago our garden was infested by rabbits and there was a row of eight beds planted in turn, with white, red, and purple verbenas. The flowers of the red and white were eaten close off, whilst those of the purple were never touched. This happened three years running, since which, the garden, being protected by wire netting, has remained undamaged.
A. M. DARBY

Japanese Mirrors

YOUR correspondents, Messrs. Atkinson, Highley, and Darbishire, have referred to several conjectures and experiments respecting the curious Japanese mirrors and the patterns they reflect. None of these gentlemen have, however, referred to the suggestion offered by Sir David Brewster in the *Philosophical Magazine* for December, 1832. In this paper Sir David drew attention to some similar phenomena in the light reflected from the surfaces of burnished buttons of metal, arguing that in the mirrors (of which at that time he apparently had seen no actual specimen) there were slight actual inequalities of surface, artificially produced, but concealed from observation by their slightness of depth and by the brightness of the polish. This, of course, may be independent of the particular figures raised in relief on the back, as in the case cited by Mr. Darbishire; and so thought Sir David, for he added:—

"Like all other conjurers, the artist has contrived to make the observer deceive himself. The stamped figures on the back are used for this purpose. The spectrum in the luminous area *is not an image of the figures on the back.* The figures are a copy of the picture which the artist *has drawn on the face of the mirror*, and so concealed by polishing that it is invisible in ordinary lights, and can be brought out only in the sun's rays."

I trust Mr. Atkinson may be able to learn in Japan the real process of manufacture of these curious toys. Meanwhile are there not specimens in many of our museums that would repay examination? Were there not some amongst last year's exhibits at the Loan Collection of Scientific Apparatus?
SYLVANUS P. THOMPSON

University College, Bristol, June 25

NOTE ON THE ELECTRICAL DISTURBANCE WHICH ACCOMPANIES THE EXCITATION OF THE STIGMA OF MIMULUS LUTEUS

MANY years ago my attention was drawn to the excito-contractility exhibited by the lipped stigma of *Mimulus luteus*, the structure of which I then gave an account of in the *Proceedings* of the Edinburgh Botanical Society. In connection with my recent investigation of the excitatory variation in *Dionæa* I have, during the last few weeks, in co-operation with Mr. Page, made experiments for the purpose of ascertaining whether in this organ, as in the leaf of *Dionæa*, the change of form provoked by mechanical stimulation is accompanied by a similar electrical disturbance.

Mimulus luteus is a favourite window plant on account of its showy flowers and the facility with which it can be cultivated. The mechanism of the contraction of the stigma can be best studied in the inferior of the two lobes, of similar size and form, of which the organ consists. In the unexcited state, when the flower is in full bloom, this lobe is curled outwards. The curling outwards is due, as I long ago observed, to the turgidity of the layer of loosely connected conducting cells, ending in papillæ, which constitute the stigmatic surface. So long as this tissue is turgid the elastic lamina by which it is backed is prevented from straightening itself, so that the whole lobe forms a scroll of which the axis is transverse. The effect of touching any part of the lobe, and particularly the papillary surface, is to diminish the turgidity of the tissue, as the result of which the organ slowly expands so as to face and ultimately meet its fellow.

The excitatory change of form which I have described is, as in the case of *Dionæa*, associated with an electrical disturbance of which the following are the most important features:—(1) The sign of the variation is the same as in *Dionæa*, the excited structure becomes negative to the rest of the plant. (2) The extent of variation is somewhat less than in *Dionæa*, the electromotive force developed between the stigma and style being usually about 25-thousandths of a Daniell, whereas in *Dionæa* the variation may amount to from 40- to 50-thousandths. (3) The variation is of relatively long duration; it reaches its maximum at the ordinary temperature of summer, about five seconds after excitation. It subsides at first rapidly, then very gradually, so that the effect may not have entirely passed off until two or three minutes have elapsed.

As in *Dionæa*, the period of electrical disturbance is shortened by increase of temperature. Thus in five stigmas in which the period was measured at 20° C. (68° Fahr.) and at 37° C. (98° Fahr.), the mean duration of the interval of time between the commencement of the electrical disturbance and the moment at which it began to subside was 6·2 sec. at the higher temperature, and 3 sec. at the lower.

In general, the stigma, when in the unexcited state, is positive to the style. As, however, it can be shown that other factors, not concerned in the excitatory process, are operative in the production of this result, not much importance is to be attached to it.

I send this short note in order that physiologists interested in the subject may be able to repeat the observations during the present season.

University College, June 27 J. BURDON-SANDERSON

TAUNTON COLLEGE SCHOOL

THE circumstances alluded to last week, under which the Taunton College School is threatening to collapse, and is in immediate danger of losing the head-master who has made it what it is, are interesting on public grounds to the advocates of scientific instruction, as well as to the general educationalist. In a pamphlet published in 1865, and containing letters from Dr. Daubeny, Prof. Phillips, and Dr. Acland, Mr. Tuckwell was, we believe, the first English schoolmaster to assert publicly the claims of science to an honoured place in the curriculum of all first-class schools; and his evidence before Lord Taunton's Commission, his papers read to the British Association in 1869 and 1871, and his communications to the Royal Science Commission, show how diligently he has for twelve years past been working out in his school at Taunton the many practical problems which beset the introduction of a new subject into an ancient, established, jealous system. The school has thriven in his hands, risen rapidly in numbers, and gained the highest public distinctions at the Universities, the India Civil Service, Cooper's Hill, and Woolwich; and though the short-sighted economy of his governing body left him for years without a science master or a laboratory, and refused him a museum, botanical garden, and science class-rooms, he has overcome all these difficulties by patience, by the munificence of friends, and by pecuniary sacrifices; and at this moment many distinguished scientific visitors are glad to testify to the completeness of a system which passes the whole school through a course of physics and chemistry, and includes physical geography, botany, and meteorology in its more special training. In 1875 the number of boys had risen to 120, but the thrift of the governing body kept down the number of the masters. The typical proportion of assistant-masters to boys in modern schools of this size is one in sixteen; the Taunton masters were only one in twenty-seven. The school could not continue to succeed under this policy; the masters were unequal to the work; the number of boys fell off until a visitation of fever brought them below the paying point, and the school, already heavily in debt, was on the point of being closed. The panic-stricken officials laid the blame upon the head-master; his theology and politics were pronounced suspect; his unpopularity had caused the falling numbers; and when his friends came forward liberally with money and promises of money the governing body took the money, but upon condition that the head-master should leave at Christmas. Against this parents and old pupils are indignantly remonstrating; both have sent to Mr. Tuckwell public addresses of sympathy and confidence; the parents forwarding also a strong protest to the president of the governing body, and in many cases threatening to remove their sons if Mr. Tuckwell goes. So far, however, the custodians of the school's interests show no sign of yielding; it seems certain that the head-master will be turned out, and more than probable that the school may, after all, collapse.

There are two points in this struggle between philistinism and culture on which we should like to dwell, in the interests both of general and of scientific education. The first is the mischief being worked in the less important first-class schools by the constitution and habits of their governing bodies. These were the pet institutions of the Endowed Schools' Commission. They were to include the educated gentleman of the county and the representative tradesman of the town: the first, rich in recollections of Eton and of Christ Church, was to initiate, develop, control; to support and instruct the head-master; and to keep his *bourgeois* brother straight; while that second-rate but docile coadjutor was to back the enlightenment of his superior, and to reconcile while he typified the democratic feeling so essential, it was thought, to the local popularity of a school. Charming in theory, it was in fact the weak point in the Commissioners' scheme. The feet on which their image had to stand were of iron mixed with miry clay; the two refused to coalesce, and the clay came uppermost. The gentlemen make admirable governors, but they are in London, in Scotland, on the Continent, at Quarter Sessions; and the local men, who are always on the spot, become virtually the governing body. They too frequently know nothing of education. They cannot understand a head-master's ideas and aims; they in too many cases govern the school as if it were a workhouse, and treat the head-master as they habitually treat the master of their union. The world has not forgotten Felsted Grammar School; and the committee of head-masters could tell us of many other cases, less notorious, but not less galling and mischievous. No first-class school can thrive unless its governing body is composed of gentlemen, who understand, as Mr. Walter said the other day at Wellington College, that their first duty is not to interfere with the head-master.

The second point is one which we have often urged before: the opposition offered by many of the clergy to the *Culturkampf*. Of course there are notable exceptions to this incrimination; but the *Viri Obscuri* of Revellius, and the clerical bigots who combined to oppose the new learning of Colet, Erasmus, and More, would recognise their legitimate posterity in those of the present day, who, themselves uneducated even according to the narrow standard of the past, join in denouncing science and unsectarianism as the irremissible sins of a head-master. Bishop Fox, the founder of the ancient school at Taunton, was rattened by the Oxford clergy for forcing the new study of Greek upon his college of Corpus Christi; his representative in Taunton shares his fate to-day, driven from the school which he has refounded for forcing on it the new study of science.

We write in no hope of assisting the head-master, or of educating his opponents into large-mindedness. Mr. Tuckwell will see his schemes collapse, and be parted from the profession in which all eagerly attest his success, and to which he has given the best years of his life. The school will either break up under the irritation of the parents, or its distinctive features will perish with the ruler who called them forth. The order of the old teaching, the assertion of the old theology, will resume their way in Taunton School. Chemistry, and physics, and botany; Shakspeare, and Milton, and Macaulay, and Guizot, will give way to gerund-grinding and Latin verse. Where Wesleyans, Independents, Quakers, Catholics, and Unitarians worshipped in the same chapel and attended the same scripture-classes, sectarian exclusiveness will re-enter its swept and garnished home. We can only chronicle the facts as indicating the obstacles to be met and reckoned with by the pioneers of modern educational progress. We can only express sympathy with the head-master, who will yet find some compensation for his worries in the unusual warmth of testimony contained in the address which first brought these circumstances to our knowledge, and in the consciousness that, having advanced a noble cause, his work will not in the end be thrown away.

ON DROPS

AMONG the many ways in which electricity is called in to give assistance in various physical investigations, one of the most elegant and interesting is the application of the electric spark to render momentarily visible a body that is rapidly moving or changing its form. The duration of the electric spark is so short—probably not more than $\frac{1}{24000}$ of a second—that a body, such as a rotating wheel or oscillating rod, moving in a dark room with extreme rapidity, will, if illumined by an electric spark, seem stationary, since the wheel or rod has not time to change its position appreciably during the short instant for which it is visible. If the spark be bright, the impression is left on the eye long enough for the attention to be directed to it, and for a clear idea to be formed of what has been seen.

The writer of this article has recently applied this method to watching the changes of form in drops of various liquids falling vertically on a horizontal plate. As usually seen, a drop of water falling from a height of ten or twelve inches on a smooth solid substance, such as glass or wood, seems to make an indiscriminate splash. The whole splash takes place so quickly that the eye cannot follow the changes of form; the impression made by the last part of the splash succeeding that of the first part so quickly as to confuse it.

A little careful observation, however, shows that the drop passes through very definite symmetrical forms, and that a splash is by no means an irregular hap-hazard phenomenon.

Let the reader let fall a few drops of milk, about $\frac{1}{4}$ inch in diameter, on a smooth dark surface of wood or paper, from a height of, say, six inches (milk is better than water, as it is easier to see, especially on a dark ground); he will observe that the liquid makes a blot with a more or less regular undulated edge, but the splash is too quick to follow with the eye.

Let him now substitute a drop of mercury for the milk. By watching the splash very intently he will be able to catch a glimpse of the mercury spread out in the symmetrical star-like form of Fig. II*a* of Set 2. After the drop has been thus spread out it recovers its globular form, since the mercury does not wet the plate. On increasing the height of fall a few inches, it will be noticed that small drops split off in a more or less complete circle, and are left lying on the plate, while the rest of the drop gathers itself together in the middle of the circle.

The chief reason why these appearances could not be seen with milk is that the milk wets the glass or wood and sticks to it, while the mercury does not. But by smoking a slip of glass or card tolerably thickly in the flame of a candle we get a finely-divided surface of lamp-black to which the milk does not adhere any more than the mercury, and by very careful watching we may notice that the same radial star is formed by the milk, but it is much more difficult to catch sight of than the mercury star. But if the mark on the lamp-black be examined after the drop of milk or mercury has rolled away it will be found to consist of delicate concentric rings with numberless fine radial striæ where the smoke has been swept away. These may be seen very well by holding the glass plate up to the light if it has not been too thickly smoked.

The marks thus made are very beautiful and symmetrical, and it will be found, if the glass be uniformly smoked, that the same-sized drops of the same liquid falling from the same height will produce almost exactly similar marks: while if the height be changed the mark on the lamp-black will be somewhat changed; and it is a fair inference, if each drop makes almost exactly the same complicated, symmetrical mark, that the splash of each drop takes place in almost exactly the same way.

The glimpse that may be caught of the drop in the way described is obtained when the drop is really almost stationary, having flattened itself out on the plate and being on the point of contracting again to its original form.

That a drop if so flattened out will recover itself is seen on pressing down a drop of mercury with the finger or a drop of water with a piece of black-lead or other substance to which it does not adhere. On removing the pressure the drop springs back to its old form; the force which causes this being exerted by the curved surface of the liquid at the edge of the flattened drop, on the liquid within. The flatter the drop becomes the greater is the curvature of the edge and the greater the corresponding pressure tending to restore it to its original globular form. The extent to which a drop that has fallen on a plate will spread out depends on the velocity with which it strikes the plate, *i.e.*, on the height of fall; so that as long as the drop returns to the globular form the whole phenomenon of the splash may be regarded as an oscillation similar to that of a pendulum; the velocity of the liquid outwards being checked, overcome, and finally reversed by the ever-increasing pressure of the curved edge, just as a pendulum has its velocity checked, overcome, and finally reversed by the action of gravity.

It is only when the height of fall is very great that the liquid flies off in all directions and the splash ceases to be an oscillation; this case corresponds to that of a simple pendulum started with a blow so violent as to break the string.

But the liquid star and the complicated pattern on the smoked glass show that the splash is not a simple spreading out of the drop equally in all directions to return again.

In order to observe the form of the drop at any given instant during the splash, it is necessary to make use of the electric spark and to take advantage of the fact that drops of the same size falling from the same height will all behave in the same way.

It will be necessary to let a drop, say of mercury, fall on a plate in comparative darkness, and to produce a strong spark at the instant the bottom of the drop comes in contact with the plate, and so illumine it; the observer will then see the drop in the form it has at that instant.

A second drop must be let fall in the same way, and be illumined by the spark not at the first moment of contact, but a shade later, say $\frac{1}{100}$ second later, when the drop will have spread itself out slightly on the plate, and similarly we must illuminate a third drop a shade later than the second, and so on. The observer can, after a little practice draw from memory on each occasion the drop in the form in which he has seen it. It will be seen that the process consists in isolating consecutive phases of the splash from those that precede and follow, and which take place in darkness and so do not confuse what has been seen as they would do in continuous daylight.

The device adopted by the writer for so timing the appearance of the spark as to illumine the drop at any desired phase of the splash consisted essentially in breaking the current of an electro-magnet at the instant the drop began to fall; the magnet thus ceasing to act, releases a spring which immediately begins to pull the terminal wire of a strong electric current out of the other terminal, which is a cup of mercury, and the strength of the spring and the depth of immersion of the wire in the mercury are so adjusted that the wire leaves the surface of the mercury, and the required spark is produced at the instant the drop reaches the plate.

For the next drop the spark is made to appear a shade later, either by slackening the spring or increasing the depth to which the terminal wire is immersed in the mercury.

The following figures have been drawn in the way described, and show the behaviour of a drop of mercury about $\frac{1}{7}$ in. in diameter, falling from a height of about three inches on to a glass plate. Each figure represents a rather later stage of the splash than the preceding.

Set 2 was drawn from the final stages of a milk drop, $\frac{1}{4}$ in. in diameter, falling 4 in. on to smoked glass; but the forms are almost identical with those of mercury. Of this set I*a* and II*a′* are vertical central sections of the middle part of the drop, while II*a* and III*a* are alternative forms of II and III.

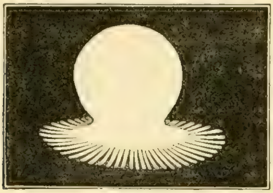


Fig. 1.

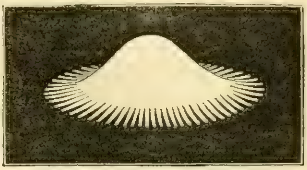


Fig. 2.

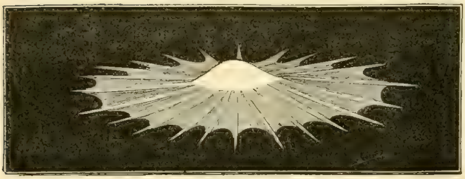


Fig. 3.

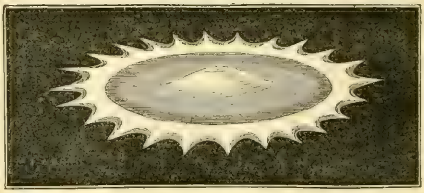


Fig. 4.

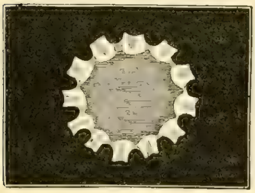


Fig. 5.

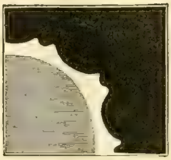


Fig. 6.

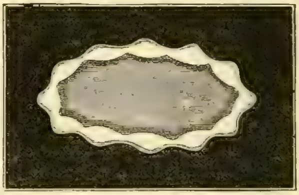


Fig. 7.

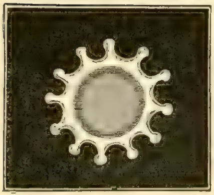
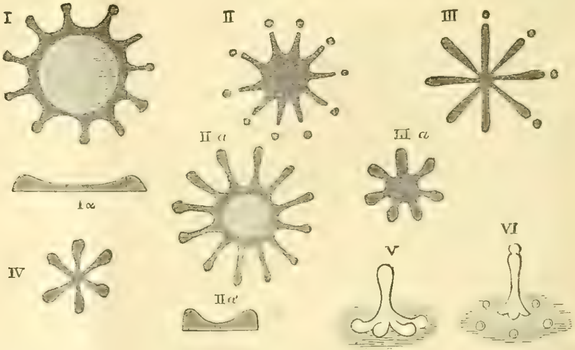


Fig. 8.



Set II.

From the ends of the rays of Fig. 4, usually twenty-four in number, small drops often split off. These are not shown in the figure. One of the most curious features of the phenomenon is the transition from twenty-four rays to twelve arms, shown in Fig. 5. The beauty of many of the forms, especially of the ridged shell-like form shown in Fig. 4, when composed of shining quicksilver apparently rigidly fixed, is very striking. Very similar forms are obtained with milk, but whether with milk or mercury are liable to occasional variations. For a more detailed account the reader is referred to the *Proceedings* of the Royal Society, Nos. 174 and 177, 1876-77.

A. M. Worthington

CHEMICAL NOTES

On Errors in the Determination of the Densities of Mixed Vapours.—In the *Compt. Rend.*, lxxxiii., Messrs. Troost and Hautefeuille record some experiments made by them to discover the error which occurs in determining the vapour-density of substances by the application of the law of Dalton on the tension of mixed gases, and Boyle and Gay-Lussac's laws, as applied to a mixture of the vapour examined, either with air or with some other vapour. They examined a mixture of carbon and silicium chlorides, using a modified form of Gay-Lussac's vapour-density apparatus. On increasing the amount of carbon chloride, the tension of the silicium chloride diminished. The vapour-density of silicium chloride alone varied only from 5·94 to 6·0, but in presence of carbon chloride was found to increase 6·27 to 8·2.

On the Properties of Ruthenium.—In the same journal an account is given by Messrs. St. Claire-Deville and H. Debray, on the physical and chemical properties of the above metal. They find that the metal forms an oxide RuO_2, thus differing from osmium. By fusing the pure metal with potash and saltpetre, then saturating the rutheniate thus formed with chlorine, and distilling in a current of the gas at about 80° C., they obtain the tetraoxide RuO_4 in yellow crystals, which, when reduced, yields the pure metal. The metal they obtained by purification from its alloy with zinc, was found to have a density of 12·261 at 0°. They also obtained a compound, $Ru_2K_2O_8$, in black crystals, on saturating the rutheniate of potash with chlorine. For the analysis of ruthenium ores, the process they employ is based on the foregoing remarks. After the fusion of the ore with saltpetre and potash, the whole mass is distilled with chlorine, the excess of gas, together with the RuO_4, being absorbed by solution of potash. The potash solution is then treated with alcohol which precipitates the ruthenium as oxide, and this is finally reduced to the metallic state with hydrogen.

Aconitic Acid in Cane Juice and Raw Sugar.—In a late number of the *American Chemist* an account is given by Dr. Arno Behr of some experiments he has conducted on the above subject. For examining the properties of this acid he has found the so-called melado a proper material, this substance being merely cane juice boiled down to a concentration such as allows the sugar to crystallise out, the mother liquor being then drawn off and used for the production of the acid. The author has analysed an acid substance formed by decomposing its ammonia compound with sulphuric acid and extracting with ether, and assigns to it the composition $C_6H_6O_6$. He has also prepared silver, calcium, and ammonium salts of the acid body, the per centage composition of these salts agreeing closely with the theoretical composition of the silver, calcium, and ammonium aconitates. Although from the results of his analyses he has no doubt of the substance in question being aconitic acid, yet the melting point, 172-173° C., which he found was not in accordance with that generally given, viz., 140° C.° ; the author therefore prepared some pure acid which had a melting point of 168-169° C. The acid formed from aconitine fused at 165° C. It is difficult, however, to determine the melting point as the acid is decomposed in the process of melting. The author has found the melado to contain 0·149 per cent. of aconitic acid. The sweet waters from charcoal filters used in refining raw sugar contain it in an appreciable quantity, and some molasses sugars give a peculiarly opaque solution from which a sandy sediment is deposited, appearing under the microscope to consist of small rhombohedral crystals, and which, on analysis, proved to be calcium aconite. The author thinks aconitic acid to be a normal constituent of sugar, and that it is worthy of remark that the two plants yielding the most sugar—the beet-root and sugar-cane—also produce two acids standing so chemically near each other as citric and aconitic acids, and which contain in their molecules the same number of carbon atoms as fruit sugar.

Minerals containing Columbium from New Localities in the United States.—Mr. J. L. Smith, of Louisville, has examined several species of minerals containing columbium, and claims the restoration of this name for the metal instead of that of niobium, generally given to it in England and on the Continent. His reason for making this reclamation is that the name niobic acid was incorrectly given by H. Rose to one of the acids found by him in his researches on the columbite of Bodemais, and subsequently proved by him to be identical with the columbic acid originally discovered by Hatchett in 1801. The name niobic acid, however, given by Rose, has never been altered, and Mr. Smith thinks the original columbic acid should have been retained. In remarks on the chemical constitution of the minerals described by him, Mr. Smith thinks that the composition of the columbates, although appearing at first sight complex and irregular, becomes much simpler when due allowance is made for the intermixture of the different varieties with each other. Columbite, the best known of the minerals, can be well recognised as a simple columbate of iron and manganese ; microlite appears to be a columbate of lime ; pyrochlore, a columbate of the cerium oxides and lime, but whether or not a neutral columbate remains to be investigated. Hatchetolite he considers as a neutral columbate of uranium and lime, and samarskite a basic columbate of iron, uranium, and yttrium oxides. Yttrotantalite and euxenite are basic columbates of yttrium and uranium, the first being anhydrous when pure, the second containing water. Fergusonite is a hydrated basic columbate of yttria, and Rogersite, a columbate still more basic. In arranging a general view of these minerals Mr. Smith does not take into account the constituents which exist in small quantities only.

Coefficient of Capillarity for Certain Liquids.—M. Gueront, in the *Comptes Rendus* (lxxxiii. 1291) describes experiments in which he finds that in any series of organic compounds the coefficient of capillarity decreases as the amount of carbon in the substance increases. He has examined three series of bodies, the fatty acids, the acid ethers of ethylic alcohol, and the ethers formed by the union of acetic acid with the different fatty alcohols. In the series of fatty acids those members above propionic acid agree with the above statement, but the two lowest members, acetic and propionic acids, are exceptions ; this he thinks probably due to impurities ; the two series of ethers, however, agree perfectly with the law. From his observations it becomes evident that the coefficient of capillarity of the ethers is higher than that of the alcohols or the acids from which they are formed, showing that the introduction of an organic radical into the alcohol molecule renders the body more fluid. On comparing the two series of ethers it was found that the isomeric ethers have nearly the same coefficient, but the acids isomeric with them are much lower. Thus valeric acid, which is isomeric with ethyl propionate and propyl acetate, has a coefficient only about one quarter that of these latter. The reason suggested for this difference is that in the two isomeric ethers atoms are grouped in a similar way, while in the isomeric acids the grouping is different.

METEOROLOGICAL NOTES

Sun-Spot Periods and Auroras from 1773 to 1827.—We have received a communication from Mr. Buchan inclosing the following table, showing the number of auroras observed by Mr. James Hoy at, or in the vicinity of, Edinburgh, each year from 1773 to 1781, and at Gordon Castle, Banffshire, from 1781 to 1827 :—

Year.	Sun-spots.	Auroras.	Year.	Sun-spots.	Auroras.
1773	40	5	1801	39	2
1774	48	5	1802	58	2
1775	28	1	1803	65	6
1776	35	3	1804	75	5
1777	63	0	1805	50	2
1778	95	6	1806	25	2
1779	90	11	1807	15	1
1780	73	6	1808	7	1
1781	68	2	1809	3	1
1782	33	3	1810	0	0
1783	22	2	1811	1	0
1784	5	0	1812	5	0
1785	21	0	1813	14	0
1786	89	10	1814	20	1
1787	105	12	1815	35	1
1788	108	9	1816	45	1
1789	111	22	1817	44	3
1790	84	4	1818	34	1
1791	53	6	1819	22	5
1792	47	1	1820	9	3
1793	40	2	1821	4	1
1794	34	1	1822	3	0
1795	22	1	1123	1	1
1796	15	3	1824	7	2
1797	8	1	1825	17	0
1798	4	0	1826	29	0
1799	10	1	1827	40	1
1800	18	0			

This table is of peculiar value with regard to the many questions at present under discussion in connection with sun-spots.

SUN-SPOTS AND THE PREDICTION OF THE WEATHER OF THE COMING SEASON AT MAURITIUS.—In the *Monthly Notices*, new series, No. 1, of the Meteorological Society of Mauritius (December 21, 1876), Mr. Meldrum gives a clear and interesting summary of his researches into the relations of sun-spots to several atmospheric phenomena. A valuable table appears on p. 14 setting forth the number of cyclones which have occurred in the Indian Ocean between the equator and 34° lat. S. each year from 1856 to 1875, the total distances traversed by these cyclones, the sums of their radii and areas, their duration in days, the sums of their total areas, and their relative areas. The well-known thoroughness with which the Meteorological Society of Mauritius has worked at the storms of the Indian Ocean ensures that the subject has been exhaustively treated. The period embraces two complete, or all but complete, sun-spot periods, the former beginning with 1856 and ending 1867, and the latter extending from 1867 to about the present time. The broad result is that the number of cyclones, the length and duration of their courses, and the extent of the earth's surface covered by them all reach the maximum in each sun-spot period during the years of maximum maculation, and fall to the minimum during the years of minimum maculation. The peculiar value of these results lies in the fact that the portion of the earth's surface over which this investigation extends is, from its geographical position and what may be termed its meteorological homogeneity, singularly well fitted to bring out prominently any connection that may exist between the condition of the sun's surface and atmospheric phenomena. A drought commenced in Mauritius early in November, 1876, and when the paper was read on December 21, Mr. Meldrum ventured to express publicly his opinion that probably the drought would not break up till towards the end of January, and that it might last till the middle of February, adding that up to these dates the rainfall of the island would probably not exceed 50 per cent. of the mean fall. This opinion was an inference grounded on past observations, which show that former droughts have lasted from about three to three and a half months, and that these droughts have occurred in the years of minimum sun-spots, or at all events in years when the spots were far below the average, such as 1842, 1843, 1855, 1856, 1864, 1866, and 1867, and that now we are near the minimum epoch of sun-spots. It was further stated that the probability of rains being brought earlier by a cyclone was but slight, seeing that the season for cyclones is not till February or March, and that no cyclone whatever visited Mauritius during 1853–56 and 1864–67, the years of minimum sun-spots. From the immense practical importance of this application of the connection between sun-spots and weather to the prediction of the character of the weather of the coming season, we shall look forward with the liveliest interest to a detailed statement of the weather which actually occurred in that part of the Indian Ocean from November to March last.

METEOROLOGY IN SOUTH AUSTRALIA.—The publication of the meteorological observations made in this colony, which required to be discontinued in 1870 owing to the heavy pressure of official duties devolving on Mr. Charles Todd in connection with the construction and organisation of the Overland Telegraph, was resumed in an extended form in January, 1876, and we have now before us the first nine monthly issues, which bring the publication down to the end of September last. The reports detail, with some care, the conditions under which the observations are taken, the three or six daily observations made, and full *résumés* of the monthly results. An extremely valuable part of the reports is the monthly table of the rainfall at upwards of eighty stations, as observed by the officers of the postal and telegraph departments, and a number of volunteer observers who have co-operated with Mr. Todd in observing the rainfall for many years. The stations are arranged in geographical order from north to south, commencing with Port Darwin on the north coast, and along with the monthly amounts there are also given the averages of the month at all those places at which at least seven years' observations have been made. Among the many points of interest offered by these tables are the torrential rains of the north coast in the first three months of the year, frequently rising to from ten to sixteen inches in the month, their rapid diminution on advancing inland to Barrow's Creek or Alice Springs, and the great diminution in April, and the rainless, or all but rainless character of the northern region from June to September, when the prevailing winds of Australia become decidedly continental, or blow from the interior seawards. Since it would be impossible to over-estimate the importance of barometrical and thermometrical observations from this extended network of stations in South Australia, we very earnestly hope that the Colony will soon take steps to obtain these observations and publish them in the interest of meteorology.

RAINFALL OBSERVATIONS IN THE EAST OF FRANCE FROM 1763 TO 1870.—In the *Bulletin Hebdomadaire* of the Scientific Association of France, of the 10th instant, Prof. Raulin gives an interesting historical account of all the rainfall observations made during these 108 years anywhere in that section of France which is marked off by lines joining Givet on the Meuse, Lauterbourg on the Rhine, Belley near the Rhon, and Decize on the Loire, and which thus comprehends seven well-marked regions, viz., the plain of Alsace, the chain of the Vosges, the plateaux of Lorraine and Bourgogne, the plains of Champagne and Bresse, and finally the chain of the Jura mountains. During the past three years Prof. Raulin has been engaged collecting all available materials for a monograph on the rainfall of this part of Europe, which, judging from his great monographs of the rainfall of other sections of France and of the rainfall of Algeria, will doubtless take its place as a permanent contribution of very high value to meteorological science.

Iowa Weather Report.—We observe from a circular issued by Prof. Gustavus Henrichs to the volunteer observers of Iowa (U.S.), that his report of the observations made at the meteorological stations of that State during 1876 is to be published as an Appendix to the *Report of the Iowa State Agricultural Society*, and that as the monthly reports are published in fully twenty of the newspapers, the *Weather Review* will be discontinued. The *Weather Report* about to be published will embrace an account of the meteorological system now in full operation over the State, and discussions of the rainfall, storms, and other phenomena, the normals which have been ascertained for different localities, and the detailed observations made at the Central Weather Station.

OUR ASTRONOMICAL COLUMN

The Saturnian Satellite, Hyperion.—Prof. Asaph Hall, in *Astron. Nach.*, No. 2,137, publishes an ephemeris of this faint object about the approaching opposition of Saturn, with the view to facilitate observations, especially near the conjunctions. He remarks that although the satellite was discovered (by Bond and Lassell) nearly thirty years since, the difficulty of observing it has been so great that no satisfactory determination of its orbit has been practicable; most of the observations being made near the elongations, the position of the plane of the orbit is not accurately deducible therefrom, though it probably does not coincide with the plane of the ring, but appears to lie between those of Titan and Japetus. With the view to assist observation in the present year Prof. A. Hall has calculated elements from his observations in 1875, which may be stated as follows:—Perisaturnium passage, 1875, August 24·0036 mean time at Washington; distance of perisaturnium from the node 40° 0′, eccentricity 0·125, semi-axis major 214″·22, period of revolution 21·3113 mean solar days. For the reason stated above it is supposed for this approximate orbit that its plane coincides with that of the ring, the node of which on the earth's equator is assumed to be in 126° 9′·1, and its inclination thereto 7° 3′·8. From these data auxiliary quantities and an ephemeris for Washington midnight, August 1–September 15, are added, and it is suggested that with the aid of the former comprising the interval June 1–December 28 a more accurate calculation may be made by Mr. Marth's formulæ.

Taking the solar parallax at 8″·86 Prof. A. Hall's elements would give for the mean distance of Hyperion from the centre of Saturn 914,000 miles, distance in perisaturnium 800,000, in aposaturnium 1,028,000 miles.

The first computation of the orbit of this satellite was by the late Prof. G. P. Bond, of Cambridge, U.S., from his distances observed between 1848, September 19, and January 12 following; his period of revolution is 21·18 days, mean distance 214″, eccentricity 0·115; the elements will be found in the *Proceedings* of the American Academy of Arts and Sciences.

The Triple-Star 7 Camelopardi.—The third component of this triple star was detected by Baron Dembowski on September 28, 1864, having been overlooked by Struve at Dorpat, who measured A and B in 1831, his mean result being 1831·57, pos. 238·32°, dist. 25·647″ The Galarate epoch for the new companion is (A C) 1865·33 pos. 308°·83, dist. 1″·245. Baron Dembowski says the object was one of great difficulty for his refractor principally on account of the sombre hue of the star C, which did not appear always of the same intensity; referring to his observations at the epoch 1865·25, he remarks, "Elle avait alors une couleur de cendre mouillée; je n'ai jamais vue d'étoile aussi sombre.' His magnitudes of C in 1864-5 vary from 7·0 to 9·0, while in the middle of November, 1865, he could not perceive the least trace of the star. Mr. Crossley measured A B at the end of December, 1873, but has no reference to the third star. The object will be worth watching on the score of variability and the unusual duskiness noted by the Galarate observer.

The Cape Astronomical Results for 1874.—Mr. Stone has just circulated his volume of observations made at the Royal Observatory, Cape of Good Hope, in 1874, being the thirteenth separate publication which has emanated from this important and active astronomical establishment since the year 1871, when Mr. Stone undertook its direction. We believe there is not a refinement in observing or computing which is not introduced into the Cape work, and the results have consequently a very high value, comparable with the best work of the kind published by the great European and American observatories, where attention is given to stellar astronomy. The volume for 1874 contains the mean positions of 1,246 stars, including all Lacaille's stars of the *Cœlum Australe Stelliferum*, which now fall between 155° and 165° of north polar distance, and some additional ones in the same zone. Lacaille's stars between N.P.D. 145° and 155° were similarly observed in the course of the year 1875, and those between N.P.D. 135° and 145° in 1876, the reductions to mean places for the former zone having been completed at the beginning of the present year. A complete determination of the accurate places of all Lacaille's stars, founded on the Cape observations, is therefore in a very forward state.

As an appendix to this volume of Cape Observations, Mr. Stone presents tables intended to facilitate the computation of star-constants, which appear likely to prove of very great service to the practical astronomer. By a slight modification of Bessel's form for star-corrections he has been able to tabulate the quantities in a very convenient and compendious manner, so that the whole computation occupies but a short time. Mr. Stone hopes that the use of these tables may render it unnecessary to give star-constants for every star contained in future catalogues, the labour of forming which, and of insuring their accuracy is very great. It is probable, as he observes, that the use of star-constants in various catalogues has been in many cases extended beyond the time when they could be introduced with a due regard to the precision required in modern stellar astronomy, which will be obviated by the use of the tables in question. It is understood that Mr. Stone liberally offers to supply a copy of these tables to anyone who would find them of real service, and who will make application for them. A few remarks on the *modus operandi* with the tables are reserved for a future column.

THE BRITISH ASSOCIATION AT PLYMOUTH

FEW towns in the United Kingdom have so much to interest alike the scientific and the general visitor as Plymouth; and the meeting there of the members of the British Association in August next should prove alike pleasant and profitable. For the general visitor it will perhaps be enough that the Plymouth Hoe is one of the finest promenades in England, and that the landscapes of the neighbourhood are at once most varied and most attractive. The man of science will be able to enjoy all this and a good deal more. The zoologist may if he pleases revel in dredging expeditions in and off the Sound, which are sure to yield an ample reward. For the mechanician there are three of the most notable works of modern engineering skill to inspect—the Eddystone Lighthouse, the Plymouth Breakwater, and the Royal Albert Bridge, while the Government dockyards and factories at Devonport and Keyham, and the war vessels which stud the Hamoaze, will have a general as well as a special interest. One of the most enjoyable excursions of the Exeter meeting was that to the Three Towns, on which occasion the Government establishments were visited and gunnery and torpedo practice, with all the latest electrical arrangements, witnessed on board the *Cambridge*. The science of war has by no means stood still since then. The botany of the locality presents some peculiar features, and the algology is very rich.

In the domain of natural science special interest however attaches to the local geology. Plymouth rivals Torquay in its development of the great Devon limestone, which lines the northern borders of the twin estuaries of the Tamar and the Plym, along which the Three Towns are built, and trending southward and eastward, occupies the northern shores of Cattewater, and after a break reappears in mass at Yealmpton. The Hoe is limestone—a natural esplanade, an ancient plateau of denudation, with occasional alluvial deposits of sand and clay in pockets and fissures, remains of raised beaches, and a few ossiferous cavities. The limestone abounds in fossils, coralline in the more massive portions as a rule, but with areas crowded with molluscs of the ordinary Devonian type. Its chief palæontological interest lies, however, in its bone caves. The ossiferous caverns of Oreston, a little village on the southern bank of Cattewater, which were discovered originally in the course of quarrying the stone for the breakwater, whilst other members of the series have been opened from time to time since, are well known by description at least to geologists. Those of Yealmpton have hardly attracted so much attention. The fauna differs in both series in some important particulars from that of Kent's Cavern, though including in each case the ordinary cavern carnivora. The whole literature of the Oreston and Yealmpton caverns will be found in the *Transactions* of the Devonshire Association, compiled by Mr. Pengelly. And if the palæontologist should then feel special interest in a locality which has yielded so much to his branch of science, the stratigraphical geologist will find some notable materials for the study of the "still-vexed Devonian question" in the sections along the eastern shore of the Sound and elsewhere. The cliff section from Mount Batten, by Staddon Heights and Bovisand to the mouth of the Yealm has been described by Sedgwick, Murchison, de la Beche, Phillips, Holl, Pengelly, Jukes, and other eminent geologists, and interpreted very diversely, though the balance of opinion still remains that its shales and sandstones overlie the limestone. The contortions and plications are, however, in some parts very remarkable, and should be studied *in situ*.

There is nothing very noteworthy in the immediate mineralogy of Plymouth, but the mining districts of Cornwall and Devon, within easy reach, are the richest mineralogical field in the kingdom, and in the barrows circles, cromlechs, pounds, dolmens, and menhirion, still scattered in profusion over the wild flanks of Dartmoor, and along many a Cornish moorland, the anthropologist will find plenty to delight him. Upon the importance of the contributions of Kent's Cavern to the early history of man we need not dilate. The results of the explorations there, with the literature of the cavern, prepared by the indefatigable pen of Mr. Pengelly, will be found in the Devonshire Association *Transactions*.

The Plymouth Institution, with which is amalgamated the Devon and Cornwall Natural History Society, and which fittingly took the initiative in proposing the invitation of the Association, is the centre of the scientific life and work of the neighbourhood. It is a society of some standing, for it was founded so far back as the year 1812, and its members have done much to elucidate science in its connections with the district, and to cultivate literature and the fine arts. The natural history section of its museum is rich in local ichthyology, and fair in some other departments of its fauna. There are some very valuable antiquities; and the mineralogical and geological collections, though far from complete, are by no means wanting in interest. Bones from the ossiferous fissures on the Hoe, the caverns at Oreston and Yealmpton, and from Kent's Hole, form a prominent feature of its palæontology; and there are a few specimens which have a special value in having been presented by the Rev. Richard Hennah, who first established the fossiliferous character of the Plymouth limestone. The Institution issues *Transactions*, and has published some valuable papers bearing alike on science and upon local history, topography, and literature, from the "Law of Electrical Accumulations," by Sir W. Snow Harris, F.R.S.; to a paper "On the Letter R," by R. F. Weymouth, D.Lit. It will be evident, therefore, that the institution has been doing good work.

But now for some particulars concerning the local arrangements. These are in the hands of a large and influential executive committee, with sub-committees for the chief departments—finance, reception, sectional, excursion, fine art, &c. The mayor is the chairman of the executive; the secretaries being Messrs. W. Adams, W. Square, and H. Whiteford, while Mr. F. Hicks is the treasurer.

In one respect, and that a most important one, Plymouth will distance almost every town the Association has visited. We allude to the convenience of its sectional accommodation. The great hall of the Plymouth Guildhall, with its royal statues and magnificent historic windows, is the noblest hall in the whole south and west of England. Here the president will deliver his address and the evening meetings be held. In the law courts adjoining, and the spacious rooms of the municipal offices, some of the sections will be accommodated. Others will meet at the Mechanics' Institute, the Athenæum, and the Royal Hotel, the whole of which are within less than five minutes' walk of the Guildhall and each other. Since one or two of the other section rooms were decided on, it has been suggested that the sections to which they were appropriated may also be accommodated within the limits first indicated; but whether that be so or not, in the most remote case the most distantly located sections will only be six or seven minutes' walk apart. The members of the Association will know how to appreciate this.

Close by the Guildhall is St. Andrew's Hall, a large building recently erected as a skating rink. This will be utilised in connection with the Association for an exhibition of the fine arts. Plymouth is the artistic centre of Devon and Cornwall, which have given birth to many famous painters, and the exhibition is intended to be specially representative of western art. The Queen is among the contributors, and leading residents throughout the two counties. Living artists will be well represented; but the staple of the exhibition will consist of examples of Reynolds, Opie, Eastlake, Prout, with Haydon, Northcote, and other artists of note. With the exception of Opie, who was a Cornishman, and Reynolds, who was born at Plympton, four miles off, the artists here named are Plymothians.

Every effort is being made to get up an enjoyable and scientifically interesting series of excursions. It is perhaps rather a disadvantage in one way that the neighbourhood of Plymouth should be so beautiful, for therein lies a strong temptation to let fine scenery get the better of hard science. However, it so happens that there is very little difficulty in combining both. In 1841 there was but one excursion—to Tavistock and Wheal Friendship. This year there are six proposed—three for the Saturday and three for the Thursday following, in addition to which the Earl of Mount-Edgcumbe has most kindly consented to open his magnificent park on the Saturday to the members. The botanists will need no excuse for visiting Mount-Edgcumbe; if the geologists do they may find it in the interesting intrusive rocks at Cawsand, referred to in De la Beche's Report. One of the excursions proposed for Saturday is by steamer to the breakwater, and Smeaton's famous work, the Eddystone Lighthouse, winding up with a trip round the harbour, with its men-of-war, dockyards, forts, and factories. The Government establishments are always open to English folk. Our foreign friends who may desire to go over them, will have to provide themselves with a special order. There will be also a trip to Liskeard, for the Caradoc and Phœnix mines, and the famous Cheesewing. As mining is the special industry of Cornwall, and to a great extent of South Devon, it has been thought desirable to have two mining excursions—one on each day. South Caradoc is one of the richest copper mines in Cornwall; Phœnix is a tin mine; and both are admirably managed and excellently adapted to illustrate mining operations. The mineralogy of this district has some peculiar features. Phœnix has lately yielded the rare minerals chalcosiderite, andrewsite, and the beautiful turquoise-hued henwoodite. The third excursion will be to the Lee Moor China clay works. These are situated on the skirts of Dartmoor, not far from Plympton, are of immense size, and afford probably the best illustration of this great industry, which Cornwall and Devon owe to the researches and ingenuity of Cookworthy, chemist and potter, manufacturer in the Plymouth china of the first true English (hard) porcelain. It is likely that this excursion will be taken up by the Plymouth Institution, and so arranged as to embrace a visit to Princetown, and its convict prison, and some of the fine prehistoric antiquities of Dartmoor; if not there will probably be an extra excursion with this object given by the institution.

Thursday will be a long day, and wholly given up to excursion pleasures. The mining excursion will be up the lovely river Tamar to Devon Great Consols, which communicates by a railway of its own to shipping quays at Morwellham, in the close vicinity of the most picturesque scenery of the Tamar valley. On the way, by the kindness of the Countess Dowager and the Earl of Mount-Edgcumbe, the party will have an opportunity of inspecting Cotehele, one of the most perfect examples of a mediæval mansion now extant. At Devon Consols—not long since the largest and richest copper mine in the land, which gave in dividends considerably over a million—not only are mining operations conducted on the most extensive scale, but there are enormous arsenic works, huge water-wheels, and many other objects of interest. The other excursions arranged for the day are to Torquay and Penzance. The good people of Torquay intend to follow the capital precedent set in 1869, and to invite and entertain a number of guests. *En route* from Plymouth a steamer trip may be made down the lovely river Dart; and at Torquay there are plenty of objects of interest. The Torquay Natural History Society has a well-stored museum; Kent's Cavern is of course a museum in itself, with a very Cerberus of a curator in Mr. Pengelly; and then there are the works of Mr. Froude, F.R.S., at Chelson Cross, where he conducts those delicate experiments for the Admiralty on the forms of ships and their properties of stability, and to which he intends to invite members of the Association who are specially interested in this branch of mechanical science. Steps are, we believe, being taken at Penzance to give the excursionists thither a hearty welcome. The museums of the Penzance Natural History Society and of the Royal Geological Society of Cornwall, the latter of which contains the best public mineralogical collection in the West of England will be thrown open to them, and excursions in all probability organised to the chief attractions of the neighbourhood. It is hoped to provide special railway facilities for those who may wish to visit other parts of the country—such as Tintagel or the Lizard, or the western mining district. At Truro is the excellent museum of the Royal Institution of Cornwall, which will be open to visitors.

The former meeting at Plymouth, of the Association, was in 1841, with Dr. Whewell, as president, and was a very successful gathering. Six-and-thirty years are a long time, and it is remarkable that so many who took a prominent part on that occasion are yet with us. One of the vice-presidents still survives—the Earl of St. Germans; two of the local secretaries, Mr. R. W. Fox, F.R.S., and Mr. R. Taylor, F.G.S.; a vice-president of the statistical section, the Earl Fortescue, then Viscount Ebrington; Dr. Owen, F.R.S., vice-president for Zoology and Botany; and Mr. Robert Hunt, F.R.S., then secretary of the section of Chemistry and Mineralogy, are still with us. There will not be wanting opportunity, therefore, of comparing personal experiences in 1841 and 1877.

INDIAN RAINFALL AND SUN-SPOTS

ON May 24 Gen. Strachey read a paper before the Royal Society entitled "On the alleged Correspondence of the Rainfall at Madras with the Sun-Spot Period, and on the True Criterion of Periodicity in a Series of Variable Quantities."

He stated that a paper had recently been printed by Dr. Hunter, the Director-General of Statistics to the Government of India, having for its object to show that the records of the rainfall at Madras, for a period extending over sixty-four years, establish a cycle of rainfall at that place which has a marked coincidence with a corresponding cycle of sun-spots—the rainfall and sun-spots attaining a minimum in the eleventh, first, and second years, and a maximum in the fifth year.

The Madras register extends over sixty-four years, beginning with 1813. The mean rainfall for the whole period is 48·5 inches. The deviations from the mean vary from 30·1 inches in defect to 39·9 inches in excess. The arithmetical mean of these deviations (disregarding the signs) is 12·4 inches.

Dr. Hunter divides the sixty-four years' observations into six cycles of eleven years, and calculates the arithmetical mean of the successive years of the whole series of cycles. The results are as follows:—

	Years of cycles of eleven years.										
	1st.	2nd.	3rd.	4th.	5th.	6th	7th.	8th.	9th.	10th.	11th.
Average difference from the mean of 64 years.	in. +0·6	in. +0·7	in. +9·8	in. +2·4	in. +1·9	in. +5·8	in. +4·4	in. −3·4	in. −11·5	in. +0·7	in. −13·5

In the above calculation the first year of the cycle of eleven is 1813, so that the average period of maximum sun-spots will be about the third or fourth year of the cycle, and the period of minimum will be about the tenth or eleventh of the cycle. This table apparently indicates a period of maximum between the third and the seventh years, and of minimum between the eighth and the second years.

But as the only signification of the arithmetical mean value of a series of observed quantities is that it is one above and below which there is an equal amount of deviation in the individual observations, the question whether or not the mean values thus obtained can be accepted as showing a definite law of variation from year to year in the cycle must be determined by examining the differences between those means and the individual observations on which they are based.

Treating the observations in this manner, it appears that the mean difference of the individual observations from the means shown in the table amounts to 11·2 inches, and differs but little from the mean difference of the individual observations from the arithmetical mean of the whole series. In other words, the supposed law of variation obtained from the means of the six eleven-year cycles hardly gives a closer approximation to the actual observations than is got by taking the simple arithmetical mean as the most probable value for any year.

In order to obtain a practical test of the probable physical reality of the cycle of eleven years, the author calculated a series of mean values corresponding to those given in the table for a series of cycles of five, six, seven, eight, nine, ten, twelve, and fourteen years. The mean differences between these means an the observed quantities are all within a very small fraction of o another, and of the mean obtained from the eleven-year cycle—

in short, one cycle is in this respect almost as good or as bad as another.

Now, if in any series of quantities, such as the rainfall observations at Madras, there be a law of periodicity, each observed quantity may be supposed to be compounded of a periodical and a non-periodical element. If we take the sum of a large number of cycles, each of which coincides with the cycle of periodicity, the non-periodical elements will tend to be eliminated, and the means for the successive years of the cycle will indicate the periodical elements for the successive intervals. At the same time the differences of these means from the several original quantities from which they were obtained will be the several non-periodical elements.

In proportion as the periodical elements are small or large in relation to the corresponding non-periodical elements, so the differences (obtained as above) will be inversely less or more different from the differences between the individual observations and the mean of the whole of them; and if there be no periodicity, the two sets of differences would, in a sufficiently long series, be identical.

Hence it may be inferred that when the differences (taken as before) closely approximate in magnitude to the mean difference of the original observations from the arithmetical mean of all of them, the periodical elements in those observations must be correspondingly small; and this applies manifestly to the eleven year-cycle and to the whole of the arbitrary cycles for which the differences were calculated.

Further to test the reality of the periodicity, the author rearranged the series of sixty-four years' observations, in a purely arbitrary manner, in cycles of eleven years, by drawing the actual observations at random one after another, and setting them down in succession till the whole were exhausted. From three arbitrary cycles thus prepared, the differences averaged 10·9, 11·2, and 11·6—results which again indicate that the actual sequence of the observed quantities of rain has no material effect on the mean differences, or any such tendency to a diminution in their numerical value, which is the necessary accompaniment of a true periodical element.

Moreover, the mere circumstance of any series of cyclical means showing a single maximum and single minimum gives no real indication of such a result being a truly periodical feature. It is obviously to argue in a circle, first to assume a cycle on which to work, which shall give a single maximum and minimum, and then to infer that there is true periodicity because of the single maximum and minimum. The test of the periodicity is in truth to be sought altogether outside of the particular values of the successive elements of the cyclical means.

It is manifest that a complication of periodical elements may so mask one another as to prevent positive results being obtained by the examination of the means and differences in the case before us. But the whole scope of the present argument is negative, and it leads to the conclusion that there is no proof of greater tendency to periodicity in the eleven-year means than in the original isolated observations.

As the sun-spot period is not exactly a cycle of eleven years, the author examined the results obtained by a comparison of the observations corresponding to the known periods of maximum and minimum sun-spots, without reference to any special length of cycle. These results he also considered to be negative.

A further test of the character of the conclusions was sought from the rainfall observations at Bombay and Calcutta, which have been made for the greater part of the period over which those at Madras extend. It is hardly conceivable that there should be a coincidence with the sun-spot period, such as is supposed to have been found at Madras, based on any physical cause, which should not in some way be discernible in the rainfall at Bombay and Calcutta.

The results thus got are also held to be entirely negative, and to indicate no concordance among the means of the several years of the cycle at the different places. The Bombay and Calcutta observations, treated as those of Madras were, to ascertain the deviations of individual observations from the successive means of the cycle, give quite similar results.

Although the special object of the communication was to deal with the alleged correspondence between the Madras rainfall and the sun-spot periods, the author had also turned his attention to Mr. Meldrum's speculations of a similar character, and had tested some of them in the manner explained.

Among these were the Greenwich observations for fifty-five years, which will be found at p. 307 of vol. xxi. of the *Proceedings* of the Royal Society, and the results got from them were quite analogous to that obtained from the Indian observations.

Further, to illustrate the argument on which the paper was based, the case was considered, in which a well-ascertained periodicity exists, as that of the diurnal barometric oscillation. The figures used were taken at random from an old Madras register, the intervals being made two-hourly, and the entries and the differences of the observed barometric heights from the daily means in thousandths of an inch, so as to reduce the calculations.

The figures being merely illustrative, the circumstance of their not exactly representing any physical phenomenon was a matter of no significance.

The treatment of these figures led to results very different from those got from the rainfall observations. The mean difference of all the supposed observations from the mean of all of them being thirty, the mean of the differences between the two-hourly means and the original figures was reduced to seven, indicating the distinct presence of a periodicity.

Re-arranging the figures in an arbitrary cycle of ten periods instead of twelve, the mean of the differences which before was seven was increased to thirty, showing that with the total destruction of the periodicity the mean difference of the two-hourly means and the original figures was the same as the mean difference with the arithmetical mean of all of them.

In conclusion, the author specially explained that he did not call in question the possible or actual occurrence of terrestrial phenomena corresponding to the sun-spot period, but only desired to point out that in the case of the rainfall observations under review the evidence was not sufficient to establish either any periodicity or such a correspondence.

In some remarks made subsequently the author pointed out how the comparison of the successive combination of the observations, beginning with one cycle and then combining two, and so on, till the whole were united, supplied another way of treating the figures which showed that the successive means of the differences between the mean rainfall for the combined cycles and the mean for the several years of the cycle when combined, followed the law that would hold good if there were no appreciable periodicity, that is to say, that this mean should gradually diminish in a ratio inverse to the square root of the number of cycles combined.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Prof. Humphry has been appointed to represent the University at the 400th anniversary of the University of Upsala in September next.

Prof. Adams' report to the observatory syndicate for the year from May, 1876, to May, 1877, does not contain anything of unusual interest. The year has been exceptionally favourable for astronomical observations, and 3,618 observations were made with the Transit circle. All the publications of the observatory are well forward, and the general work has been carried on smoothly and efficiently.

The following awards have been made for proficiency in natural science at St. John's College:—To F. T. S. Houghton, a foundation scholarship, the Wright's Prize, and augmentation of exhibition to 100*l.* for the past year. To Marr, Slater, F. J. Allen, Stewart, augmentations of exhibitions.

LONDON.—A public meeting in support of the London School of Medicine for Women was held the other day at St. George's Hall, the especial object being to raise 5,000*l.*, with a view of enabling the Executive Council to carry out an arrangement with the authorities of the Royal Free Hospital, under which students from the school were to receive clinical instruction. Mr. Cowper-Temple, M.P., Mrs. Garrett-Anderson, and Mrs. Westlake were amongst the speakers. 2,600*l.* have already been subscribed.

The Senate of the London University have decided by a majority of five not to postpone giving medical degrees to women till all the other faculties were open to them.

MANCHESTER.—On Friday last the scholarships and prizes gained during the session by students in the Faculties of Arts, Science, and Law were distributed at the Owens College. The Dalton Senior Mathematical Scholarship was gained by J. P. Whitney; the Dalton Junior Mathematical Scholarship by J. D. Pennington; the Dalton Chemical Scholarship by J. K. Crow; the Platt Physiological Scholarship by L. Larmuth. Mr. Crow presented a research upon the "Hypovanadous Compounds," and Mr. Larmuth a research upon the "Physiological Action of certain Vanadium Compounds." The number of students in the various departments of the College during the session just closed has been—Arts, Science, and Law, 415; Medicine, 175; Evening Classes, 900; making a total—allowing for double entries—of 1,450, being an increase of seventy on the number registered during the session of 1875–76.

LEEDS.—On Saturday the prizes and certificates awarded to the students at the termination of the third session of the Yorkshire College of Science were distributed by Lord Frederick C. Cavendish, the President of the College. Thoroughly gratifying reports of progress were made by all the professors. Not only has a site been obtained for the erection of permanent buildings, but an architect has been appointed to prepare plans and superintend the erection of the buildings. The first step has been taken to provide what we have all along advocated, a complete curriculum in literature as well as in science, by the appointment of Mr. Marshall as classical professor, and it is hoped that in a very short time a professor of modern literature will also be added to the staff. Liberal contributions have already been made towards the great expenses required to start the institution, and the University Extension Committee have handed over the whole of the funds of which they had been made administrators in connection with Leeds. Still to attain anything like efficiency the sum already obtained must be doubled. It is the people of Yorkshire who will mainly benefit by this new institution, and we hope it will not be difficult to convince them that it is both their interest and their duty to provide the greater portion of the funds required.

DURHAM.—At a recent Convocation the following degrees and licences were conferred in connection with the University:—B.Sc.: John Thomas Dunn, Mather Scholar. Associates in Physical Science: Edwin Cooke, John Richard Hutchinson Williamson.

PARIS.—The reconstruction of Charlemagne College, one of the most celebrated national colleges in Paris, has just been finished. The fitting-up of the buildings has begun, and they will be ready by October next. This college was established after the Revolution in the Hotel d'Anville, rue St. Antoine, which had been purchased from Anne de Montmorency by the Cardinal of Bourbon, and bequeathed by him to the Jesuits, then in hostility with the University.

A large number of houses having been pulled down in the Quartier Latin to make room for the Boulevard St. Germain, the works for the enlargement of the School of Medicine have been begun, and will be completed before the Exhibition. The expense will be 2,838,000 fr.

NOTES

THE varied and cultured tastes of the Emperor of Brazil are unusual even among private individuals, and probably without a parallel among his own limited class; his activity and eagerness for knowledge are astonishing. While in Paris, as we stated at the time, he was present at almost every scientific meeting of any importance, and in London this interest in science manifests itself quite as strongly. He has attended every meeting of the Royal Society, since his arrival, was present at Mr. A. R. Wallace's lecture, carefully inspected the Science School at South Kensington, called the other day on Mr. Crookes, visited Dr. Siemens on Tuesday and Mr. Spottiswoode on Wednesday, and indeed has conversed with almost every man of science in London who has been doing any original work during the past few years. These visits are not mere formalities, for the Emperor is not satisfied until he masters whatever new research is submitted to him. On Tuesday he was made an honorary member of the Anthropological Institute and of the Royal Historical Society. Were the Emperor to stay here for some time we believe his presence would have a distinct influence on the public recognition of science; and if there were any one in this country in a similar station who took an equally real interest in science, we believe it would be all the better.

UNDER the auspices of the Sanitary Institute, Dr. Richardson, F.R.S., will deliver a lecture at the Royal Institution, Albemarle Street, on Thursday next, at 4 P.M., "On the Future of Sanitary Science in relation to Political, Medical, and Social Progress." We hope to give a verbatim report of this lecture in next week's number.

THE last meeting of the Royal Society previous to the recess was held last Thursday.

WE regret to see that Poggendorff's name has been entirely suppressed in the title-page of the new volume of the *Annalen*. The journal now edited by Borchardt still bears Crelle's name, and our *Philosophical Magazine* still keeps the names of Tilloch, Nicholson, and Thomson on its title-page. A similar allusion to the man who has made the *Annalen* what they are, would have been a better tribute to his memory than the short account of his life which closes what we must now call the last volume of *Poggendorff's Annalen*.

THE twenty-sixth meeting of the American Association for the Advancement of Science will be held at Nashville, Tennessee, commencing August 29, 1877. The president at this meeting will be Prof. Simon Newcomb; the permanent secretary is Prof. F. W. Putnam.

THE Geologists' Association have arranged for an excursion into Derbyshire on Monday, July 23 and five following days, under the direction of the Rev. J. M. Mello, Prof. Boyd Dawkins, and Mr. Rooke Pennington.

THE Select Committee to which the Ancient Monuments' Bill was referred met on Monday, when Sir John Lubbock was chosen chairman. The Committee meets again on Monday next, when evidence will be taken.

AT the meeting of the Royal Geographical Society on Monday, the last of the three lectures on scientific geography arranged for this session was given by Mr. A. R. Wallace, "On the Comparative Antiquity of the Continents." The object of the lecture was to establish the comparative antiquity of continents by an examination of the living and extinct animals found in each, and the lecturer came to the general conclusion that the main divisions of the earth had been nearly the same from the earliest period. The Emperor of Brazil was present during the lecture.

IN our report of the Anniversary Meeting of the Geographical Society it was stated that the Society contemplated organising an African Exploration Fund. The Society, it is known, has taken no share in the International African Association founded by the King of the Belgians; while doing everything to forward the views of that association, it seems to be of opinion that England ought to carry out African exploration independently. The Prince of Wales has become patron of the African Fund, and a special committee has been appointed, with the president of the society as chairman, the society having given a special donation of 500*l.* A map accompanying the programme of the scheme shows how large an area has been explored by British travellers, and several routes in Eastern Africa are suggested for exploration or careful examination. A comparison has been made of the length of each journey in Africa in a few recent instances, with the cost of making it. It appears that the total expense of despatching a well-equipped exploratory expedition from England may be roughly reckoned at the rate of 1*l.* 10*s.* for each geographical mile of country travelled over in Africa, supposing the expedition to return to the place whence it set out. In through journeys the rate is in many cases nearly twice as great. The aggregate length of the seven specified routes is about 7,700 geographical miles; consequently, the total cost of the proposed explorations, at the above rate, would amount to about 11,550*l.* No doubt many besides fellows of the society will be willing to help forward this new scheme. In connection with this we may here state that the International African Commission has concluded its labours. It has decided that the organisation of stations in Africa belongs to the Executive Committee. The principal object of these stations will be the suppression of the slave trade. It has also been decided that an expedition shall leave Zanzibar in the direction of Lake Tanganyika. The King of the Belgians has been re-elected president of the Commission.

Trübner, of Strassburg, publishes this month the first number of a new *Zeitschrift für physiologische Chemie*, edited by Prof. Hoppe-Seyler, of Strassburg and other eminent German chemists. The purpose of the journal is to keep together such original papers in physiological chemistry as are now scattered over various chemical, physiological, and medical journals. The new *Zeitschrift* will be published every two months.

IT is stated that in a field near Cologne the Colorado potato beetle has been found in every stage of development.

IN connection with the agitation in favour of planting the streets and squares of Manchester with trees, and protecting the suburban vegetation, which is being carried on by the Field Naturalists and Archæologists' Society of that city, a paper by Mr. R. H. Alcock, F.L.S., containing some curious information, was read at the last meeting of the Society. Mr. Alcock has experimented for the last twenty-five years in planting trees in the vicinity of his mill, situated in Bury, a smoky manufacturing town, a few miles from Manchester. He finds that the plane (*Platanus orientalis*), which is so successful in London, will not grow at all in Lancashire smoke even with careful culture. But on the other hand he has been very successful with the beech, sycamore, birch, wych elm, and Turkey oak. The lime, however, is the tree chiefly recommended: indeed, Mr. Alcock says of it that he is absolutely certain it will grow well in the Manchester thoroughfares if properly planted. If the Manchester people are enabled to walk *unter den Linden*, they will have reason to thank the Society which is making such commendable efforts to solve the problem in question.

IT is just 140 years since the National Library of Paris was made public. The area of the building has been enlarged more than twenty times since that, and a scheme is now being planned for isolating the building from every other house.

AN important French work has just appeared at Geneva—"Le Massif du Mont Blanc. Étude sur sa Constitution géodésique et géologique, sur ses Transformations et sur l'état ancien et moderne de ses Glaciers," by E. Viollet Le Duc, with 112 illustrations, and a map on the scale of 1 : 40,000. The work is the result of seven years' exploration, during which the author has set himself to map and describe with all possible accuracy, the characters of the rocks and of the soil, the successive beds of glaciers, the positions of moraines, the forms of the *cônes de déjection*, as well as the general aspect of this great upheaval. The map, based on the former maps of Capt. Mieulet, on the well-known relief of Mont Blanc constructed by Bardin, on the surveys of Forbes, on the work of Alph. Favre, and on careful surveys by the author himself, is indeed a remarkable work, scientifically and artistically. After many attempts the author has given up the idea of representing the relief by level curves, and has returned to the old graphic system under a light corresponding to that of the sun about 10 o'clock on a summer morning. In this way the relief of the locality is so perfectly represented on the map as really to deceive the eye. The geological description deserves the attention of all geologists.

MR. E. G. RAVENSTEIN, F.R.G.S., read an elaborate paper at the Statistical Society on Tuesday evening last week, on the populations of Russia and Turkey. The former of these Empires has 84,584,482 inhabitants, the latter only 25,986,868, or, including Egypt, Tripoli, and Tunis, 43,408,900. The population of Roumania is 4,850,000, of Servia 1,352,500. The population of Russia increases at the rate of 1·1 per cent. per annum, the increase amongst the Jews being at least double what it is amongst the Christians. With respect to Turkey there exist no data for calculating the increase, though it is most probable that the dominant race does not increase at all, a fact accounted for by vicious practices, and by the sacrifices demanded from it for the defence of the empire. Some curious facts were commented with respect to the proportions between males and females. Throughout Asiatic Russia and in a considerable portion of European Russia the male sex preponderates. The same fact has been noted in Roumania, in Greece, and in other parts of Europe. The author thus summed up the results of his investigations:—In the Russian Empire there are 100 Russians to every 50 members of other nationalities, and 100 Christians to every 16 Mohammedans and Pagans. In Turkey, on the other hand, 100 Turks have opposed to them 197 members of other nations and 100 Mohammedans, 47 Christians.

MR. STANFORD has just published sheet No. 1 of a large-scale map of the seat of war in Europe. It exhibits with great minuteness the region on both sides of the Danube where operations are being at present carried on, and extends southwards as far as the latitude of Philipopolis. It is admirably executed, and will enable a reader to follow the movements of the belligerents with complete satisfaction. Mr. Stanford also publishes a bird's-eye view, by Maclure and Macdonald, of the seat of war in Asia and Europe from Kurdistan, much better executed than the generality of similar maps.

FURTHER details appear in the American papers of the recent destructive earthquake and wave on the west coast of South America. A Lima correspondent states that at about 8·30 on the night of May 9, a severe earthquake shock, lasting from four to five minutes, moved the entire southern coast, even reaching down as far as Antofagasta. The first shock was succeeded by several others of less intensity, and the sea, receding from the shore, seemed to concentrate its strength for the fearful and repeated attacks it made upon the land. It left Callao and proceeded southward. At Mollendo the railway was torn up by the sea for a distance of 300 feet, since repaired; and a violent hurricane afterwards set in from the south, preventing the approach of all vessels, and unroofing the houses of the town. At Arica the shocks were very numerous, and caused immense damage in the town, the people flying to the Morro for safety. The sea was suddenly perceived to recede from the beach, and a wave from ten feet to fifteen feet in height rolled in upon the shore, carrying before it all that it met. Eight times was repeated this assault of the ocean. Iquique is in ruins. The movement was experienced there at the same time, and with the same force. Its duration was exactly four minutes and twenty seconds. It proceeded from the south-east, directly from the direction of the Ilaga. The town of Tarapatt, twenty-five leagues inland, and the villages of Rica, Matella, and Canchones were more or less damaged. The shock of earthquake was especially severe at Chanavaya. In some spots the earth opened in crevices of fifteen metres in depth, and the whole surface of the ground was changed. At least 200 persons were killed. At Antofagasta the atmosphere was illuminated by a red glare, supposed to proceed from the volcano of San Pedro de Atacama, a few leagues in the interior. The sea completely swept the business portion of the town during four hours. At Huanillion the wave which succeeded the earthquake was nearly sixty feet in height. Mexillones was visited by a tidal-wave sixty-five feet in height. Two-thirds of the town were completely obliterated. In connection with the so-called "Tidal Wave" of the Pacific, Mr. Manley Hopkins writes to yesterday's *Times*:—About the 1st of May last the great crater of Kilauea, on the flank of Mauna-Loa, had become active. On the 4th rather severe shocks of earthquake were felt at the Volcano House. At 3 P.M. that day a jet of lava was thrown up to the height of about 100 feet, and afterwards other jets, to the number of fifty, perhaps, were in operation. Subsequently jets of steam issued along the line formed by a fissure four miles in length, down the mountain side. On the 5th, an observing party finding the disturbance lessened, descended into the vast crater. On the plain which forms the floor of the crater a mamelon had been thrown up 1,400 feet in diameter and 700 feet in height. Fire and scoria spouted up in various places. Pele's hair, vitreous filaments formed in the volcano, abounded. Things returned to a quiescent state. Between 4 and 5 A.M. of the 10th an oscillation of the sea was observed at Hilo, on the east coast of the great southern island of Hawaii. At a quarter before 5 the great "earthquake wave" struck the village. The greatest difference between the crest and the trough of the wave was here, and it measured 36 feet. On the opposite side of the island, in Kealakekua Bay, where Cook died, the measurement was 30 feet. In other localities the difference varied down to 3 feet. The regurgitations of the sea were violent and complex, and continued through the day. The great wave seems to have struck all the islands at the same time without reference to position. The height of the waves was nowhere so great as at Hilo. In 1868 a great earthquake wave destroyed Arequipa and Arica; 30,000 lives were lost at that time. Allowing five hours for the difference of longitude between those ill-fated towns and Honolulu, and supposing that the centre of the seismic action was rightly placed, the wave on that occasion, 1868, was calculated to have travelled the 5,000 miles between Arica and Honolulu in twelve hours, or at the rate of 446 miles an hour.

The shock of an earthquake visited the district between Aix-la-Chapelle and Cologne at about 9 A.M. on Sunday. The movement was from south-west to north-east, and lasted from three to fifteen seconds. The vibration resembled that caused by a heavy goods train. The *Cologne Gazette* remarks that the last earthquake in the Rhine district occurred on November 17, 1868, two days after a considerable eruption from Vesuvius had commenced.

At a recent meeting of the Christchurch (N.Z.) Philosophical Institution, Dr. Haast gave an account of the discovery of remarkable ancient rock paintings in the Weka Pass Ranges. Some of them are fifteen feet long; they represent animals of foreign countries, weapons and dresses of semi-civilised people; underneath are characters like those of the Tamil language, and those on the ancient hill found in the North Island.

The great Moscow Polytechnic Museum was opened on June 12 by an extraordinary meeting of the Society of Friends of Natural Science.

The Irkutsk newspaper *Siberia* announces that on April 28, at 9.30 A.M., an earthquake was felt at Irkutsk. The shock was very short and rather strong.

The second fascicule of the sixth volume of the *Memoirs* of the Kazan Society of Naturalists contains the annual report of the Society. The most important work done by the Society was a geological exploration of the permian and carboniferous deposits along the banks of the Volga, between Stavropol and Syzran, by M. Stuckenberg, and of the banks of the Kama in the Vyatka government, by M. Zaytseff.

The additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus ruber*) from West Africa, presented by Mr. Edward Poulson; a Yellow Baboon (*Cynocephalus babouin*) from West Africa, presented by Mr. H. E. Walters; a Purple Kaleege (*Euplocamus horsfieldi*) from the North-west Himalayas, presented by Mr. John Ditmas; an Imperial Eagle (*Aquila imperialis*), European, a Barrabands Parrakeet (*Polytelis barrabandi*) from New South Wales, deposited; seven Spotted-billed Ducks (*Anas pœcilorhyncha*), seven Chilian Pintails (*Dafila spinicauda*), eight Summer Ducks (*Aix sponsa*), two Bronze-winged Pigeons (*Phaps chalcoptera*), a Geoffroy's Dove (*Peristera geoffroii*), bred in the Gardens; a Hippopotamus (*Hippopotamus amphibius*), born in Holland, purchased.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 31.—"The Physical Properties of Homologues and Isomers," by Frederick D. Brown, B.Sc.

When we attempt to compare the physical properties of a series of compounds presenting very similar chemical properties, we find, that although our knowledge regarding one or two members of the series is tolerably complete, it is very restricted concerning the others.

Among the alcohols of the $C_nH_{2n+1}OH$ series, for example, there are two—methyl and ethyl alcohols—with whose physical properties we are well acquainted, but when we pass to the other members of this series we find, that with the exception of numerous determinations of density and boiling-point, experiments have been limited to the measurements of expansion which have been carried out by Kopp, Pierre and Puchot, and others.

In order to supply this want, I have undertaken a series of experiments, the first of which are here noticed.

The density, expansion, and vapour-tension of propyl and isopropyl iodides have been measured with the greatest care; the chief difficulty to be overcome being the impurity of the liquids themselves, more than a kilogramme of each was specially prepared and dried by means of phosphoric anhydride; it was then submitted to fractional distillation, about 500 grammes of perfectly pure iodide being thus obtained; this was again distilled and collected in about four portions, each of which formed the subject of a series of experiments. The results obtained with all these portions agreed most satisfactorily, showing that when the substance is prepared with care, the error due to impurity is well nigh obviated.

The following table gives the results in such a form as to show that when the tensions of the saturated vapours of both iodides are equal, the densities and consequently the molecular volumes are unequal.

Vapour-tension in millims.	Boiling-point of normal propyl iodide.	Boiling-point of isopropyl iodide.	Density of normal propyl iodide.	Density of isopropyl iodide.	Difference between densities.
200	62°·37	50°·50	1·66704	1·64590	0·02114
300	73·51	61·33	1·64493	1·62359	0·02134
400	81·95	69·70	1·62808	1·60646	0·02162
500	88·84	76·44	1·61446	1·59246	0·02200
600	94·70	82·11	1·60250	1·58068	0·02182
700	99·83	87·13	1·59221	1·57035	0·02186
760	102·63	89·86	1·58670	1·56497	0·02196

It will be seen that this is in contradiction to Kopp's law, but that it is in accordance with the modern dynamical hypotheses on the constitution of matter, since the instability of the secondary iodide may be due to the greater *vis viva* of its molecules, which in turn would cause an increase of the mean distance between the molecules.

I may here mention that I have made a very complete series of experiments on the vapour-tension of normal propyl alcohol. The curve representing these observations intersects that which expresses the tensions of normal propyl iodide, so that whereas at 760 millims. the iodide boils at 102°·5 and the alcohol at 97°·3, at 370 millims. they boil at the same temperature, viz., 79°·5, and at 120 millims. the boiling-point of the iodide is only 49°·5, whilst that of the alcohol is 56°.

This fact, which probably arises from the much greater latent heat of propyl alcohol, obviously renders useless all attempts to derive the boiling-points from the constitution of chemical compounds, so long as the boiling-points at the ordinary pressure of the atmosphere alone are taken into account.

Mathematical Society, June 14.—Lord Rayleigh, F.R.S., president, in the chair.—Prof. Crofton, F.R.S., proved some geometrical theorems relating to mean values. These theorems were chiefly interesting as examples of the employment of the theory of probability to establish mathematical results; they were of a kindred nature with theorems given in the *Phil. Trans.*, 1868, p. 185, and in Williamson's "Integral Calculus," second edition, p. 329. Mr. Merrifield made a few remarks on the communication.—Prof. Clifford, F.R.S., read a paper on the canonical form and dissection of a Riemann's surface. The object of the paper is to assist students of the theory of complex functions by proving the chief propositions about Riemann's surfaces in a concise and elementary manner. To this end certain results of Puiseux's were assumed at the outset. Prof. Smith in making remarks on the paper expressed his indebtedness to the author in having cleared up a difficulty which presents itself in Lüroth's paper on the subject.—Prof. H. J. S. Smith, F.R.S., gave a short account of a further communication upon Eisenstein's theorem.—Mr. Tucker communicated a paper by Mr. J. C. Malet entitled, "Proof that every Algebraic Equation has a Root."—The Society's next meeting will be held on the second Thursday in November.

Royal Astronomical Society, June 8.—Dr. Huggins, F.R.S., in the chair.—Some tables for facilitating the computation of star constants were presented by Mr. Stone.—Mr. Marth explained diagrams referring to conjunctions of Saturn and Mars between July and November next—being a triple conjunction.—Dr. Royston Pigott described a method of collimating reversible instruments by which the error could easily be determined within 2″. Mr. Dunkin intimated that he would be greatly disappointed to find his collimation 0″·25 out.—Mr. Gill recounted some of the troubles that beset people who go after parallax, and described some methods of getting rid of systematic errors.—The president in the name of the meeting said "Good-bye" to Mr. Gill on the eve of his departure for the Island of Ascension.—At 9 P.M. the proceedings were stopped by the president to leave time for the special meeting called to consider a proposed alteration in the bye-laws.

PARIS

Academy of Sciences, June 18.—M. Peligot in the chair.—The following papers were read:—On the notation of Berzelius, by M. Berthelot.—Some observations on the mechanism of chemical reactions, by M. Berthelot. The new facts observed relate to direct oxidation of haloid salts, and of sulphurous and arsenious acids.—On the order of appearance of the first vessels in the aërial organs of some Primula, by M. Trécul.—On the crystalline form and the optical properties of proto-iodide of mercury, by M. Des Cloizeaux. The crystals generally occur in the form of thin, flexible, weakly dichroïc plates, of the quadratic system, but liable to be mistaken for a clinorhombic combination. Across the planes of cleavage they give strong double refraction, with positive axis. The salt is completely isomorphous with calomel (or the protochloride); and it is imperfectly so with red bi-iodide of mercury, which, however, has a *negative* axis. M. Berthelot, in view of such facts, remarked on the uncertainty they throw on the employment of isomorphism as a method for determining the number of atoms contained in a compound, and consequently the absolute value of the atomic weights.—Reply to the observations of M. Mouchez, by M. Villarceau.—On M. Villarceau's *Nouveau Navigation*, by M. Mouchez.—On the interior sea of the Algerian Sahara, by M. Favé. The slope at the borders of the lake, he points out, would be very pronounced.—Theory for finding the number of variants and contravariants of given order and degree linearly independent of any system of simultaneous forms containing any number of variables (continued), by Mr. Sylvester.—On the present state of the solar atmosphere; letter from P. Secchi. In presenting a *résumé* of the spots and protuberances of 1876, he gives his reasons for thinking the sun in a state of *relative* (not *absolute*) calm. M. Janssen's view that there is rather a tendency to speedy dissolution of spots than a state of (even relative) calm, implies, he thinks, the false idea that spots can be maintained for long without the continuance of eruption. Their short duration indicates a short time of eruption, therefore weak solar activity. Spots continue because the dissolved matter is replaced by freshly erupted matter. We have no proof, either, that dissolution is more rapid at the periods of minimum. There are now signs of re-awakening activity.—On electro-magnets with rundles of iron, by M. Du Moncel. He is led to study these again by experiments of M. Fridblatt and M. Jablochkoff. The lateral action of the magnetising currents on the rundles is limited, he thinks, to a simple dynamical reaction between parallel currents, which may, with very strong currents, make plates of iron adhere strongly to the cheeks of the spiral, but which does not develop *exteriorly* on these plates well-marked magnetic polarities. This latter only occurs where the plates are so small that the spirals act on them by enveloping them like a core.—On the use of sulpho-carbonates and sulphide of carbon in treatment of the vine, by M. Marés.—On a temporary affection of sight, by M. Pierre. Reading, one day, after having had brain fever, a glazed volume, he found the characters apparently more distant than the paper (about 4 mm.); and the exercise was very fatiguing. In the next eight or ten days the characters seemed gradually to come nearer to the surface.—Historical remarks on the theory of motion of one or several constant or variable forms in an incompressible fluid, &c. (continued), by M. Bjerknes.—Determination of groups formed of a finite number of linear substitutions, by M. Jordan.—On the metallic solar eruptions observed at Palermo from 1871 till April, 1877, by M. Tacchini. In 1871 the zone of eruptions was confined between + 70° and − 40°; in 1876 it extended only between zero and − 21°; and in the first four months of 1877 there has only been one very small eruption.—On a new general method of synthesis of hydrocarbons, acetones, &c., by MM. Friedel and Crafts.—Reducing action of phosphorus on sulphate of copper; phosphides of copper, by M. Sidot.—Chemical researches on crystallised carbonate of lead formed on objects found at Pompeii, by M. de Luca.—Observations on some xanthates; separation of cobalt and nickel, by M. Phipson.—Researches on tetrachloride of carbon and its employment as an anæsthetic, by M. Morel. He was led to this application of it by the similarity of its formula (C_2Cl_4) to that of chloroform (C_2HCl_3). It is found a perfect anæsthetic and more powerful than chloroform, but quite capable of being regulated. The periods of insensibility and collapse are identical with those of chloroform; that of excitation is more pronounced. A mode of preparing the substance is described.—Observation of a bolide at Clermont-Ferrand on June 14, 1877, by M. Grerey. The head was about five to six minutes apparent diameter; the light was bright and flashing, with slight reddish and bluish gleams. No sound was perceived.—On a solar halo, by M. Vinot.

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THURSDAY, JULY 5, 1877

THE CAXTON EXHIBITION

THE exhibition just opened at South Kensington to commemorate the 400th anniversary of the first authentic publication issued from an English press, is one that must appeal to all who can read, and possesses an interest for the man of science from various points of view. We need not repeat the many platitudes that have been uttered and are now likely to be reiterated on the vast importance of the invention of printing by means of movable types. It was a gift to the people of Europe of a pair of intellectual seven-leagued boots wherewith to tread the path of culture; progress during the last 400 years has been beyond all proportion more rapid than during any previous period, and while no doubt other causes have been at work, the strongest impulse has been received from the invention so interestingly illustrated at South Kensington. Mr. Gladstone, in his speech on Saturday, stated that he did not think the invention of movable types in itself anything very extraordinary, and wondered that it had not been blundered on long before the time of Gutenberg and Fust. But the same might be said of most inventions in their first rude forms; we who are accustomed to locomotive engines and ocean-going steamers, for example, are apt to wonder how the world was so long in hitting on these applications of steam. But the truth is that in art as in nature no stage is reached by a leap; it requires a collocation of many little circumstances before any new form is ripe for development. And probably, if we could minutely trace the precedents of the invention of printing, we might find that it was the most natural thing possible that it should have taken place just when it did and not before. Probably all the material conditions or "environment" may have reached the proper stage a century before the actual invention, but then there was no Gutenberg or Fust (or whoever the genius was, for this is no place to discuss the much-discussed question) with the requisite discernment to perceive this, and the practical skill to proceed in the direction indicated by the conditions. It is curious that all the extant remains of the work of the earliest known printers are really wonderful in beauty of execution, which makes one doubtful if we have any of the very earliest specimens, and whether the date of invention should not be pushed further back than the accepted one.

The exact date of the invention, however, has not been satisfactorily ascertained. That it was complete by the year 1450 there seems no doubt, and by the year 1500 printing-presses had been set up in 220 places in Europe, and many books, mainly editions of the classical writers, and religious books, were in circulation by their means. Mainz was the city in which the new art reached its first full development, spreading thence to Haarlem and Strassburg, from Haarlem to Rome, 1466, by Sweynheym and Pannartz, who are said to have been the first to make use of Roman types, to Paris in 1469, to England about 1474, and to Spain in 1475.

The exact date of the introduction of printing into England is not certain; it may have been 1471, it was not later than 1477, the date of the publication of Caxton's "The Dictes and Sayings of the Philosophers," the first book certainly printed and published in England, at the Almonry at Westminster Abbey, where Caxton set up his press; it is to commemorate this event that this year has been chosen for the Caxton celebration. There is a story that a press was set up at Oxford a few years before Caxton's at Westminster; but the evidence for this statement is quite untrustworthy. The first English book printed was by Caxton at Bruges, probably in the year 1474, "The Recuyell of the Histories of Troye." It was at Bruges, where Caxton lived in his capacity of mercer, a man of great importance, and in the retinue of the Duchess of Burgundy, that he learned the new art of printing from Colard Mansion; when he brought the invention to England he was probably about fifty years of age, having been born in the Weald of Kent somewhere about 1420: nearly all dates connected with Caxton are very uncertain.

According to Oldys, the first book in which Caxton had any hand is one which may very fairly be considered as connected with natural science. Its title was "Bartholomeu de Proprietatibus Rerum," said to have been printed while Caxton was at Bruges in the retinue of the Duchess of Burgundy. The work is a kind of natural history, by Bartholomew Glanvill, a Franciscan friar, who flourished about 1360, explaining more especially the nature and properties of the beasts, birds, fishes, stones, &c., mentioned in Scripture. The work had already been translated into English in 1398 by John de Trevisa, and the translation was printed in England, probably on the first paper made in this country, by Wynkyn de Worde, after Caxton's death. It is only right to state, however that according to Mr. Blades, the great authority on all connected with Caxton, no impression of the edition in which Caxton is said to have had a hand, has ever been found.

Caxton, who died in 1491, although he published from his press at Westminster a wonderfully large and varied collection of works, does not appear to have been attracted to any bearing on science, strictly so called. Probably Mr. Gladstone hit on the reason in his estimate of Caxton's character when he spoke of him as a thoroughly practical Englishman who went in only for what would pay. The "Image or Mirror of the World," one of the popular books Caxton translated from the French, treats, however, of a vast variety of subjects after the imperfect natural philosophy of the day. We have an account of the seven liberal arts; of nature, how she worketh; and how the earth holdeth him right in the middle of the world. We have also much geographical information, amongst which the wonders of the Inde occupy a considerable space. Meteorology and astronomy take up another large portion. The work concludes with an account of the celestial paradise. There are twenty-seven diagrams explanatory of some scientific principles laid down in this book; and eleven other cuts illustrative of other subjects treated in the work. The work was translated by Caxton in 1481, but the first edition has no printer's name, place, or date. The history of the "Mirror of the World" may be summed up thus:—Before the middle of the thirteenth century an unknown author wrote in Latin "Speculum vel Imago Mundi." In

1245 this was turned into French metre by the Duc de Berry; in 1464 this was turned into French prose, and from this text Caxton took his translation.

Even abroad the proportion of scientific to other classes of works issued from the early printing presses was comparatively small; but this may be satisfactorily enough accounted for by the fact that there were then comparatively few really scientific works in existence. From the Italian presses a very large number of arithmetical and geometrical works were issued at the end of the fifteenth and beginning of the sixteenth century. The Alphonsine tables were printed at Venice in 1483; but one of the earliest works in any way connected with science must have been a folio sheet, "Conjunctiones et opposiciones solis et lunæ," dated 1457; the place of publication we have been unable to ascertain. There is a "Gerardis Cremonensis Theoria Planetarum," quarto, dated 1472, and an Albertus Magnus "Opus de Animalibus," printed at Rome in 1478. Other early printed works which, considering the time, may be classed as scientific, are "Questiones Johannis Cunonici super octo libros Physicorum Aristotelis" (Padua, 1475); "Garetani de Thienis in Meteor. libros Aristotelis Expostio" (Padua, 1476); "Prognosticon," a meteorological work published at Venice in 1485. But when we come into the next century the number of strictly scientific works published in England and other European countries increased with amazing rapidity, and we may say has gone on increasing in ever enlarging proportion ever since. The first English translation of Euclid by Billingsby is said to have been published in 1570.

It is a small thing that books of science are all but unrepresented in the Caxton Exhibition; these could no doubt have been obtained had they been sought for; but the object of the exhibition is simply to illustrate the origin and growth of the art of printing, which has been an inestimable boon to science as it has been to every other form of human activity, and the man of science owes as much gratitude to its inventors, and to Caxton its introducer into England, as does the worker in any other department of culture. Happily, as we hope to show, science has been able to some extent to repay her debt by importing improvements into the art which would not have been possible but for her researches.

THE DEVELOPMENT OF THE OVUM

Bütschli on the Earliest Developmental Processes of the Ovum, and on the Conjugation of Infusoria.

Studien über die ersten Entwicklungsvorgänge der Eizelle, die Zelltheilung und die Conjugation der Infusorien. Von O. Bütschli. (Frankfurt, 1876.)

FEW subjects can be more important in their bearing on biology than the more prominent of those considered in this volume. It now rests on a morphological basis which will never be shaken, that there has been a procession of the most complex animal forms from simpler and still simpler ones, until we reach eventually the ultimate of organised simplicity. There may be difficulties in the way, but they are as nothing to the overwhelming evidence which morphology provides in its support; doubt, indeed, is no longer possible; and every year diminishes the circumscribed area of difficulty. But our knowledge hitherto of the developmental processes which take place in the earlier states of the simplest elementary organisms is wholly incompetent. Much labour has been expended, and doubtless good work has been done; but as it at present stands, it is conflicting, crude, and essentially wanting in coincidence and correlation. The work before us is the result of an attempt on the part of its author to penetrate farther into the matter than his predecessors, and by completer knowledge to harmonise or explain away conflicting evidence and doubtful interpretation, and if possible to give a sequence to the morphological processes in the simplest ova, and in the least apparently organised of animal forms.

From the smallness of the space at our disposal all consideration of the second subject discussed in this volume must be passed over. It deals with cell and nucleus fission generally; but as it is chiefly theoretical, we may the more readily omit it, merely remarking that the author concludes that there is a fundamental harmony in the method of fission in the cells of both animals and plants; a conclusion which it may be fair generally to admit; but in the minute detail, only discoverable by prolonged research, there will be found palpable differences.

That which gives distinction, and to some extent importance to the book, is (1) its minute and practical investigation into the earliest changes effected by development in the ova of some of the more lowly organised animal forms; and (2) the abundance of data which it appears to provide for the support of a new theory of propagation amongst the infusoria, which Bütschli propounds and advocates.

The embryological researches under the first head were conducted principally upon the ova of the Nematoid worms and the Rotifers. To a limited extent the living egg was studied; but the greater part of the results are derived from investigations of the ova treated with acetic acid. This is greatly to be regretted. The difficulties which present themselves in the minute examination of such ova in the living condition, are doubtless great, indeed complete results could scarcely be obtained from this alone. But undoubtedly the continuous examination of a set of living ova in process of development should be carried on simultaneously with every method of treatment which will reveal structure and change in ova of the same form in the dead condition. Only in this way can every possible mutation be traced, and its correlation and sequence be established.

It is extremely difficult to distinguish even striking discoveries in this direction from the manifold claims put forward by the many observers. We must state generally the facts as they at present appear, and seek to indicate the points specially claimed as new by Bütschli. It is now well known that the ovum is not suddenly formed, and then stimulated into new activity by fertilisation. It evidently, in its very lowliest condition, goes through a process of internal growth and development; after which apparently it perishes unless fecundated. In 1864 Balbiani endeavoured to prove that besides the *germinal vesicle*, there existed one still more important, which he called the embryogenic cell or vesicle in the ovarian ovum; and it was held by leading embryologists that it was round this cell that the true embryo was constituted; but in what manner, each observer appears to have determined for himself. The disappearance of what was accepted as the germinal vesicle was generally agreed to; but whether before or after impregnation was never fully determined. That it merely retrograded to the centre and determined segmentation as the result of fecundation, was held by many; while the embryogenic vesicle was said to persist, and from it were derived the now celebrated "globules polaires," or "Richtungsbläschen," which had been variously called by different writers from Carus downwards "white vesicles," "round vesicles," "clear globules," and so forth, and which are now thought to enter directly into the genital organs of the future being; Balbiani considering them of much importance in the evolution, inasmuch as they are found just in the region of the ventral layer of the blastoderm where the genital organs appear.

We have only space for a consideration of one of the instances adduced by Bütschli of earliest ovum development; but that may suffice to indicate the distinctive nature of his work. We select the eggs of *Nephelis vulgaris.* In their youngest state, the yolk is retracted from the delicate membrane, and there is, resting on the yolk, a minute mound of spermatozoa. At a little distance from this spermatozoal eminence there is an eccentrically placed spindle-shaped body, composed of fine longitudinal fibres, which at the equator of the spindle are swollen to a thick shining granular zone. The yolk mass is depressed at one point, and the spindle has its long axis directed to that of the flattened yolk. At the ends of this body there are clear homogeneous spots, from which rays go forth in all directions through the yolk. This spindle-shaped body Bütschli affirms to be the true *germinal vesicle;* and it is this which is carried upward to the surface of the yolk, by the elevation of the upper set of rays proceeding from the homogeneous spot over its upper apex, until eventually this spindle is pushed out of the yolk in three segments. In the part first protruded fine granules appear, and these retain their connection with the fibres in the part still inclosed in the yolk, by fine filaments, which also terminate in a zone of granules. This protruded vesicle is the "Richtungsbläschen;" the real place and relation of which, in the subsequent development of the egg, is nowhere determined by these researches. In the stage of partial protrusion of this vesicle, at about a quadrant from the point of its exit, another clear space arises sending out its radial rays; this enlarges, moves to the centre, and the germinal vesicle—now the "Richtungsbläschen"—is at this time quite protruded. At a point in the yolk determined by the point of exit of the "Richtungsbläschen," two minute nuclei appear, one in the upper margin of the clear space, and the other between that and the point of exit of the said vesicle. They are at first entirely disconnected, and both, by treatment with acetic acid, prove to be true nuclei. But they soon unite in the clear spot or space, and, at its expense, rapidly grow. They become a perfect nucleus with a distinct envelope and fluid contents, and distributed within the latter are dark granules. While these processes have been taking place two of the three segments of the "Richtungsbläschen" have again united, and at the same time the transformation of the nucleus begins. At two points on opposite sides of the nucleus, and in the direction of the long axis of the yolk, there arise clear spots and their accompanying rays. Between these, the nucleus differentiates itself into long fibres, and becomes a spindle-shaped body exactly like the germinal vesicle. An equatorial zone arises in it which is called a nuclear-band (kernplatte), which now divides; and each half recedes to the opposite ends of the spindle-like body. These ends now lose their points and become rounded, and in the mean time occurs the furrowing or constriction of the yolk. Another equatorial band arises in the nucleus or spindle, and when the constriction of the yolk is half accomplished the formation of nuclei of the second generation takes place from the ends of the spindle, these being nuclei in the completest sense. These fuse together and grow at the expense of the clear space—the growth of the nuclei and the diminution of these homogeneous spaces being in all cases correlative. When these nuclei are developed both hemispheres of the yolk collapse, and an almost spherical shape is again resumed.

What became of the fibres of the spindle was never discovered, but about this time the remaining segments of the "Richtungsbläschen" reunite, and in it a system of fibres appears. The following fission processes are but repetitions of this.

It becomes from the above apparent that Bütschli takes it for granted, first, that the eggs studied had been subject to no earlier developmental changes than those with which he starts. Next, that there can be no question as to the identity of his "spindle-formed body" and the germinal vesicle. He further at first claimed the extrusion of this germinal vesicle as the "Richtungsbläschen," as a sole result of the stimulus of impregnation; and ventures to consider that the process of nucleus formation described is widely diffused in the animal world, and that it is probably universal in impregnated eggs.

But (1) there is not the remotest evidence to show that processes of considerable import may not have preceded the condition with which these investigations started; complex processes are still known to occur in the unimpregnated ovum. We have only indeed to turn to the next example given by Bütschli himself to prove all this. In *Cucullanus elegans* the ovum leaves the ovarium without an envelope; and within the yolk is seen the "large *round* germinal vesicle and the germinal spot." The latter vanishes after impregnation, and the germinal vesicle becomes eccentric—and the next thing we are told is that "the germinal vesicle was no longer in the yolk, but *instead of it* there was a spindle-shaped something like that seen in Nephelis." How was the change effected? What were the steps? The transition is all-important, but how it happened is not worked out; and it would be, in so important a question, a matter of the greatest interest to know *how* the perfect spindle-formed body, with which these observations begin, arose. Nothing final can issue in this inquiry until, from first to last, every process and every step therein has been consecutively made out.

(2) The identity of this body with what is known as the germinal vesicle is certainly probable, but by no means certain, at present. It is certainly true that this supposition derives considerable support from the fact that Ratzel

found that in the ripe ova of *Tubifex*, prior to laying, the spherical germinal vesicle lost its spherical shape, elongated, became spindle-shaped with a meridional striation, and so forth, closely resembling the nuclear spindle of *Nephelis*. But as the process is described by Bütschli this would involve the necessity that the *whole* of the germinal vesicle was extruded as the "Richtungsbläschen" in every case. Against this, however, there are irresistible facts; and in an appendix to the volume the author is bound some sense to admit that there are cases where "a part of the germinal vesicle may remain." If this be so evidently there is missing a link in the chain of observation. Difficulties of an equally complex character present themselves in the collation of these researches with those of other distinguished embryologists which it would be hopeless even to attempt to consider here.

3. That the expulsion of the "Richtungsbläschen" is a result of impregnation must also be abandoned. In the text of this treatise the author earnestly contends for this point nevertheless; and endeavours to dispel the force of the very definite results of Œllacher, Bischoff, Flemming, and Beneden. But these are points that may be settled with comparative ease, and it certainly is true that the expulsion of the "Richtungsbläschen" may show itself as one of the earliest phenomena of development in the unfertilised egg. This is now admitted, and in the appendix is allowed by Bütschli.

4. The universal application of the method of development seen in *Nephelis*, although strongly contended for, and carried by analogy into the interpretation of the theory advanced in the third part of the volume to account for the propagation of Infusoria, can only be admitted with the utmost caution. The evidence given by the author is by no means perfect. In *Cucullanus elegans*, for example, he admits that the transition of the nucleus spindle into the "Richtungsbläschen" cannot be made out as in *Nephelis*, but contends that it *ought not to be doubted*. And precisely the same difficulty attaches to the transformations of the nucleus, of which "nothing could be certainly found;" yet the same doctrine is carried over, as though precisely the same phenomena had been witnessed as in *Nephelis*. So in relation to other Nematoids, it is rather inference than evidence that the protruded vesicle is the germinal vesicle, as in *Nephelis*. So in *Limnæus auricularis*, essential points in the origin and subsequent evolution of the spindle and nuclei are presented, not as the result of observation, but of inference, and a leap across a chasm between two preparations of the ovum which show no continuity of evolution, is taken with an assurance that "doubtless," although the intermediate process was not made out, we might be guided by the analogy of *Nephelis*.

These facts are pointed out, not in the slightest degree to detract from the value of the author's observations, but simply to separate them, as such, from the inferences he draws from them. There can be little doubt that great value belongs to the discovery of the nucleus spindle and its behaviour in evolution; and there can also be little question that it is largely original research; but its relation to anterior and subsequent processes is not so definitely discovered. It is nevertheless a source of great interest to find that Balbiani has given such complete and recent confirmation to the main characteristics of the spindle-nucleus.[1] It is true that he does not confirm the division of the equatorial band in the nucleus, and claims to have shown the existence of the clear spaces and rayings accompanying the nucleus-transformations in the eggs of spiders four years before. But evidently a step is gained by these observations on the earliest development of the ovum; although, from the careful work of M. Fol, it is clear that not only the interpretation, but the detail, may be open to question.[2]

W. H. DALLINGER
J. DRYSDALE

(*To be continued.*)

THE ALKALI TRADE

The History, Products, and Processes of the Alkali Trade, including the most Recent Improvements. By Charles Thomas Kingzett. (London: Longmans, Green, and Co., 1877.)

TOWARDS the middle of last century the price of oil of vitriol was 130*l.* per ton; the same substance now sells at 5*l.* per ton. In the first years of the present century soda crystals sold at about 60*l.* per ton; their present price is about 4*l.* 15*s.* per ton.

In 1861 the Lancashire district produced 8,800 tons of soda crystals, 4,600 tons of caustic soda, and 11,700 tons of bicarbonate of soda. The same district consumed, in that year, 161,000 tons of sulphuric acid and 135,000 tons of salt. Five years later (1866) 194,000 tons of salt were consumed in the same district, while the out-put amounted to 25,000 tons of soda crystals, 11,000 tons of caustic, and 6,500 tons of bicarbonate, together with 87,000 tons of soda ash and refined alkali, and large quantities of bleaching liquor, bleaching powder, &c. The following numbers, obtained from the Alkali Association, show the increase in the alkali trade of the United Kingdom between the years 1862 and 1876:—

	1862.	1876.
Annual value of finished products	£2,500,000	£6,500,000
Weight of dry products ...	280,000 tons.	845,000 tons.
Raw materials used:—		
Salt	254,600 ,,	538,600 ,,
Coals	961,000 ,,	1,890,000 ,,
Limestone and chalk ...	280,500 ,,	588,000 ,,
Lime	—	139,000 ,,
Pyrites	264,000 ,,	376,000 ,,
Nitrate of soda	8,300 ,,	12,200 ,,
Manganese	33,000 ,,	18,200 ,,
Total	1,801,400 tons.	3,562,000 tons.
Capital employed in the business	£2,000,000	£7,000,000
Hands employed	10,600	22,000
Wages paid them annually..	£549,500	£1,405,000
Weight of soda exported ...	104,762 tons.	270,856 tons.
Value of exported soda ...	£885,245	£2,209,284

These facts enable us to form some idea of the enormous growth of the alkali trade within recent years. This growth has been in a large measure coincident with the growth of scientific knowledge. The facts discovered in the laboratory have been turned to account in the alkali work, and the theories of the chemist have not unfrequently received confirmation at the hands of the manufacturer. Conversely, the wants of the manufacturer have hastened the discovery of fresh facts, and the success or failure in the application of these facts on the large scale has reacted beneficially upon the advance of chemical theory. In 1750 sulphuric acid was manufactured by distilling sulphate of iron in earthen vessels luted to glass receivers. The destruction of plant obliged the manufacturer to adopt a better method. The chemist supplied him with the facts: Nitre and sulphur when burnt together produced sulphuric acid. The manufacturer supplied the mechanical means for realising this process on the (comparatively) large scale. Soon after this time Scheele discovered chlorine; the manufacturer, acting on the experiments of the chemist, turned to account the fact that chlorine readily combines with hydrogen. But the impulse thus given to the bleaching trade necessitated a corresponding advance in the manufacture of sulphuric acid. The chambers in which the nitre and sulphur were burnt were enlarged, improvements were adopted, and the price of the acid decreased while the consumption increased.

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[1] Sur les Phénomènes de la Division du Noyau Cellulaire, *Comptes Rendus*, Oct. 30, 1876.

[2] Sur les Phénomènes Intimes de la Division Cellulaire, *Comptes Rendus*, Oct. 2, 1876.

In more modern times we see the need of a cheaper method for manufacturing chlorine, giving rise to the successful process of Weldon, a process based upon strictly experimental laboratory data, and to the hitherto not so successful process of Deacon. We see the failure of the latter process inducing its discoverer to extend his researches, and as a consequence chemical science is enriched with a valuable paper which throws considerable light upon the general principles of chemical dynamics.

While the history of the alkali trade illustrates the benefits conferred upon manufactures by science, and the requital made to science by manufactures, it also forcibly illustrates the uses which to-morrow may bring for the waste products of to-day.

The monopoly granted by the King of Sicily to one firm in the exportation of sulphur obliged the manufacturers of oil of vitriol to have recourse to some other source of sulphur. The introduction of pyrites led to the accumulation of burnt ore, and this again to Henderson's method for extracting copper, a method which, whether considered chemically or commercially, has proved most successful. The hydrochloric acid sent out from the chimney of the alkali works has, since 1863, been almost wholly condensed, and from this once wasted acid immense quantities of bleaching powder are now manufactured. The acid liquors from the manganese still, although rich in manganese, were formerly sent into the nearest stream, thus causing at once a loss to the manfacturer and a nuisance to the neighbourhood. Now, however, these liquors are turned to use, the nuisance is abated, and the manufacturer is enriched.

But if one is to acquire a just idea of the immense dimensions, and of the importance of the alkali trade from a commercial, chemical, or general point of view, one must learn in detail the history of the manufacture, the development of the processes which gather round the alkali trade as their centre, and the connections which subsist between the practical carrying out of the manufacture and the general principles of chemical science. Such a knowledge may be obtained from the work before us. Mr. Kingzett gives a clear and succinct account of the rise of the alkali trade and of the present state of the manufacture. Notices, sufficiently detailed for the purposes of the general reader, of all the recent improvements are introduced. The allied trades, especially the bleaching powder and soap manufactures, are described.

The book necessitates a general knowledge of chemistry on the part of the reader, inasmuch as processes are everywhere referred to their fundamental chemical principles. He who wishes for a rule of thumb acquaintance with the alkali manufacture will certainly find the information given in this work beyond his scope. On the other hand, the man who, having a general knowledge of chemistry, really wishes to learn how chemical facts are turned to account in manufactures, and also how mechanical difficulties are overcome, cannot do better than study—not read only—the work before us.

The chemical manufacturer also may gain from this work a more extended knowledge of his trade, and he may receive many hints, which, if he be of an inventive turn of mind, he may some day turn to account. The author has evidently endeavoured to treat the subject from the standpoint of the scientific manufacturer, and we think he has very fairly succeeded.

Full details of the more modern improvements of Hargreaves, Mactear, and others in the manufacture of alkali are given. The Weldon process for manufacturing bleach is described minutely, the improvement suggested by Mr. Weldon, whereby loss of calcium chloride would be avoided, is mentioned, and its utility is pointed out.

Of course there are parts of the book which it appears to us admit of improvement. The introduction of an index would add to the value of the work. Might we suggest to Mr. Kingzett that it would be well to re-write the preface, and generally those portions of the work in which he indulges in philosophising? The book begins with a platitude: "The wealth of a nation may be said to be indicated by the magnitude of its commerce." It closes (the last chapter is purely statistical) with a poor simile: "Life may be compared to a spectrum with its bright and dark lines."

M. M. PATTISON MUIR

OUR BOOK SHELF

River Terraces. By Col. George Greenwood. (London: Longmans and Co.)

FOR somewhere about fifteen years no name occurred more frequently in the geological correspondence of our magazines and newspapers than that of "George Greenwood, Colonel," and no letters carried with them a more marked individuality than those to which that name was appended. They never betrayed any doubt or hesitation, but made merry over the doubts and difficulties of other and more experienced observers; they showed in vigorous language that in so far as a correspondent agreed with their author, he was right, that in so far as he differed he was wrong. Fathers in science like Lyell and Darwin, as well as striplings, not yet emancipated from geological long-clothes—one and all needed instruction and correction at the hands of the enthusiastic Colonel. He spoke of the geologists of the country as schoolboys, whom he had to drill in the beggarly elements, and divided them into classes according to their acquirements or their aptitude to receive his lessons. He began by enforcing his views as those of Hutton and Playfair, and gradually so identified himself with them that he regarded them and the very words expressive of them as his own property, which no one should claim or touch except in the way he chose to sanction. Peace be with his memory! He did a good work in his time. Men gladly overlooked his personal failings for that sound sense so often underlying his self-asserting remarks about geological forces which had not been adequately understood in this country when he began his crusade of "Rain and Rivers." The present volume is a reprint of his letters on all manner of subjects, written at different times from 1859 to 1875. But surely its publication was not needed for the scientific reputation of the author. The letters are given as they originally appeared, full of references to passing incidents, and to letters by other writers, which of course are not inserted, but without which Col. Greenwood's diatribes are often unintelligible. There is no attempt at editing. The title of the book also is misleading. Instead of a treatise on river terraces, it is a medley of clippings from the columns of various periodicals relating to such varied subjects as Spelling, the Possessive Augment, Source of the Nile, Glen Roy, a Horse-Chestnut Tree, Rain and Rivers, Sirloin, Pronunciation of Latin, Lakes with Two Outfalls, a Beech pierced by a Thorn Plant, Origin of the Chesil Bank, &c., &c.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Tait on Force

In Prof. Tait's lecture on "force," which its writer seems to have intended as a model of perspicuity and accuracy, we are told that "we must measure a force by the rate at which it produces change of momentum." Nothing could be clearer or more satisfactory than this statement. Then Prof. Tait proceeds to tell us what force "is," and we read—"Force is the rate of change of momentum"—giving to this word "is" the meaning which the so-called metaphysicians give to it; and it seems to me that we might jangle over it for ever, without ever knowing whether this latter statement be true or not; for although we may all agree as to the proper *measure* of a force, it seems to be more difficult to tell what force "is." Possibly we might roughly measure the hunger of a man under different circumstances, by determining the number of pounds of beef he would consume, but it would be hardly warranted to say that hunger "is" a certain number of pounds of beef.

Perhaps it may be advantageous to apply the name force to the thing which we have heretofore called rate of change of momentum due to force, but I cannot imagine how any one can think that a certain "rate of change of momentum" can produce a unit of momentum in a unit of time. Until this shadowy "phantom" called force can be brought a little more sharply into focus, it seems to me that considerations as to what it "is" may profitably be left to those who appear to delight in the obscurity of obscure things—the metaphysicians.

St. Louis, June 4 FRANCIS E. NIPHER

P.S.—On showing this note to a friend, my attention was drawn to a note by Robert Napier in *Engineering*, which makes the present one seem almost superfluous. Remembering, however, the difficulty which I had in understanding these very points on account of the loose way in which they are put in many text-books, I feel that too much cannot be done to prevent such things from going into text-books in the future.—F. E. N.

[Prof. Nipher's censure does not apply to my lecture, simply because he fails to remark that I had two objects in view, (1) to point out the sense in which the word *force* must be used if we desire to avoid confusion; (2) to point out that, in all probability, there is no such *thing* as force. Under the first head I of course referred to Newton's "Laws," and in them language is used which at least suggests the objective reality of force as the *cause* of change of motion. We must take Newton as we find him. But there is no inconsistency in afterwards proceeding to give reasons which appear conclusive against the objective reality of force.

With some of Prof. Nipher's other remarks I can cordially agree. Since my lecture was published I have been in almost daily receipt of passages containing errors amounting often to the wildest absurdities, due to misuse of the term force. The latest to which my attention has thus been called is in the *Cornhill Magazine* for June. Here the non-scientific public is gravely told that "*what mathematicians call the moving force exerted by the earth on the moon is eighty-one times greater than the corresponding force exerted by the moon on the earth.*"—P. G. T.]

On Time

If I understand V. A. Julius's letter in NATURE, vol. xiv. p. 122, on the measurement of time, it may be thus summarised:—

As equal times, unlike equal linear magnitudes, cannot be brought alongside of each other, their equality can be ascertained only by means of velocities. (This will not be disputed.) We define those times as equal during which the same space is traversed by equal velocities; but the postulate that a velocity, *e.g.* that of the earth's rotation, continues unchanged, is arbitrary, incapable of proof, and justified only by practical convenience.

It seems to me, on the contrary, that the postulate is not necessarily arbitrary, but may be absolutely justified by facts. The best case to put is that of the pendulum, which, according to Sir William Thomson, is probably capable of measuring time with greater accuracy than the motion of the earth itself. If we assert that equal forces acting through equal spaces produce equal velocities (and this is rather a definition than an axiom), then the assumption of the equal velocity of all the pendulum's strokes postulates nothing except that the force of gravitation continues unchanged. I admit that I see no way of proving this, but it may be safely assumed in the absence of any known or probable cause of change. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, June 19

The Antiquity of Man

I HAVE no desire to enter into the controversy respecting the age of the palæolithic implements found in brick earth near Brandon, by Mr. Skertchly. I had the great pleasure of going over some part of the ground with him and Mr. Belt in November last. But what I saw then was not sufficient for me to make up my mind upon the question. Of course Mr. Skertchly, with his immense experience, has far more evidence in his repertorium than a cursory visit could afford to me.

My object in writing this letter is to point out that, if it should eventually be proved that *a* glaciation of the surface has occurred in East Anglia subsequent to its human occupation, but one which is not identical with, but posterior to, that glaciation (whether land or marine) which deposited the great chalky boulder-clay, then this is no more than I put forward many years ago in my papers on the "warp" (*Geol. Journal*, 1866); and on the "glacial origin of denudation" (*Geol. Mag.*, 1866); and on the "denudations of Norfolk" (*Geol. Mag.*, 1868).

I think this is the direction to which the course of opinion appears to be tending, and I ask you kindly to bring under the notice of the younger generation of geologists the speculations of an elder brother.

I call the product of this supposed glaciation "trail." The more orthodox, I believe, consider it to be "rain wash," and I had hoped that some competent writer would have thought me worth confuting. But none has done so. I have reason to think that one of your correspondents did actually put down as boulder clay this very deposit, at one of the most important sections which I saw near Brandon. O. FISHER

Harlton, Cambridge, June 28

Museum Reform

No one acquainted with the condition of the greater proportion of our provincial museums can do otherwise than confess with sorrow that much of what is alleged against them in the paper of Mr. Boyd Dawkins is too true. While fully concurring in all he says as to the actual state of matters in these amorphous receptacles of curiosities and conceits, and as to the crying need for reform, you will perhaps allow me to make a few comments as to the causes which contribute to keep museum collections in their present disreputable condition, and the means by which they may be worthily organised, and raised to their high and proper position among the educational agencies of the country.

It is necessary in the first place to accept Mr. Boyd Dawkins's glorification of the collecting instinct with some modification or rather amplification of its scope. A man is indeed "poor and much to be pitied" who is not a collector in some sense; but it does not require demonstration that many of the best and greatest benefactors of mankind are not collectors in a way that contributes to the building up of museums. Statesmen, soldiers, poets, philosophers, and orators are not of necessity poor and much to be pitied because they may not devote their leisure to the collection of coleoptera nor find solace in the beauties of Lucca della Robbia. In nine cases out of ten, indeed, the collector is a person of one idea, and that idea is that the gathering, labelling, and arranging of the objects of his fancy is the beginning, end, and sum of science. He is generally an estimable person; but as regards scientific culture he is quite as well employed in collecting spoiled postage stamps as he would be in gathering together the species of any of the great divisions of the animal kingdom. When we come to the tenth man—the intellectual collector—we find a really scientific worker, but one necessarily confining himself to a limited field. He is in short a man with a hobby, or, to put it more courteously, a specialist. Put a man of this select class in charge of a provincial museum, and while probably he is too wise to speak slightingly of any department of human knowledge, he will inevitably develop his own special subject at the expense of all others. A geologist draws towards him rocks and fossils, an entomologist collects in the particular group of insects to which he has given attention, and an archæologist looks only or mainly for antiquities. If the man is a simple collector of the ordinary type he knows nothing or despises everything beyond his region, and hence in part the jumble of ethnology, art, and science which Mr. Boyd Dawkins so graphically describes.

A specialist, though an indispensable cultivator of science, is a very bad museum curator. A curator should be like a newspaper editor, a man of general knowledge and culture. Unlike an editor, he should belong to no party, but be possessed of catholic sympathies in science and art; ready to accept and use the assistance of specialists, in a way that will subordinate all departments to one harmonious general plan. Further, he should possess an experimental knowledge of the routine duties of a museum, such as can only be obtained by a training or apprenticeship in a well-organised museum.

No provision, I need hardly say, exists at the present time for training young men to museum work, and there is no pecuniary inducement held out for lads to seek curatorial qualifications. The training obtained in the great metropolitan museums is special; and in the government service there is no hiving off of apprentices. Municipal and free library authorities have not yet learnt that a well-equipped museum is an expensive institution, and though many corporation dignitaries may spend annually 1,000*l.* and upwards on purchases for their private collections, it does not occur to them that it is necessary to do more than open the doors of a public museum or art gallery, and allow collections to accumulate, arrange, classify, catalogue, and conserve very much of their own accord. And so we obtain the dusty, misleading, higgeldy-piggeldies which do duty in provincial towns as "museums."

Before these institutions can rise from their present dismal estate it is essential that much more money be devoted to them. Of course it does not matter whence the funds come—from public rates or private benefaction—provided it comes honestly; but there is, as the law now stands, hardly in any town sufficient rating power to build and maintain a museum adequate to the population and wants of the locality. Free library boards with their penny rating limit have in many instances committed themselves to very ambitious mistakes by instituting numerous district libraries, and throwing in a public museum to the bargain, under the delusion either that their penny is like the wizard's inexhaustible bottle, or that these institutions will live and flourish "without visible means of support." The result is, that while libraries have been crippled and half starved, ratepayers have been justly disgusted with the very name of museum.

The provincial public mind, both official and extra-official, stands sorely in need of enlightenment as to the nature and functions of a museum. The education of opinion on these points is the first step required for the elevation of local museums. With that effected, enlarged rating power, a demand for competent men, and adequate support to institutions on a broad educational basis would soon follow. Local museums, ceasing to be mere curiosity-shops, receptacles of "relics from Sedan," "water from the Jordan," with six-legged cats and similar monstrosities, would become storehouses of well-selected information and material for the use of teachers and investigators, as well as instructive and elevating resorts of the general public.

No class of institutions existing could be made mutually more helpful than museums. Duplicates innumerable go to wreck and destruction in the stores and cellars of almost every museum, while certainly many kindred institutions stand in need of what is simply an encumbrance to some. Similarly with labels and stores of information, each institution at present stands apart, working painfully, and perhaps erringly, at tasks which might well be spared, seeing that it is and has been done over and over again in other institutions. Again, one locality possesses rare and unusual facilities for obtaining particular classes of objects, and that advantage can, by a system of exchange, be made properly beneficial to its own museum by drawing what it needs from others. Further, in these days of comparative infancy, the experience of the officers of the older museums would be of unspeakable value to those struggling amid difficulties of which they barely recognise the nature; and to all, the countenance of the great institutions—which should be prepared to stand more *in loco parentis* than they at present do—and the advice and help of their specialists would be of much advantage. In these days of conferences, associations, and unions, it is manifest that there is room for a conference of museum keepers, and no one can doubt that vast good would result from drawing the officers of museums of all kinds into closer relationship with each other. Will the energetic officers of South Kensington not display once more their organising talent by bringing together such a conference, which, it may be hoped, would result in a permanent union among museum officers.

J. P.

Taunton College School

May I ask the insertion of the following brief remarks:—

The writer of the article in your paper of the 28th on Taunton College School is under some strange misapprehension, which perhaps may account for his unfavourable criticism of the schemes of the Endowed Schools' Commission. He clearly implies, though he does not positively state, that the present disturbance at Taunton (of which I know nothing) and the scandal at Felsted two or three years ago are in some way attributable to the wrong constitution of the governing body, under schemes of the Endowed Schools' Commission.

Taunton College School is not under a scheme of the Endowed Schools' Commission, and no scheme was ever proposed for it by that body. A scheme for Felsted Grammar School was proposed by the Endowed Schools' Commission, with the hearty goodwill of the late master, but was rejected by the House of Lords on the motion of the Bishop of Rochester (now of St. Alban's). The trustees who dismissed Dr. Grignon were the very body whose constitution our scheme proposed largely to modify, and who were in consequence not a little annoyed.

Your writer will, I hope, excuse my saying that he will serve the cause of science and of schools much better if he does not weaken his attacks on the guilty by hitting, or making feints of hitting, at the innocent.

Henry J. Roby,
Late one of the Endowed Schools'
Commissioners

Manchester, June 29

Hog Wallows

I have been watching with some interest the progress of the discussion on the "Hog Wallows" of California, which has been in progress in your paper during nearly all of this year. When a member of the California Geological Survey Corps, I had numerous opportunities of studying the phenomena in that and the adjoining states of Oregon and Nevada; more especially in the southern parts of the desert. There they are developed on the largest scale, and there their origin is obvious.

Prof. Le Conte's account of them wants but a single word to have settled the question. I attributed them, then, exclusively to the action of the wind, and after reading what others have to say about them, see no reason for changing that opinion. The Professor says, "I attribute them to surface erosion." Had he inserted *aërial*, nothing more would have been wanted; although, since he speaks of weeds and shrubs taking possession of them, subsequent to their formation, he does not seem to have exactly hit on the *rationale* of the process.

One case may serve as an illustration. In the southern end of the Reese River Valley, Nevada, is a broad, perfectly level plain without a water-course; only a few shallow dry gutters show where the rain water runs to scattered spots, where it sinks or evaporates. The region is almost rainless. The plain is covered for many square miles with these mounds, varying up to four or five feet high, and up to twenty, thirty, and even perhaps forty feet in diameter. In every case they are made up of only the finer particles of the soil, the coarser grains and gravel being visible in the interspaces. The dust and sand has in all cases been heaped up in and around a clump of sage bush which continues to grow out of the top of the mound. Little vegetation grows on the flanks of the mounds, and when it does, it forms the nucleus of a subsidiary hillock. The mounds are thus formed by building up, and only the intervening spaces are caused by an erosion, taking place to day, and not caused by water, much less by ice.
WM. M. GABB

Puerto Plata, Sto. Domingo, June 5

Fertilisation of Salix repens

DURING May I was watching the movements of the insects on a plant of *Salix repens*, when I noticed some facts which may prove interesting to some of your readers. It was mainly visited by the common hive-bee (*Apis mellifica* ☿) and the humble-bee (*Bombus terrestris* ♀). The former of these flew gaily about from catkin to catkin merely taking one bite at each; but the latter went far more systematically to work; it never flew at all, but crawled in a ludicrously feeble way from catkin to catkin, and once on a catkin it cleared it thoroughly, thrusting its proboscis between every pair of florets. I do not know whether this greater thoroughness is at all times characteristic of the humble-bee as compared with the hive-bee and should much like to be informed. And another thing which I do not understand is, that one of these humble-bees appeared to have two kinds of pollen on its legs, one that of *Salix repens*, the other of a much darker and more-orange colour, though when examined under the microscope the grains proved to be of the same shape.
H. H.

Wellington College, Wokingham, June 30

THE FUTURE OF SANITARY SCIENCE—POLITICAL, MEDICAL, SOCIAL[1]

I COULD have wished it had been in my power on the present occasion to produce one of those essays which appeal to the imagination while they prepare the mind for the reception of sanitary principles and practice. Such essays are tempting and, in their place, instructive. To-day I am bound on a voyage less pleasant, yet I hope not less useful.

There has recently been called into existence a new society under whose summons we now meet. The society has assumed to itself the expressive name of the Sanitary Institute of Great Britain. It starts as a voluntary effort by men and women who are willing and anxious to give effect to those teachings of sanitary science which the past half-century has revealed. It invites all who are concerned to utilise the knowledge that has been acquired in that time. It wishes to encourage new research. But it has for its most anxious care to render useful to mankind at large the accumulated store of knowledge which at this moment lies ready for so many grand purposes relating to health. It accepts as its object, work for health, health of all the human family.

Shall some one say the object is ambitious? Yea, we reply, it is confessedly ambitious. Shall some one say the means at command for the work to be attempted are weak? Even so. Life is short, art long. Yet the short yields the long, and but for the short the long could not be. It is out of these littlenesses of human effort that the greatnesses follow. Or, as Benjamin Rush very forcibly puts it, and simply as forcibly: "There are mites in science as well as in charity, and the ultimate results of each are often alike important and beneficial."

It is my fortune, good or bad, to have to preside over the council of this new society. Of the ability of those who form the council, and of their experience, I need not speak in detail, for their names are familiar to the world. They represent, I may say, sanitary science in all its branches, and from them, working harmoniously together, good results must be expected.

It seems fitting therefore as we enter on our work to look forward to the future. It is a part at least of our duty to look towards the future with the view of seeing in what directions we may best proceed; what assistances we may have to call upon; and chiefly what great powers we may have to consult and propitiate.

The three great powers with which our society will have to treat are the political, the medical, the social. From each of these we shall expect constant assistance. To one or other of these, whatever we do, our work will be transmitted or transferred. They will bring it into practical form and effect, or they will reduce it to nothingness. We can suggest and set forth initiatives, and with that our functions are complete in each particular branch to which we address ourselves.

It is our special duty to keep this special fact steadily in view and to limit our labours by it. It too often happens that young societies like young men are apt to believe that they can conduct national processes as easily as they can conceive them, and under this belief fail most signally with the best of attempts. I remember in my early career getting a lesson from one of our late well-known statesmen on this very point. I was explaining to him the efforts I had made in 1855 and the succeeding three years to establish a registration of the diseases of this kingdom, and I bewailed the hard experience which proved that the greater the scientific success of the effort the more impossible it became to carry it out. In fact, said I, in a pitiful strain, the success almost ruined me in mind, body, and estate. "Served you right," was the immediate reply, "Served you right. If individual men could carry out national projects where would be the nation?" The reply was hard as it was unanswerable, and from that time to this I have given up all thoughts of doing more than sowing seed in the field of literature and leaving it to the chance of fructification on that extensive soil; or in showing some mere model of experiment which, perchance, may grow into working form. And this, I think, is the whole natural scope of our Institute,—to sow the seed of sanitation; to think out plans of projects for working methods; to lend its many minds, as if they made up the mind of one man, for devising from the past the best for the present, and respectfully to declare our conclusions.

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[1] An address delivered before the Sanitary Institute of Great Britain at the Royal Institution, on July 5, 1877, by Benjamin W. Richardson, M.D., LL.D., F.R.S.

The directions in which we shall have to move, the lines on which we shall have to move, are, I repeat, chiefly three—the political, the medical, the social. The powers on these lines must be approached in every work of ours, however simple, however complicate it may be. I shall try, as the title of my discourse explains, to indicate certain points in which we are most likely to come in contact with these powers and the changes we may expect to work in and through them.

The Political Part.

In this country political action has been varied in relation to sanitary improvements. Sometimes political necessity has crossed sanitary progress, as, for example, in the imposition of a tax on sunlight, on foods that are essential to life, and in the granting of licences for the sale of pernicious drinks. At other times, and by fits and starts, political action has been in aid of sanitary work. So far back as the reign of Edward the Third, 1361, a royal proclamation was made through Parliament for preventing the slaughter of cattle in the streets of London because of the pollution of the streets and the drains which arose from that cause. From that time under great emergencies other similar acts came into force. They rarely lasted very long. As the urgent necessity for their existence passed away, they were allowed to fall into abeyance, and no permanent machinery was kept in order for insuring their continued and effective action.

Let me not, however, in saying this, be understood as conveying any special charge of neglect against English legislation. It is just to state, as an historical fact most creditable to our national history, that our legislators have by a long precedence taken the lead in sanitary affairs over those of other nations. In 1802 the great sanitary act for regulating the labour of children in factories set the example from which much useful legislation has followed at home and abroad. In 1838 that great original sanitary scheme for the registration of the births and deaths of the kingdom was inaugurated, to become a collection of facts relating to life, and disease, and death, of which there is elsewhere no parallel. And, since the era of the Crimean campaign, so much legislation has been attempted bearing on health, I dare not attempt even to enumerate the titles of the different measures that have been introduced. At this moment there can be no doubt as to the sincerity of our governments, of whatever party they may be composed, for dealing with every subject relating to the public health in an efficient manner, and in as rapid a progression as the slow and sure mode of parliamentary procedure will permit. The subject indeed presses at this moment with so much force on the governing mind, that if there be any danger ahead it is the danger of too miraculous a draught of small enactments, to the exclusion of comprehensive measures which all who run may read.

In saying this it is necessary to guard myself against error of expression. By comparison with all the nations of the world beside, we have obtained legislative measures which are splendidly comprehensive. No other country in the world can present an approach to the Public Health Act of 1875. That Act, as far as it goes, is admirably constructed. Its constitution of sanitary authorities throughout the kingdom; the power it vests in those authorities to appoint learned medical officers of health; the provisions it makes for securing to each locality better sewerage, freedom from nuisances, improved water supply, regulation of cellar dwellings, governance over offensive trades, and removal of unsound foods; the provisions for prevention of spread of infection and for the erection of hospitals and mortuaries; and the provisions for the regulation of streets and highways, lighting of streets, establishment of pleasure grounds, and regulation of slaughter houses; these, as well as the general provisions for the carrying out of the Act, are most commendable as practical plans by the working of which the nation may be tempered into sanitary mould of thought and character.

In a word the Act of 1875 is an improvement of the first degree on all that has preceded it, and although much of it, by the necessities of the constitution of our country,—which recognises the domination of free will even in its age of ignorance,—of a permissive nature, the working of the Act must in a few years remove a great amount of disease from the land and prevent the invasion of diseases of an epidemic and spreading type.

Sanitation however admits of being studied from two distinct points of view, the legislative and the scientific. The legislator may say, and perhaps with justice, that the production of such a measure as the Act of which I now speak is as much as can be done. The man of science may say that this is childish talk, that much more requires to be done, and that after all that which has been done, though it be comparatively great, is practically imperfect and very little. Science in this respect is always in advance of legislation, and that is her true place,—the pioneer's place. I remember the time perfectly when every fragment of the Public Health Act of 1875 was in the hands of men of science solely, and was called a chimera, over which great lawgivers shook their wise heads and passed by.

At this moment the positions of science and legislation are relatively the same as they have ever been, and it is fair for us men of science now as in the past time to declare the way ahead for the law-maker. I shall proceed again, therefore, as I have often before, to indicate one or two new starts in sanitary legislation, not from the legislative but from the purely scientific point of view, uninfluenced by the many and vehement individual grievances and troubles which beset the path of the minister of state. In so doing I shall indicate also, by inference, what I think our society ought to support in the sanitary policy of the future.

In the first place, then, we ought to expect in the political progress of sanitation that there will be established in connection with the Government one central department in which every subject, directly and even indirectly, connected with the health of the people, will be considered. This department, it is to be hoped, will be under the control of a Cabinet Minister, and will supervise the sanitary work performed at present by the Local Government Board, the Registrar-General's department, the sanitary regulations of jails and reformatories, and all the duties now pertaining to the supervision of factories, in so far as the health of the employed is concerned: in fine, every sanitary work that can be weeded out of every other department of the state.

To such a central board or department a specific name is necessary. The name should be as distinct as that of the department for war, for the navy, for the exchequer, or for the post-office. The name, it is to be hoped, will be emphatically the Health Department, and the chief of it the Minister of Health.

It may be urged that substantially we are drifting into some such order as is here suggested. It may be urged that the Local Government Board is step by step assuming the duties assigned, as above, for the State Department of Health. To some extent this is true, and it might be advisable, for the sake of the connection which must always exist between such a central board and the various local boards, in the kingdom to add to the name of Health Department that of Local Government Board. But for the sanitary object the leading name must be Health, and Local Government must come in merely as indicative of the connections that exist between the State and the local centres—as the machinery.

In this question of progress there is involved an immense deal in a name. It is essential to the scientific sanitary teacher that every reasoning mind in the kingdom should become familiar with the two significant words, public health, or national health. It is equally necessary to let the people know fully that the Government has the health of the country under its general and wise supervision. But it is utterly impossible to make either of these facts understood by the masses so long as any sanitary authority, central or local, has a title which fails to convey the meaning of its functions. To speak to the masses who are listening to a lecture or discourse on health about a local government board is only to confuse them. They ask you afterwards what it all means, and they go away imbued with the impression that it means anything except what relates to the health of the people.

I am speaking very practically in suggesting that in the course of political sanitary progress it is an absolute necessity for success to give its proper and only name to the department of state which presides over the national health. I do not state too much in declaring that every public measure would carry more weight if it went forth as being under the supervision of the health department. It may appear a refinement of illustration, and yet it is a sound argument that vaccination would have met and would meet with far less opposition if it were enforced under the general supervision of a State department of health. As it is the people connect the carrying out of vaccination with something other than health, and even as distinct from the idea of conservation of health. It is looked upon as a legal tyranny, having no scientific setting forth of its intention, and as springing from no scientific authority. If you attempt to reason with its active opponents on the subject, and refer to the authority that exists, they dispute the competency of the authority in name and form; and, foolish as the objection may be, it is potent for obstruction.

In making this suggestion there is no necessity to offer a word against the continued action of local self-government. The work of the local centres in all parts of the kingdom instead of being in any degree curtailed and restrained, should be encouraged and maintained. In the sanitary local work the word health should, however, again come forward as the one prominent designating term to which all others should be subject.

Our Sanitary Institute could not turn its attention to any more suitable labour than that of inculcating the necessity for the institution of one state department exclusively devoted to the health of the people. In the success attending such an effort a double result would be achieved. The country would have secured for it the best and most direct guidance on its most vital interest, and scope would be given to the industry of men of science in a new direction. Men, whose lives have been devoted to the study of life and health, would be prepared by their devotion for the accepted service of their country in public form, and the Houses of Parliament would become, at last, congenial spheres for their labours. The Houses would be strengthened by such adhesions; the men would be more useful and honoured.

Another work in the political line which will be demanded in the future for the benefit of the sanitary cause is the preparation of such a digest of all our practical sanitary laws that every person of intelligence can read and understand what may be legally enforced for the maintenance of health. What may be done in this direction ought to be so simple and so plain as to be brought into a school-book. Not a line should be left for the subtlety of the legal brain to twist into contortioned illegibility. The laws by which the health of a man, and thereby of a nation, can be preserved to the utmost, are so simple in nature that nothing but the utmost simplicity can truly express them, and the whole labour of the future, if it is to be of any service whatever, must be directed to the discovery and establishment of such simplicity of exposition and direction. Up to the present time much that has been done has been provoked by that most untrustworthy of all human provocatives to action,—fear. Some great epidemic has occurred that has caused universal dismay; some great catastrophe has occurred, like that of the Crimean campaign, which has excited universal criticism on the failure of sanitary provisions by the authorities of the nation. Some such slip has been permitted in sanitary rule as that which recently let scurvy undermine the workers during a great enterprise of discovery. Straightway on the heels of such events there have been commissions of inquiry, and as a direct or indirect result there has often come forth some particular enactment. Or—and this is by no means rare—some individual of the House of Commons, impressed with the danger of a great national evil, has pressed for a national remedy, and, by steady persistence session after session, and by showing that he never knows when he is beaten, has forced the Government to take up his measure and to carry it through.

From these modes of legislating for health we have obtained many minor acts which fill and refill the national statute books. And still this process promises to go on, a process of labour in a circle with much loss of time and expenditure of force without ultimate progression.

It would be vain to find fault with the past for its doings. As vain to find fault with the State for meeting State disorders by empirical remedies as it would be to find fault with the physicians of a former day for the same mode of procedure. If the people demand a recipe they must have it, be it from the State or the family physician. The question that now comes forward is whether the time has not arrived for ceasing to treat the health of the nation by specific or supposed specific remedies for particular errors, and whether we may not find in the future a few very simple and natural guiding principles on which all acts of Parliament relating to the health of the people may be based?

Before this effort can be attempted the existing acts that touch on health,—public health acts, metropolitan health acts, contagious diseases acts, vaccination acts, factory acts, acts relating to the importation of cattle, adulteration acts, and others relating to prisons, workhouses, and the like, and which, if they even lie latent are not repealed,—these, one and all require to be considered together, with the view of determining whether an English or even a British act of settlement for the vital regeneration of the realm is not practicable on a simple natural basis of natural requirement.

I am fully aware that this suggestion carries with it the idea of a gigantic labour; but it will have to be done, and once fairly tackled I dare say the apparent difficulties will readily dissolve away. It is a mere question between doubting and attempting: and we all know and feel that—

> "Our doubts are traitors,
> And make us lose the good we oft might win
> By fearing to attempt."

Supposing the existence of an efficient central department of health acting under the direction of a minister of health, a grand new duty, as it seems to me, would be to determine what is the evil or what are the evils that have to be removed in order that the cleanest bills of health may be regularly presented to the nation. Without such preliminary knowledge all sanitary work is unsound to the last degree. It were as wise for me to write a prescription for a man without inquiring into his disease, his antecedents, and modes of life, as for the State physician to prescribe for national sickness without inquiry into the nature of the sickness, its antecedents, and the cause or causes that led up to it. The great work, therefore, and indeed the first sanitary work of the future, standing before all other sanitary legislations except the formation of the central authority, is the systematic enumeration, week by week, of the diseases of the kingdom, through the length and breadth of the kingdom. It is utterly hopeless to attempt any decisive measure for lessening the mortality, which is certainly more than double what it ought to be, until this State labour is faithfully carried out. It is vain, comparatively speaking, to know what totality disease hands over to death, unless we know also what health under one or other cause of disturbance yields over to disease. Physicians and statists strain their eyes to try to get at the extent of disease. Laborious geographers like Mr. Haviland spend years in constructing maps from the tables of mortality, in order to get a mere approximation of the distribution of disease in England: and meanwhile disease itself, constantly cheating the observers, is making its way without being under any systematised recorded observation.

For the omission of a registration of disease there is no conceivable excuse. The thing has only to be done. The organisation of the Registrar-General's department has fully opened the way to the collection and the utilisation of the facts relating to birth and death. These elements swing in the statist's balance readily, and are weighed by our consummate state weigher of life and death, Dr. Farr, as accurately as the Chancellor of the Exchequer balances the national ledger. With equal readiness Dr. Farr, if the data were collected for him, could tell from week to week the health as well as the mortality of the kingdom. In a short time, under such regular record, the whole nation would know the reigning health, the reigning disease, of every centre of life. And if, as might easily be done, the diseases of the lower animals and the diseases of the vegetable kingdom were included in the returns, all the facts of disease would be completely rendered.

I think I have already referred to an effort I made many years ago to carry out this design of registering the diseases of the kingdom. I refer to that effort again for a simple reason,—for the purpose of indicating that there is really no greater difficulty in getting the facts than there is in utilising them. I attempted no more than the registration of the epidemic diseases, and I could afford no more than the publication of a quarterly abstract of the data that were forwarded. But in a short time fifty medical observers were sending in returns from as many stations, extending from St. Mary's, Scilly, to Lerwick, in the Shetland Islands. These stations could easily have been increased to any extent, and the amount of information regularly communicated was indeed most valuable.

Two facts connected with this attempt are perhaps worthy of note, one as showing something determined, and the other as showing something suggested. In the returns sent from the district of Canterbury in the spring quarter of the year 1857 was included the first account of the invasion of this country, at least in any known time, by the disease since then so prevailing and fatal, diphtheria. This disease first appeared in the little village of Ash, and was called the Ash fever. The outbreak was observed and recognised by Mr. Reid, of Canterbury, and was reported to my register by Mr. Haffenden, who collected for me the facts of prevailing diseases from eight medical observers living near to him, of whom Mr. Reid was one. The first facts of a new disease in this country were thus recorded on the spot, which is something even as matter of history. How such a fact, reported at once to a central government authority, might be dealt with; how promptly a central authority so advised might act in arresting a fatal epidemic at its origin, and what national service might be rendered thereby, you, quite as well as I, can judge!

The fact of a suggestive nature springing from the working of the returns is not less interesting. The labour led me to refer to the returns of sickness sent every week by the medical officers of the Poor-Law districts to their boards of guardians. I found that these returns, over 3,000 in number, which, when they have served their local purpose, are practically worthless, could by the slightest modification be utilised as returns of the sickness of all the sick parochial population under official medical care, and I submitted a plan for such introduction to public approval and to the Government, but without effect. Yet if the plan had been adopted from those three thousand weekly returns, cast away and still cast away, I calculate that 156,000 tables of disease per year would have been submitted to scientific analysis which, since the time when the suggestion was first made, would have multiplied into 3,276,000 tables, including in each table a record of at least ten times as many particular examples of disease. To what important national uses such an array of facts systematically arranged and examined could have been applied you, as well as I, can judge! And still neither of us can judge effectively, because in dealing with data taken from nature there is always something important to be elicited which never was looked for, and often, too, that something unlooked for is better than that which was specially looked for.

Our Sanitary Institute will do well in continuing to press this scheme for the registration of disease on the Government, and it may greatly assist the work by lending its mind to the best means of collecting the facts on which the weekly reports of disease will have to be based. I might enlarge on this part of my subject, but I should prefer to remain silent until the views of the medical officers of health, now a large and influential class, have been correctly ascertained. My present purpose is served if I have sufficiently directed public attention to the principles of the design.

In the future of sanitary science the politician must come forward more determinately than he has yet done, in order to secure for those over whom he governs three pure requisites—pure water, pure food, pure air.

The Public Health Act of 1875 deals with the water question, and makes provisions for the local authorities to supply their respective districts, by means of a company, or by independent action. For my part I see no hope of any effective change for the better by these propositions. It is utterly hopeless to trust to companies in a matter of such vital moment. It is equally hopeless to trust to the undirected action of local authorities. If we trusted to such agencies for the collection and delivery of letters by post, does any one suppose that the results of our present postage system would be attained? Yet important as intercommunication by letter is, it is less important than the supply in due quantity and pure quality of that vital fluid which makes up three parts out of four of every human organism, and which is wanted as much by the millions who never receive a letter, as by the millions who do. In this political part of sanitation, the Government must do one of two things. It must either produce a process or processes for pure water supply, and insist on every local authority carrying out the proper method; or it must,—and this would be far better,—take the whole matter into its own hands, so that under its supreme direction every living centre should, without fail, receive the first necessity of healthy life in the condition fitted for the necessities of all who live.

By recent legislation we have some security for obtaining fresh animal food, and foods freed of foreign substances or adulterations. The penalties that may be inflicted on those who sell decomposing, diseased, or adulterated foods are beginning to have effect, and much good is resulting. Nevertheless, even here the legal rule falls short of completeness. The inspection of animal food is as yet most unsystematic and imperfect. With all our richness of means ready at command, we have not approached that admirable system for the inspection of animal foods which our Jewish brethren, through ages of ignorance and oppression, have managed so efficiently to carry out, and which has entirely saved them from many of the great calamities of disease that have fallen on less careful people. The complete inspection of animal foods, including milk, is a clear piece of sanitary law which, from day to day and hour to hour, must ultimately be enforced.

Imperfect as legislation may be in respect to supply of pure water and food, it is advanced in these directions when the steps it has taken for supplying pure air are brought under observation. There is no practical legislation of any kind on this requisite. The air of our large towns is charged with smoke and impurity. The air of our great factories is charged with dusts which destroy life with the precision of a deadly aim. Dr. Purdon, one of the certifying surgeons under the Factory Acts, reports that in the flax-working factories under his care the carders, who are all females, if they get a carding-machine at eighteen years, generally die at thirty years. Can any fact be more terrible than such a fact, that a girl of eighteen should have to live by an occupation that will bring her existence to an end in fourteen years, and to that end with all the prolonged wasting, sleeplessness, suffering, incident to the disease consumption of the lungs. If it were the fate of these doomed workers that at the close of fourteen years' work the majority of them were taken forth and shot dead in an instant, their fate were infinitely better than it is. The heart of the nation would thus be roused, and the law in all its majesty would be put in operation to arrest the progress of the crime and to punish the offenders. Yet, year after year as effective an offence goes on, and because the results of it is hidden in the sick-room there is no arrest of its progress, no punishment for its commission.

In the application of political science to preservation of health not one subject presses more earnestly than the question of the supply of a pure atmosphere to the millions of industrials of these islands. In an inquiry I recently undertook on this matter for the Society of Arts, Manufactures, and Commerce, the facts that came before me were as of a new world. You will find a compact mass of these facts in the lectures I had the honour to deliver before that learned society. Those lectures contain a tithe only of the things seen. I am quite sure that our leading politicians can have no adequate conception of the mental and physical condition of the great industrial classes, or of the need that exists for reconciling those classes to their fate. These truths are plain.

The catechism has failed to satisfy them. Bad air keeps up in them a depraved mental as well as physical state. Their poverty and not their will consents to their condition. In short, as a physician dealing with the physiological and psychological phenomena belonging to a class instead of an individual,—and this is all the difference there is between a politician and a physician,—my diagnosis is that a serious organic state febrile, fitful, fatal, exists in this part of the nation; that it demands the watchful consideration of all physicians, State and ordinary; and that the sooner the natural cure for it, pure air, and plenty of it, is let in the better for every class everywhere.

All political troubles have a physiological cause. To the Statesman not less than to the physician, physiology is the only true source of knowledge. A society such as ours, therefore, possessing as it does professed physiological skill, may render most important service by tracing out for the legislator the simplest scientific means for removing with atmospheric impurities and by preparing for that sanitary future when men universally shall breathe purity even with their freedom.

If any other incentive to action in this direction were required it would be the further fact that all diseases, mental and physical, national and individual, begotten of an impure atmosphere, are transmitted on. The consumption of body, the restlessness of mind are reproduced and gain intensity of development with each generation until practically they inaugurate a distinct racial type of human imperfectedness.

With this topic of legislating for pure air would come in naturally the question of homes for the people and the development of those recent acts which have been passed to meet the necessity. These efforts of the world political can scarcely be over-estimated; but there is one movement which stands before them and which has been singularly overlooked. It is essential that the home of the working man should in every case be cleared of the details of daily work. So long as he is compelled to work in the room in which he sleeps and takes his food, so long his home must be an unhealthy centre, and too often it will be the centre from which infected work will pass out, bearing infection into the homes of the wealthy. A modification of factory legislation by which a free and properly regulated work-room should be within the easy reach of every working man in every crowded centre is a necessity which all sanitary labourers should strive to get supplied. Our Institute has another urgent task before it in the effort to enforce this necessity on public attention.

In the future of sanitary science one more amongst many other reforms of a political character must needs claim important consideration. I refer to the political assistance that must be given to all of us who are engaged in the labour of quenching the drunkenness of our land. Our best sanitary efforts will fall far short of their deserts until this object shall be achieved. Over the future of sanitary science will be suspended a pall of sorrow until this object shall be achieved. Does any one desire to know how the mortality of the kingdom is modified by strong drink, let him read the knowledge in the State record book which tells that those who sell the destroyer die by it at the rate of one hundred and thirty-eight to the hundred of the whole population. Then, starting from this signal fact, let him trace the influence of the destroyer through all the courses of diseases which, under learnedly obscure names, spring from it and kill from it in all classes of society. Finally, let him reckon up the hereditary evils which are engendered by the same destroyer and the influence of that on the course of disease, and his lesson will be in some measure complete.

I do not think this the occasion to discuss the value of the different political sanitary measures that have been, or are at this time, in the public mind for the repression of the national evil now touched upon. Be it sufficient for me to state two impressions only. Firstly, that every day's experience of the question in various communities where as a teacher of abstaining temperance I am wont to labour, indicates to me that unless the State does come to the aid of the teacher the battle against intemperance must be indefinitely prolonged. Secondly, that if the State, itself doing nothing active in the way of repression, would but determine to cease to legalise the cause of the evil and to make revenue out of the transaction, the labours of the temperance reformer would have the most prosperous season of success presented to their view. Hitherto this has not been considered as a sanitary question. In the future no sanitary student will venture to exclude it from his studies.

The contemplation of the political sanitary future of this kingdom offers many other topics, all of which I must leave, in order to devote a few minutes to our subject in its relation to medical science.

The Medical Part.

The influence which sanitation will exert in the future over the science and art of medicine promises to be momentous. It promises nothing less than the development of a new era; nor is it at all wide of the mark to say that such new era has fairly commenced. The greatest of the world's philosophers, the philosopher whose thoughts cover the world of science as with a garment, I mean Lord Bacon, said of the medicine, of his day, that it stood for judgment on quite different merits than did other learned pursuits. "Other arts and sciences," he argued, "are judged of by the power and ability exhibited in the conduct of them by their professors, and not by success or by events. The lawyer is judged by the skill of his pleading, not by the issue of the trial; the pilot by his skill in directing the course of the ship, not by the fortune of the voyage. But the physician can perform no particular act by which his ability can be directly demonstrated, and therefore he is principally judged by the event, which is very unjust. For who shall decide, if a patient die or recover, whether the good or the evil is brought about by art or by accident? Whence," says he, "imposture is frequently extolled, and virtue decried. Nay the weakness and credulity of men is such, that they often prefer a mountebank or a cunning woman to a learned physician. So the ancients made Esculapius and Circe brother and sister, and both children of Apollo. Hence," he adds, "physicians say to themselves in the words of Solomon, 'If it befall to me as befalleth fools, why should I labour to become more wise?' And therefore one cannot wonder that they commonly study some other art or science more than their profession, because they find that mediocrity and excellency in their own art makes no difference in profit or reputation; for man's impatience of diseases, the solicitude of friends, the sweetness of life, and the inducements of hope, make them depend upon physicians with all their defects."

Had Bacon spoken these sayings in the present day, he had spoken, with one or two exceptional errors, as truthfully as he spoke in his own time. Had he been a physician, he might indeed have gone further than he did. He might have urged his too frequent inadequacy himself to decide whether his own success rested, in particular instances, on skill or on accident. He might further have added how oftentimes the cheek of the right-minded physician pales or burns with doubt as he hears his own praises declared for skill which he himself cannot for a moment take credit to his own heart. This has been the fate of medicine until our day. On such fate all the quackeries have flourished; on it all the "pathies" and dogmatic systems of medicine have flourished; on it the idea of cure has found too willing acceptance and belief.

At last a change has come over the science of medicine. With true nobleness of purpose, true medicine has been the first to strip herself of all mere pretences to cure, and has stood boldly forward to declare as a higher philosophy the prevention of disease. The doctrine of absolute faith in the principle of prevention indicates the existence of a high order of thought, of broad views on life and health, of diseases and their external origins, of death and its correct place in nature. The doctrine of absolute faith in curative medicine, of power vested in the hands of a distinct sect or class, and exercised by them as by regal right and without the assistance or interference of those upon whom it is exercised, indicates a low standard of knowledge; a too confiding spirit in the wisdom of a minority; a departure too wide from the safe law of self-preservation; and an ignorance of the avoidable causes of diseases; a blindness and therefore an unnecessary exposure to danger; an overweening and sudden fear of dangers of all kinds little and great, and a hasty and thoughtless pursuit after that mode of rescue from dangers of disease which claims for itself the greatest pretensions and boasts the greatest successes.

It shall remain as one of the glories of medicine that she herself has first seen these truths, and, willing to sacrifice her own interests to truth and light, has put them forward without fear, without reward. In the science of prevention medicine takes in fact all the world with her. The science becomes a political, a social, as well as a medical study. It appeals to every mind. When it once is so set forth it fills all men with its teachings. It models itself into household truths and commingles with the moral and even religious elements of life. Admitted for a season into the household, it steps forth again to find its way into the legislature. It becomes eventually a governing science—a law.

This scientific course commenced, must needs go on. But in its going it must needs also change greatly the old face of medicine, and remove in the change the Baconian reproach. I do not think there is much difficulty in foreseeing what in the main the change will be like.

I need not say that the "pathies" will go. The pathies of all kinds are as dead as door-nails, and wait only to be decently interred in a common grave. In time the word cure will go altogether. It is clear already that there is indeed no such thing. A man born to live through a given cycle lives through it free of disease, unless he be stricken from without. If he be stricken, and by the stroke the natural functions, by the exercise of which he lives, are not so disturbed but that they can swing back again in due order, he may recover; if he be stricken beyond this, he will die. Nature will pursue her course undisturbed by either event. She will make no special effort to kill, and assuredly she will put out no special hand to save. A man may intervene, and may, by knowledge, put the stricken body into such a condition that it may swing back into natural course whereby he will have put it into a condition in which it will not die. This is the very highest development of medical art resting on science. But it is not cure, in the common meaning of that term.

By the progress of sanitary science and by its influence on practical medicine we shall attain these perfect rules of management after the infliction of the stroke of disease; and I do not doubt that the art of placing the stricken under such conditions that they may not die will for ever afford scope for the inventive genius of man. The more immediate triumphs will, however, come in that part of the work which is purely preventive. Down from the skies comes the forked lightning and lays a man prostrate. It is a question for the ages who shall place that man in a condition under which he shall certainly swing back again into life. But the preventive art that puts up a metallic rod to divert the lightning from other men, that is the present triumph of human skill; skill which, carried to perfection, shall prevent the stroke and put out the second art by removing the necessity for its application.

With the progress of sanitary science we must expect to see preventive medicine taking the ascendancy. Cure will cease, prevention will grow. Humanly-made epidemics, like the great plague of London, which was planted and reared in the rush-covered floors of domiciles saturated with the organic refuse of years, or like the modern typhoid, which is fed by streams of drinking water uncleansed from human excreta, such self-made epidemics will be prevented by simple mechanical skill. Diseases imposed by indulgence in harmful pleasures and appetites, or by physical overwork and shock, will be removed by the effect of moral influences and knowledge of cause; and gradually, I believe, those persistent evils, which, like the lightning-stroke, come without human ordinance or fault, will be placed also under some protecting care, and, if not removed, reduced to a short calendar.

It is felt by some that the medical Sanitarian of the future will have his best efforts thwarted by the forcible excess of life beyond the means that can be found for the support of life, as if life were a mere secondary principle in the universal order. I see no such cause for fear. That in the progress of life on the earth the day will ever come when the earth will not supply food for its people is to my mind pessimism carried into an insane vulgarity. It is clear that man can always reduce to his wants the lives of all animals except man. The question rests therefore on the abnormal increase of man alone. Nature knows that and rules accordingly. Let man remain savage, and, however sensual he may be, he will die fast enough by war, plague, famine, or luxury. In that state he will never overstock the earth, but either grope in solitary places a neglected family, unprotected from all the killing vicissitudes, or will sink into luxurious barbaric decadence. Let man become exalted in life; exalted by communion with noble pursuits; with pursuits of science, art, letters, and cultivation of greatest happiness for the greatest number, and his sensual life will become too subject to the virtue to leave a chance for the danger which a low sensuality sets up as a terror and at the same time a temptation for the vulgar.

I think it my duty to deal plainly with a question which affects so closely the future of sanitation, and to express, from an experience which is confirmed, as I know, by some of the brightest ornaments of my learned profession, that nothing is wanted to correct the danger of over-population but improvement of mental process; nearer communion with the eternal mind in His works; purer artistic education, healthier homes, more rational amusements, and the ennobling influence of a holier life amongst those who assume to be the cynosures of the nation.

On the whole the prospects of medical learning and action will be greatly improved by sanitary advancement. It is possible that fortunes or reputations resting on faith in famous curers will dwindle slowly away, and that not for long will the skill of the physician be valued by the fallacious reckoning of mere results. But in exchange there will be opened to the physician a career in which skill of labour will be exhibited together with results, the results obvious as to their relation to the work, and both, if good, successful beyond praise.

The Social Part.

The future of sanitary science in relation to social life generally, its effect that is to say on all classes of the community, promises steady progress. No one who has been actively engaged for the past quarter of a century in sanitary work can doubt this statement. Throughout all sections of the community there is desire to know; and if the legislator will be content not to legislate until he sees that free-will guided by knowledge is in the same train with him—it doesn't matter in which class,—all will go well. The workers in our Sanitary Institute though they be not legislators can, nevertheless, greatly assist Parliament by bringing free-will into harmony with knowledge, and though the distinction does not at first sight stand out, in separating free-will from ignorance and from those automatic demonstrations of ignorance which are the outward and visible signs of unhealthy habits of life.

The social work that has to be carried out for the future of sanitary science is purely educational. Educational not merely by lectures and books and lessons from books, but by demonstrations of sanitary works, plans, buildings, mechanisms, results of all labours bestowed on the cause. Without venturing on details of this kind which would land me in another address, I may be content to touch on two points, both of vital moment for the future.

The first of these relates to modes of teaching so as to carry the sympathies of the learner and his more refined tastes along with his reason; to attract and charm his senses as well as his intellect. It is said of us sanitarians, and sometimes I fear with some truth, that we would make health hideous. We need not do so; and if the feat has ever been accomplished it is but the work of a "'prentice han," that ought to be forgiven. Health truly is beauty in the living evidences of it, and should be so in those inanimate evidences which the builder and the engineer construct for us. I would therefore urge that in all coming sanitary work, theoretical or practical, the sanitarian should call the artist also to his side, and that no design of a sanitary kind should ever be executed in which the hand of the artist does not play its beautifying part.

And if I might suggest so much to the imaginative scholars who live to make life sweeter to the many, I would ask them,—poets, painters, sculptors, players, musicians,—to believe that to render practical even their refined labour is to render that labour more acceptable, more diffusible, more durable.

The second topic relates to those who require first to be taught the sanitary lessons of the future. I want strongly to enforce that it is the section of the nation which Dr. Farr classes as the domestic, the six million of women of the nation, on whom full sanitary light requires first to fall. Health in the home is health everywhere. Elsewhere it has no abiding place.

I have been brought indeed by experience to the conclusion that the whole future progress of the sanitary movement rests for permanent and executive support on the women of the country. When as a physician I enter a house where there is a contagious disease, I am, of course, primarily impressed by the type of the disease and the age, strength, and condition of the sick person. From the observations made on these points I form a judgment of the possible course and termination of the disease, and at one time I should have thought such observations sufficient. Now I know them to be but partly sufficient. A glance at the appointments, and arrangements, and managements of the house is now necessary to make perfect the judgment. By this is shown what aid the physician may expect in keeping the sick in a condition most favourable for escape from death; and by this is also shown what are the chances that the affection will be confined to one sufferer or distributed to many. As a rule to which there are the rarest exceptions, the character of the judgment is hereupon dependent on the character of the presiding genius of the home, —on the woman who rules over that small domain. The men of the house come and go; know little of the ins and outs of anything domestic; are guided by what they are told, and are practically of no assistance whatever. The women are conversant with every nook of the dwelling, from basement to roof, and on their knowledge, wisdom, patience, and skill, the physician rests his hopes. How important, then, how vital that they shall learn as a part of their earliest duties, the choicest sanitary code. How correct the decision of the founders of the Sanitary Institute, that from the first they should include sanitarians of both sexes as working associates.

To women more than to men this work is new. To women more than to men this work is hard to realise. Naturally more conservative than men they are moved with less haste to tasks of reformation and reconstruction. More sensitive to criticism than men, they are given, at first, to resent, as if it were an insult to past customs and usages to which they are attached, the suggestion of innovation. But these passing difficulties removed, there is in the hearts of women such matchless generosity, such overpowering love for every device tending to promote the happiness of all things of life, that we sanitarians may indeed be content for the future of sanitary science in its social aspects, if we do no more than win them to our cause and entrust its details to their ministering spell.

PERU[1]

STUDYING in Mr. Squier's new work the records and ruins which attest the civilisation of Peru before the Spanish Conquest, one finds oneself repeating the often-asked question, Did these advanced arts and institutions arise out of native savagery, or were they at least developed under the guidance of ideas imported from the Old World? Mr. Squier holds that they were indigenous, and his opinion (which is that also of Mr. Markham) must have great weight, not only from the minute care with which he has examined the ruins during his two years' exploration, but from his familiarity with the Spanish literature on the subject. Some readers, however, like the present reviewer, while admitting that much of the Peruvian culture has such a stamp of peculiarity that it must be home-made, may not feel quite so certain of the whole being absolutely free from foreign influence. It is much to affirm of a bronze age people like the Peruvians (for particulars and drawings of their somewhat special types of bronze implements, see pp. 175, 579) that they invented this alloy independently. For an excellent case of mingled native originality and similarity to Old World types, attention may be called to the stone-circles of Sillustani, as exemplified by Fig. 1, reproduced from Mr. Squier (p. 384). He calls them "sun-circles," which, however, is begging the question of their as yet unproved purpose. At any rate there they stand, circles of erect unhewn stones like the cromlechs of Europe and Asia, but with a special feature in the surrounding pavement or "platform" of well-fitted hewn stones, with a gutter running all round the circle near the inner edge.

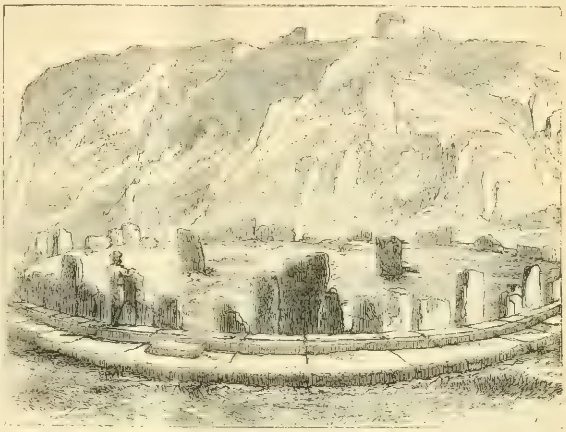

FIG. 1.—Sun-Circle, Sillustani.

On the hill above are seen the ruins of chulpas or burial-towers. Fig. 2 shows two of these, the left-hand one being a beautiful example of building in close-fitting blocks of hewn-stone, an art which had attained in Old Peru to a perfection hardly reached elsewhere in the world. This tower is thirty-nine feet high, and widens as it rises from sixteen feet at the base to nineteen feet at the spring of the dome top. In a still larger chulpa there are hewn trachyte blocks as large as twelve feet long on the curve of the face, by seven feet high, and five feet deep. The stones, fitting together face to face without mortar, are imbedded within in the mass of the structure, which is of rough stones laid in clay. Extraordinary skill in masons' work is shown by these blocks being not only cut in the sides and outside curvature to a radius from the centre of the monument, but in the gradual swell of the structure as it widens out, as well as the curve of the dome, being accurately taken in each block (p. 382). The blocks were not shaped after being put in position, as is proved by numbers of them lying on the ground, perfectly cut to conform to their places in towers that were never finished, so that they were hewn to plans in which every dimension of the structures had been previously fixed. Yet with all this skill there was not the mechanical knowledge to provide anything like pulleys or cranes to hoist the heavy blocks into their places. The inclined planes of earth and stones built up against the chulpas still remain, up which the stones were moved, probably with levers, and possibly with rollers also. Looking at the woodcut, one sees a low opening cut through a block at the base of the tower, just large enough to admit the body of a man; this leads into the circular burial chamber, vaulted with flat, over-lapping stones. This primitive arrangement of the "false arch," which reminds one of those which children make with their bricks, is usual in Peruvian as in Central American architecture. Yet, as if to complicate the problem of architectural history in America, there are exceptional cases where, as at Pachacamac, true arches of sun-dried bricks are still to be seen (p. 71).

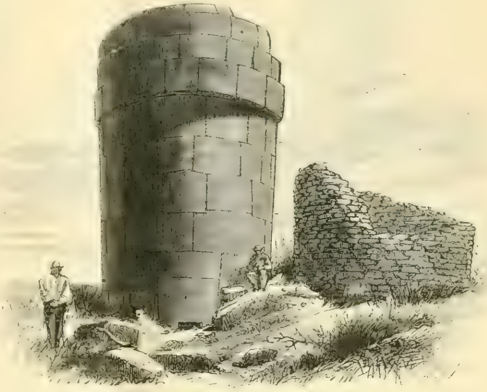

FIG. 2.—Round Chulpas, Sillustani.

Of the more usual Peruvian masonry, where the blocks, accurately faced, are kept in position by their mere bearing on one another, without cement or mortar of any kind, Fig. 3 presents a specimen. It is interesting for other reasons, being one of the Ynti-huatana, or "sun-years," by which the solar year was determined. The following passage from Garcilaso de la Vega's "Royal Commentaries of the Yncas" seems to describe structures of this kind:—

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[1] "Peru. Incidents of Travel and Exploration in the Land of the Incas." By E. George Squier. (London: Macmillan, 1877.)

"The Yncas were also acquainted with the equinoxes, and observed them with great solemnity. . . . To ascertain the time of the equinox they had a stone column, very richly carved, erected in the open spaces in front of the temples of the sun. When the priests thought that the equinox was approaching, they carefully watched the shadow thrown by the pillar every day. The pillar was erected in the centre of a large circle, occupying the whole width of the courtyard. Across the circle a line was drawn from east to west, and long experience had shown them where the two points should be placed on the circumference. They saw, by the shadow thrown by the column in the direction of the line, that the time of the equinox was approaching; and when the shadow was exactly on the line from sunrise to sunset, and the light of the sun bathed the whole circumference of the column at noon, without any shadow being thrown at all, they knew that the equinox had arrived. Then they adorned the pillar with all the flowers and sweet herbs that could be gathered, and placed the chair of the sun upon it, saying that on that day the sun with all its light was seated upon the pillar" (Markham's translation, published by the Hakluyt Society, vol. i. p. 179).

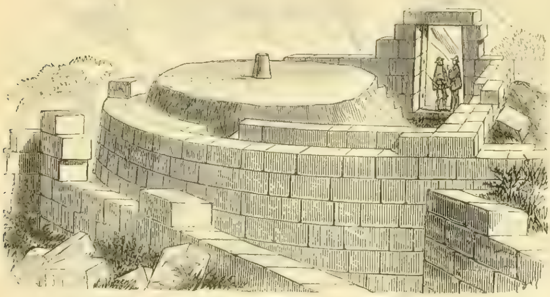

FIG. 3.—The Inti-Huatana of Pisac.

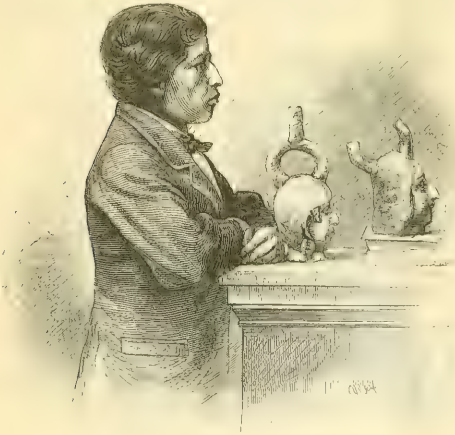

FIG. 4.—Ancient Vases and Modern Peruvian Heads.

It is true that a "stone column very richly carved" seems an inappropriate description of the plain truncated conical gnomon shown in the drawing. It is, however, as Mr. Squier says (p. 525), "sharply cut and perfectly symmetrical." Can this be all that Garcilaso meant by "columnas de piedra ríquisimamente labradas"? or were others of these structures furnished with more ornament? Garcilaso also describes towers near Cuzco erected for determining the solstices; but Mr. Squier considers that his account is confused, and that these so-called towers were only Ynti-huatanas. From this opinion Mr. Markham, writing in the *Academy* for May 19, quite dissents, and probably most readers who go through the whole of Garcilaso's chapter will consider that he had some idea of what he was writing about, and will take it on his (and other) evidence that the Peruvians had, in fact, solstice-towers as well as these equinox-cones. After all one must admit, with Mr. Squier, that the Peruvians had not advanced so far in astronomy and computation of time as the Mexicans and Central Americans.

No traveller before Mr. Squier had thoroughly explored the great lake of Titicaca with its sacred island, celebrated in tradition as the place whence Manco Capac and his sister-wife Mama Ocllo, children of the Sun, and first of the Yncas, came down to govern and civilise Peru. In this cold desolate region, twelve to thirteen thousand feet above the sea, ruins of palaces, convents, and the temples of the Sun and Moon still remain to attest its sanctity under the Ynca rule. Mr. Squier's estimate of the true value to be placed on the traditions of the Yncas is reasonable and moderate. To the warlike genius which enabled them to subjugate the vast land, to the political genius with which they organised the system of communication and social control, which is one of the most wonderful phenomena in the history of nations, he does full justice without countenancing the absurd idea that the whole development of Peruvian culture is to be attributed to this one conquering tribe. His researches, indeed, bring out more clearly than ever the distinctness of much of the native civilisation of Peru from that of the Yncas, whose rule had not been extended over the whole land till near the time of the Spanish Conquest.

The ruins of the temple of Pachacamac, and elsewhere near Lima, show us a people similar in origin and language to the Yncas, but who had their nationality and culture before the conquering tribe came down upon their coast from the high valleys of the Andes. The Chimus of the Truxillo district spoke a language still known in some villages, and said to be quite distinct from the Quichua of the Yncas. Yet these people had attained to peculiar skill in metal-work and pottery. Indeed from this district come the well-known Peruvian vessels with double spouts or double bodies often modelled in the form of an animal or a pair of animals, and with a kind of whistle uttering the creature's proper cry when the vessel is tilted so that the water in it forces air in and out through the hole. Not less curious are the well-modelled head-vases, which (Fig. 4) give us the means of comparing the features of the ancient and modern inhabitants. With reference to this and other drawings here reprinted from the hundreds contained in Mr. Squier's volume, he may be congratulated on the thoroughness with which he has enabled his readers to understand the book which suggested his exploration—Prescott's "History of Peru."

EDWARD B. TYLOR

THE METEOROLOGY OF THE FUTURE

A VISION

FREELY translated from the Japanese by one who is thankful that he does not live in Japan. The translator would remark that in importing our Western institutions it is of great consequence to appropriate likewise the spirit which pervades them. "Dead flies cause the ointment of the apothecary to send forth a stinking savour" (old Eastern proverb).

(*N.B.—This translation is dedicated to the Council of the Royal Society.*)

A QUIET people are the dead,
What stillness do they keep!
The battle rageth overhead,
But marreth not their sleep.

And yet that this is sometimes broke
Hath been revealed to me;
'Twas in eighteen hundred and eighty-one
At the bottom of the sea.

There lay poor Jack, his perils past,
No more to turn the quid,
Nor pipe his eye (since barnacles
Were feasting on the lid).

Long thin sea-ropes in tangled coils
Were round and round him curl'd,
Yea, scaly things above him swam,
Down in that green sea-world.

He could not weigh his anchor up,
He could not heave astern—
And thus in my bewildered ear
He spun his doleful yarn.

"*'Twas not the storm that brought me here*
"*In Davy Jones's grip,*
"*But 'twas because my mates and me*
"*Sailed in a rotten ship.*"

I answered him: But knowst thou not
That Plimsoll's noble band
Board every ship, and punch its ribs
Before it leaves the land.

No rotten beam but would be seen
By such a skilled detector—
The dead man groaned—"*Alas! dear mate,*
"*They jobbed the ship's inspector.*

"*Not two hours' sailing from the Nore*[1]
"*The wind began to veer,*
"*The storm was strong, the ship was weak,*
"*And we were driven here.*"

Belay! belay! thou man of Death,
Thy yarn won't hold together,
Dost thou not know we have a board
That telegraphs the weather?

A board that sits both night and day,
With facts and figures stored,
Why man alive!—The dead man groaned—
"*Alas they jobbed the board.*"

Why dost thou groan thou man of death?
Why so blaspheme and cuss?
Their chairman sure was not the smith
To forge thy fetters thus?

His was a mind of many sides,
Well filled with *a* and *b*,
And *x* and *y*, and likewise *z*—
"*But he didn't know the sea.*"

As thus he spake I forthwith said—
Well, even if this be true,
The captain makes not all the ship,
Now, what about the crew?

There's one I know—"Divine" doth best
Express his god-like presence;
"*He knew the sea but never dived*
Beneath its phosphorescence."

Then, I replied, there was a third,
In fame to none will yield,
He led the band who reaped renown
On India's famine field.

Was he the man to see thee die?
Thou wilt not tax him—come?
The dead man groaned—"*I met my death*
Through a sun-spot maximum."

One more—the rover of the crew—
Hath sailed o'er many seas;
Come now, be reasonable, he—
"*Was busy shelling peas.*"

I wakened up in sheer disgust,
And to myself I said,
The living man for prejudice
Is beaten by the dead.

THE SPECTRA OF CHEMICAL COMPOUNDS[1]

IN this paper Mr. Moser discusses the question, whether chemical compounds have a spectrum of their own or whether they only show the superposed spectra of their elements. To those who have worked at the subject the question can hardly be called an open one. Ever since the too much neglected work of Mitcherlich it was proved that each compound has its own characteristic spectrum, and whatever evidence subsequent workers have added to the question, the merit of having decided it belongs solely to Mitcherlich.

Nevertheless Mr. Moser's work is a valuable one and for several reasons. Other questions still at issue are intimately connected and cannot be discussed without once more referring to Mitcherlich's work. Most of these questions are not as yet amenable to strict proof but must be decided by the common sense of those who work at the subject. It is therefore of importance that as many as possible should take up the question, and though each worker may add little to the stock of knowledge, the consensus of opinion, thus established, will advance the subject materially. It is a pleasure to find that Mr. Moser has apparently arrived at the same theoretical views which have formed the leading string during the last years to the experimental work of Lockyer and others in this country. Mr. Moser mentions the suggestion of Prof. Helmholtz that the line-spectra are due to the vibrations of an atom while the band spectra are due to the more complex molecule. This is precisely the view first put forward by Lockyer, and it has thus received a striking confirmation from an independent quarter.

Mr. Moser experiments on the changes which certain absorption spectra undergo by a variation first of the thickness of the absorbing layer, and secondly of the temperature. With regard to the variation of thickness of the absorbing layer the conclusions seem simple enough. The bands get darker and wider. The influence of temperature is divided into two parts. On the great majority of bands the increase of temperature has apparently the same effect as an increase of mass; that is, it increases the absorption.

It is more than probable that this increased absorption is really due to an increase in the number of molecules giving the absorption spectrum in question. If a gas is near the temperature at which it condenses, its molecules as a rule aggregate before they finally pass into the liquid state. Each molecular aggregation has its own spectrum, and thus a change of temperature producing a difference in the relative number of molecules in these different states will increase the absorption of one spectrum, while it weakens some other absorption spectrum. If the spectrum which gets weaker lies in the invisible part, the only visible effect of temperature is to increase the darkness of the whole observed spectrum. In many cases, however, we can follow out not only the increased absorption of one spectrum, but at the same time the gradual breaking up of another spectrum, and this affords a beautiful illustration of the gradual dissociation which we can now trace step by step.

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[1] Of course this word does not occur in the original—only its Japanese equivalent.

[1] By James Moser (*Pogg. Ann.*, vol. clx., p. 177.)

In order to make my meaning clearer I shall trace in detail the effect of an increase of temperature on the absorption of sodium vapour. If we heat up sodium in an iron tube we observe at first a continuous spectrum stretching from the red and blue end of the spectrum towards the green. If the mass of sodium vapour is sufficiently great the two banks will join, and the whole spectrum will be absorbed. If we now raise the temperature the light will force its way through the bank in the green, and then, almost as suddenly as if a curtain was withdrawn, the spectrum will open from the green towards both sides. The continuous absorption now has entirely disappeared, but a series of finely-shaded bands are seen. The D-line will soon make its appearance, and if we now continue to increase the temperature it will increase in darkness, and we shall soon arrive at a point where the bands quickly fade away, and as we see them disappearing, we see the whole absorption thrown into the D-line, which gets thick and of an intense black colour.

No one who has ever actually observed this fact can for a moment doubt that we have here to do with a real breaking up of the band-molecule, as we may call it, into the simpler molecular state in which it gives the line absorption. Similarly at lower temperatures the band-molecules aggregate and give the continuous spectrum of which we have spoken.

The three vapours which Mr. Moser has examined are: Iodine, bromine, and the oxides of nitrogen. That the darkening of the absorption bands of iodine is due to a breaking up of a more complex molecule is shown by the fact, discovered by Lockyer, that there exists a molecule of iodine which gives a continuous absorption, and which is more complex than the band-molecule. There can be little doubt that this also is the explanation of the analogous phenomenon in bromine vapour. The brown vapour of the oxides of nitrogen are known to be a mixture, and it is, therefore, not surprising that the relative quantity of the different oxides changes with the temperature.

It is, indeed, the second effect of temperature observed by Mr. Moser, that some of the bands disappear as the temperature is raised. There can be little doubt that in the case of the oxides of nitrogen, the three disappearing bands are due to a compound which is broken up as the temperature is raised.

A curious alteration is noticed in the case of iodine and bromine vapour. In each case one band was observed to disappear and to be replaced by a number of fine equidistant lines. I noticed some time ago a similar replacement of a fluted band by a number of fine lines approximately in the same place in the emission spectrum of carbonic oxide. The change requires a more careful study before any decided opinion can be given as to its cause.

ARTHUR SCHUSTER

OUR ASTRONOMICAL COLUMN

THE LATE PROF. SANTINI.—Giovanni Santini, Professor of Astronomy and Director of the Observatory in the University of Padua, whose decease has been announced in the daily journals during the last week, had nearly completed his 91st year, having been born at Borgo S. Sepolcro, in Tuscany, on the 30th of June, 1786. Educated at the University of Pisa, he applied himself especially to the study of the exact sciences, and in 1814 was appointed successor to Vincenzo Cheminello at the Observatory of Padua. He subsequently became Rector of the University and Director of Mathematical Studies, to which position was attached the Professorship of Astronomy.

Santini was the last of a phalanx of distinguished practical astronomers among whom were Argelander, Bessel, Carlini, Encke, Mädler, Struve, and others, which will long live in the annals of the science. The work with which he has been more particularly associated and brought into prominence in the astronomical world, relates to the celebrated comet of Biela, the perturbations of which body were calculated by him upon a uniform system from 1826, when the comet's period of revolution was first determined, to 1859, for which year he prepared an ephemeris, though the track in the heavens was too unfavourable to permit any hope of observations. Santini's memoirs on Biela's comet appear partly in *Memorie dell' I. R. Istituto Veneto di Scienze, &c.*, earlier ones in the *Transactions* of the Academies of Padua and Modena. His colleague, Dr. Michez, took up his work for 1859 and continued the calculation of perturbations to 1866, in which year the comet was sought in vain, and we have no further computation of the effect of planetary attraction upon its motion.

Amongst the other astronomical works of Prof. Santini are several catalogues of stars in the neighbourhood of the equator, or from declination + 10° to − 12°, for the epoch 1840, in which the differences from the positions in Bessel's zones are exhibited. Also an investigation of the mass of the planet Jupiter, from observations of the elongations of the fourth satellite, made at Padua in the first four months of 1835, which gave a result confirming the value deduced by Sir George Airy by similar observations at Cambridge a short time previous, Airy's figure being $\frac{1}{1048 \cdot 69}$ and Santini's $\frac{1}{1049 \cdot 2}$. He was the author of a valuable work for the student, "Elementi di Astronomia," which contains a great amount of information relating to the practice of astronomy, that can hardly yet be said to have become antiquated, though the second and last edition appeared in 1830; the writer of these lines gladly acknowledges his indebtedness to this work, when a student of astronomical methods of observation and calculation, some thirty years since. Santini was elected a correspondent of the Institute of France (Academy of Sciences) in 1845.

THE DOUBLE-STAR 72 OPHIUCHI.—The close companion to this star was detected by M. Otto Struve, with the Pulkowa 15-inch refractor, in 1842. In a note to his catalogue of 1850, he remarks: "I have looked at this star very frequently, and have noted it many times as a single star. On three occasions, however, I have seen it double, always very nearly in the same direction, and at a distance of 1"·5. I do not know how to explain these discordances, except by supposing that the light of the satellite is very variable." In 1847 the angle was measured 166°·3, and the distance 1"·59. Dawes states he had examined the star with different telescopes, including Mr. Lassell's 20-feet reflector, but had never obtained a glimpse of the companion. Secchi thought he saw it double in July, 1857, but placed small reliance upon the observation; in August, 1859, he obtained undoubted evidence of duplicity; the angle was 3°·75, and the distance 0"·604 for 1859-61, magnitudes 4 and 7; he remarks on this occasion: "E certamente doppia, e ben separata." The suspicion of the discoverer is, therefore, likely to be confirmed when large instruments are brought to bear upon the object frequently.

A REPORTED OCCULTATION OF MARS BY VENUS, A.D. 368, JULY 30.—Amongst the observations extracted from the Chinese Annals by the Jesuit missionary Gaubil, and printed in the *Connaissance des Temps* for 1810, is one which is thus translated: "An 368 = 3ᵉ année Tai-ho, 6ᵉ lune, jour Kia-yn (30 juillet) Venus éclipsa Mars." It may be worth while to examine how far M. Leverrier's tables will represent this reported observation. Calculating for July 30 at 0h. and 8h. Paris mean time, we find the following quantities:—

SUN.

True longitude.		Log. earth's radius-vector.
° ′ ″		
127 10 31·4	... ...	0·0045397
127 29 50·7	... ...	0·0045058

VENUS.

Geoc. longitude.	Geoc. latitude.	Log. radius vector.
° ′ ″	° ′ ″	
166 25 48 ...	+ 0 30 59 ...	9·860940
166 49 12 ...	+ 0 29 53 ...	9·860966

Distance from the earth at noon, 1·1263.

MARS.

Geoc. longitude.	Geoc. latitude.	Log. radius vector.
° ′ ″	° ′ ″	
166 35 25 ...	+ 0 34 50 ...	0·196730
166 48 17 ...	+ 0 34 37 ...	0·196623

Distance from the earth at noon, 2·2168.

Hence the conjunction in longitude would occur July 30 at 7h. 18·5m. Paris mean time, or July 30 at 15h. 4m. mean time at Nanking, where the Chinese Court was then established; Mars north of Venus 4′ 39″. Consequently, according to M. Leverrier's tables, there would be no occultation, but a very close approach, and considering the calculated time of conjunction, at an early hour in the morning, whereas the planets would be evening stars, it would appear that an occultation was only inferred from some previous or subsequent observations or both. The accuracy of the Chinese record, so far as regards a near appulse of the two planets about the time named, is however confirmed.

THE PREHISTORIC STEPPES OF CENTRAL EUROPE

IN a recent number of the *Magdeburgische Zeitung* is an interesting article by Dr. A. Nehring, upon the former extension of the steppes of Russia into Northern Germany. The country between Magdeburg and Halberstadt now belongs to one of the most fruitful and best cultivated districts in the Fatherland. Yet there are good grounds for believing that in former days this country was for a lengthened period a steppe—probably not an isolated steppe, but connected on the east with the great steppes of Russian Asia.

Northern Germany, including the country designated above, is generally regarded as having been in former times either overflowed by the sea and beset by icebergs, bringing down erratic blocks from the Scandinavian ice-mass, or, as Cæsar and Tacitus subsequently found it, covered with thick forests and extensive marshes. Both these views are correct—the one for what is usually called the Diluvial epoch—the other for the period immediately preceding historic times. Yet we may be allowed to ask what was the state of things in the intervening period? that is, after the sea had left the plains, and before the wood from the neighbouring heights had grown over it. It is probable that the former sea-bottom, which made its appearance as a sandy plain saturated with salt, in many parts of North Germany became a steppe. The same thing has happened in other parts of the world.

A steppe need not necessarily be quite flat. Within the range of the extensive plains of the present steppes and prairies there are not unfrequently hills and undulating or elevated surfaces, and rocks breaking the uniform level. The absence of wood is characteristic; the sandy surface is covered with grasses, dwarf herbs, and stunted bushes, which increase rapidly after the rainy season, but fade away altogether in the dry season, and present the appearance which we generally associate with the word "steppe." The soil [is not altogether unfruitful, for the sandy loam is much appreciated by many kinds of plants. It is only where the former sea-bottom consists of pure sand that herbage is altogether deficient. We designate as deserts such tracts of land—especially when they occur in hot countries. The soil of the "steppe" proper is often very fruitful, but its defect is that it has no regular supply of water, being only here and there varied by streams, marshes, and lakes, the latter generally salt. In the neighbourhood of such water a continuous vegetation may be developed, but the greater part of the steppe is covered with herbage for only a few months after the rains, and this disappears as quickly as it grows. Heat and cold, drought and flood, luxuriance and want succeed one another very rapidly.

The fauna of the steppe is, in its most obvious features, quite peculiar. Such of its animals as live on the ground and cannot escape the bad season of the year by emigration, become so accommodated to the climate and soil that they are never met with in other places—that is in woody or marshy districts. Among these the steppe-rodents (such as Jerboas, Sousliks, and Voles) are most remarkable. They find sufficient sustenance in the twigs, leaves, and berries of the steppe-plants. The dry sandy soil is well adapted for their underground dwellings, which protect them from the severe winter and from the attacks of the beast of prey. Let us take, for example, the steppes which lie between the Lower Volga and the Upper Ob. The characteristic animals of this district are (1) the large Jerboa (*Alactaga jaculus*); (2) several species of Souslik (*Spermophilus altaicus*, &c.); (3) the Steppe-marmot (*Arctomys bobac*); (4) the little Piping-hare (*Lagomys pusillus*); (5) the Wild Ass (*Equus onager*); (6) the Saiga Antelope (*Saiga tartarica*). The remaining mammals met with, whether as residents or as temporary visitors, belong either to the fauna of Central Europe or to that of Northern Siberia.

Now this is exactly the same series of mammals which Dr. Nehring's continued researches in the stone quarries of Westeregeln (in the Circle of Manzleben) have brought to light. As regards the number of individuals the steppe-mammals show an undoubted predominance. The most numerous are the Jerboas and the Sousliks, which must formerly have inhabited the neighbourhood of Westeregeln in large numbers. Nearly quite as numerous must have been the wild asses, of which the teeth and bones occur in large masses. There are also many remains of Voles, mostly of such species as at the present time are only found in Eastern Europe and Western Asia. Of the marmot and piping-hare Dr. Nehring has at present only single examples, but expects to find more as his excavations continue.

Of the characteristic steppe-mammals mentioned above the Saiga is the only one not yet found at Westeregeln. But it is to be expected that it will yet be discovered there, because the whole facies of the extinct fauna indicates its former presence, and the Saiga has already been found fossil in several places further to the west. Perhaps also a specimen of a supposed large sheep (*Ovis*), which was obtained some years ago near Westeregeln, may really have belonged to the Saiga.

On the whole, if we put the Saiga aside, the Diluvial fauna of Westeregeln seems manifestly to have been a steppe-fauna, and brings us to the conclusion that the district in which these animals dwelt must have been a steppe of similar character to that which now extends between the Volga and the Ob, and perhaps have been even in direct connection with it. That the animals, the remains of which are found in the Diluvium of Westeregeln, actually lived on the plains under the Harz, Dr. Nehring has proved satisfactorily by his own researches. It follows that this country must have formed a steppe during a certain portion of the Diluvial period.

If the above conclusions are right it follows that in a former epoch those parts of Central Europe which were formerly covered by the sea generally became steppes before coming into their present condition. Perhaps the Magdeburg-Halberstadt steppe extended southwards over Aschersleben and Halle into the valley of the White Elster, for Prof. Liebe has found, near Gera, fossil remains of several specimens of the large Jerboa, as also of the Souslik and other animals which have been obtained at Westeregeln. Besides, remains of the same animals, as well as those of the Saiga antelope and wild ass, have been found at several other points to the west. It follows, therefore, that the steppe must have extended considerably in that direction.

The result of these investigations is the more important as manifest traces seem to show that at the Steppe period man had already occupied the plains of middle Europe, and occasionally took up his abode even on the ancient steppe of Westeregeln.

The cause of the disappearance of the ancient steppes of Central Europe Dr. Nehring supposes to have been the gradual increase of the forests which advanced along with the change of climate. In the Steppe period England and Scandinavia were still joined to the continent of Europe, the North Sea and the East Sea did not exist in their present extent, the Gulf Stream had a more northern direction, and the climate was drier and more severe than it now is. As the climate softened and the forests advanced from the wooded hills, the steppe animals gradually withdrew themselves towards the east, and disappeared, leaving only fossil remains to attest their former abundance.

THE COLORADO BEETLE

WE have already several times referred to this destructive insect, and now that it has reached Europe we give an illustration of the creature in its various conditions, along with some notes which have been forwarded to us by Mr. Andrew Murray. The Board of Trade have reissued the circular, with a coloured illustration, referred to in our article on Our Insect Foes, vol. xv., p. 85.

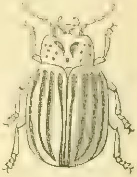
The Colorado beetle belongs to that subdivision of vegetable-feeding species known as Phytophaga. It may help the general reader to an appreciation of their place and character if we mention the Turnip flea as a British species of this section, and still nearer to it those brilliant green little gems that are to be seen in quantity on the leaves of the white nettle (*Lamium album*) in summer, and which in Scotland bear the colloquial name of Virgin Maries, an appellation, however, which is also there sometimes indifferently given to the ladybird. The genus in this great section to which these little insects equally with the Colorado beetle belong is named Chrysomela. It is true that its first describer, Say, named it *Doryphora decemlineata*, and that at first that designation acquired such extensive currency that it was all but universally adopted; and many people from old habit or deference to general usage, even when they know it to be an error, still use it; but all entomologists know that it is not a Doryphora, but a portion of the great genus Chrysomela, without going into other details. The difference between them can be very easily pointed out by one single character. Doryphora is a massive Chrysomela with a strong spike projecting forwards from the middle (the mesosternum) of the under side, while Chrysomela has no such spike. The former is a genus peculiar to the South American region, including Central America, and contains the largest, finest, and most beautiful species of the family. Some of them are somewhat similarly marked to the Colorado beetle, which no doubt led to Say mistaking the genus. But although the Colorado species and its allies are clearly enough Chrysomelæ, systematists in arranging that genus have broken it up into several sub-genera or new genera, and the latest authority (Chapuis) has placed them in a genus named Leptinotarsa, but at the same time indicates his opinion that a further subdivision must take place, which will leave the Colorado beetle and its relations in a sub-genus by themselves, as was long ago (1837) proposed by M. Chevrolat, under the name of Polygramma. For the characters of these subdivisions we refer to M. Chapuis's genera, and for the specific characters of the species falling under Polygramma to Stahl's diagnosis and Mr. Riley's first Missouri Report (1869). These are, first, the true Polygramma (*Decemlineata*, Say) that has occasioned all this alarm, which is the most northerly species, its native home being the eastern base of the Rocky Mountains and the prairies extending eastward. Next a variety called *Multilineata* by Walsh and Stahl, but which is not specifically distinct. Then a good species, *Polygramma juncta*, which ranges through the Confederate States of North America, and is easily recognised by two of the dorsal black lines uniting to make one thicker one. Farther to the south, about Vera Cruz and Costa Rica, &c., there is another species very like the Colorado species, called *Polygramma undecemlineata*, Stahl, and which is found in enormous numbers in these parts of Mexico. Lastly Stahl records another, which we have not seen, from Mexico, under the name of *novemlineata*. All these feed on different plants, although probably plants all belonging to the same order, the Solanaceæ.

Potato leaf with eggs of Colorado Beetle on under side.

Larva of Colorado Beetle: natural size.—NOTE. The double row of black spots along the abdomen is not sufficiently distinct in this cut.

Pupa of Colorado Beetle; natural size.

Colorado Beetle; natural size.



Magnified Sketch of Colorado Beetle.

NOTES

THE Society of Arts has awarded its Albert Medal "for distinguished merit in promoting Arts, Manufactures, or Commerce, to Jean Baptiste Dumas, member of the Institute of France, the distinguished chemist, whose researches have exercised a very material influence on the advancement of the Industrial Arts." The Society's Medals for papers read during the session, have been awarded to Prof. Barff, M.A., for his paper on "The Treatment of Iron for the Prevention of Corrosion;" J. Meyerstein, for his paper on "Stenochromy, a Novel Method of Printing in Colours;" A. J. Ellis, F.R.S., for his paper on "The Measurement and Settlement of Musical Pitch;" B. St. John Ackers, for his paper entitled "Deaf not Dumb;" Commander Cameron, R.N., C.B., for his paper on "The Trade of Central Africa, Present and Future;" James Irvine, for his paper on "Our Commercial Relations with West Africa, and their Effects on Civilisation;" Sir Douglas Forsyth, C.B., K.C.S.I., for his paper on "The Progress of Trade in Central Asia;" W. Thomson, for his paper on "The Sizing of Cotton Goods."

A CIRCULAR has been issued by Messrs. Rowe and Groser, the hon. secretaries of the British Association Reception Committee at Plymouth, giving some interesting information about that town. Appended to this is a useful table of the tourist fares to Plymouth from some of the principal stations in the kingdom. Besides Mr. Warington Smyth, Prof. Odling and Mr. Preece have been named as lecturers. One of the excursions is likely to be to Exeter; at least the inhabitants of that interesting city are taking active steps to bring this about. Those of the members who were at the Exeter meeting of 1869 have no doubt many pleasant memories of the visit. The fine museum, which was only completed in time for the reception of the Association, is now filled and admirably arranged under the guidance of Mr. W. S. M. D'Urban, F.L.S. The Dublin people have already begun to prepare for the meeting in that city in 1878. A meeting was held in the Mansion House the other day, under the presidency of the Lord Mayor, when it was announced that subscriptions had already been received to defray expenses. Dr. Ball is one of the hon. secretaries.

AT the conclusion of the last meeting of the Royal Astronomical Society, as we stated last week, a special meeting was held to consider a proposed alteration in the bye-laws. The following is a short account of the business transacted:—Before the last election of officers of the Society (in February) two or three of the fellows printed a balloting list of their own, and having circulated it amongst the fellows without any indication of its private origin, many of them used it as a polling paper at the election, under the impression that it had been issued by the Council. The election was also influenced in another way by the putting up for secretary the name of a fellow who had declined to serve. By these manœuvres a curious anomaly in the bye-laws was made effective—so effective indeed that one member was elected to the council by a few votes, whilst another who had obtained a far larger number of votes was ruled not elected. With the view of avoiding such thwarting of the will of the society in future, the council appointed a committee to revise those of the bye-laws which bore upon the subject. The present special meeting was called to consider the proposed alterations, and they were now submitted for approval or rejection. An amendment, however, which was proposed by Lord Lindsay, the foreign secretary, was carried, deferring their consideration until after the next election of officers.

A VERY satisfactory Report by the Savilian Professor of Astronomy, Oxford, as Director of the University Observatory, has been made to the Board of Visitors for the year between June 1876 and June 1877. The instruments all seem to have worked well except the sun spectroscope, which became seriously deranged in the month of August last, and has not yet been reinstated. 426 photographs of the moon (making altogether, to this date, 652) have been taken with the De La Rue reflector during the year; of these by far the greater number appear to be suitable for micrometric measurement. This will be systematically applied so soon as the Observatory is in possession of the costly micrometer now in process of construction by Mr. Simms, and which is to be the gift of Dr. De La Rue to the Observatory. Then will commence the difficult but interesting research relative to the amount of the physical libration of the moon. 259 complete measures of 117 double stars have been taken during this year with the great equatorial. A second set of observations of six of the satellites of Saturn has been completed. These comprise thirty measures of the co-ordinates of Titan, twenty of Rhea, fourteen of Iapetus, fourteen of Dione, twelve of Tethys, and two of Enceladus. The sun's chromosphere had been observed and delineated on twenty-two days. Eight measures of the difference of the R.A of Venus and λ Geminorum, and seven of the difference of declination, were taken at the time of their conjunction. Preparations are being made for observations of the planet Mars at its approaching opposition, with a view to the determination of solar parallax. For this purpose the director has devised a new form of micrometer capable of measuring with the requisite exactness distances to the extent of forty minutes of arc. If this instrument realises his expectations he thinks it may supersede Bessel's heliometer. It may properly be called a *duplex heliometer-eyepiece.* The director suggests the advisability of now printing the Proceedings of the Observatory.

A STATUE has been erected at Nancy by public subscription, to Mathieu de Dombosk, the creator of the Roville experimental farm, and one of the earliest scientific agriculturists of France. He was born at Nancy on July 30, 1777.

THE Prussian Government has ordered Berlin to be connected with Hamburg by a subterranean telegraph, in order to avoid perturbations during thunderstorms, which have been very frequent this spring. Similar measures will be adopted for other large towns in Germany.

M. GAUTHIER VILLARS has published in Paris a volume of logarithms, containing tables for all numbers from 0 to 434,000,000,000 with twelve decimals, by M. Namur, secretary of the École Moyenne of Thuin-on-Sambre (Belgium). This wonderful volume, selling at three francs, has been printed by order of the Royal Academy of Sciences of Belgium.

ON Wednesday, June 27, the Harveian Oration at the Royal College of Physicians was delivered by Dr. Sieveking. The orator vindicated the claims of Harvey as the true discoverer of the circulation of the blood, the merit of which had been last year publicly ascribed by the Italians to their countryman Andrea Cesalpino.

LIGHTING experiments with gramme machines are being tried daily at the Palais de l'Industrie, in Paris. The area of the building is 2½ acres, the elevation of roof 95 feet. This immense space has been lighted *à giorno*, with two electric lustres each composed of six electric lamps. The motive power required is fifty-horse power, and the results are very satisfactory, although it has not been stated whether they are superior to those of the Alliance system, and Jablochkoff electric candle. The Great Northern Railway Company regularly use electric lamps for their luggage room. The Paris-Lyons Railway is preparing an experiment for the illumination of the whole of the large Paris station. All these experiments are conducted with the intention of testing several electric systems, in order to obtain an immense lighting power for the 1878 international exhibition.

TOWN Councils are seldom noted for either their intelligence or their foresight. We are glad to find, however, there is at least one exception, in the Exeter Town Council, who have decided to postpone the purchase of their town gas-works, "on account of the success of the electric light, and the probability of its superseding gas." This is creditable to the Exeter Councillors, who, we believe, have been the first public body in this country to recognise the value of this latest application of electricity. We hope their expectations will be fulfilled.

A ROMAN correspondent of the *Times*, writing with reference to the shower of sand which occurred at Rome on June 22, sends a translation of the remarks of Father Joseph Lais, published in the *Voce della Verità*:—"The rain of sand continued although to a smaller extent, on the 23rd of June, on which day the heavens were deeply overcast. The sand fell in small perfectly spherical masses of about 1-25,000th of an inch in diameter, at a *maxi-*

mum. It would appear that vesicular vapour, by the action of the wind, had cemented the grains of sand so as to form globules, analogously to what we see on a larger scale in the formation of hail. We are entitled to assert this, seeing the speedy disaggregation of these globules into grains of sand, when brought into contact with a little drop of water in the field of a microscope. The fall on the 22nd was so abundant in Rome that from the amount, 0·25 gramme, gathered on an earthenware disc of 30 centimetres in radius, we argue a fall of not less than eight quintals per square kilometre." The correspondent himself writes: "I am by no means satisfied that the rain was of sand and water. The drops on my drawing paper were easily absorbed by a pocket-handkerchief, and left no stain on the paper; but my drawing still bears many stains from drops which apparently I had not touched. Since then I have washed the sky over with them, and have afterwards sluiced the surface of the paper with water from a sponge; yet there they remain. If sand they be, that material appears to have a most unusually tenacious affinity for the paper. If the drops were of sand and pure water I should expect to find that as soon as the water had evaporated, the sand would no longer adhere to the paper and that the spots would no longer be on my drawing."

THE *Melbourne Argus* informs us that on May 11 the tide rose from five to eight feet on the eastern coast of New Zealand, and that at Sydney and Newcastle, on the New South Wales coast, the tide also rose above its usual height, though in a less marked degree. It will be noted that the great earthquake-wave which did so much damage to the coast towns of Peru occurred on May 10, the time of propagation of the wave from Peru to New Zealand being, however, not yet precisely stated.

SIGNOR GESSI, the celebrated African explorer, while proceeding to the Lake District, had all his scientific instruments and baggage burnt.

MESSRS. MACMILLAN AND CO. have in the press, and will shortly publish, a translation of Fleischer's Volumetric Analysis. In this work the author's aim is to systematise the volumetric processes. A general scheme of analysis without previous separation of bases is also a feature of the work. The translation is made by Mr. Pattison Muir of the Owens College. The translator has added a few notes and supplementary methods.

THE latest news from Yeniseisk announces the passage through this place of MM. Wiggins and Schwanenberg, on their way towards the north. Capt. Wiggins goes towards his steamer, which has wintered at Zureika, and after having taken on board the tallow he proposes to export, he will return, *viâ* the Kara Sea to England. M. Schwanenberg proposes to undertake an exploration of the graphite mines of the Yeniseisk district, and to take a cargo of graphite to Europe. There is, however, little hope that this latter project will be realised.

WE are glad to learn from the Annual Report of the New Russian (Bessarabian) Society of Naturalists that this young scientific body has displayed during the past year great activity. The following are the more important papers published by the society:—On the family of ephemerides from the stand-point of the Darwinian theory, and on the metamorphoses of axolotls, by Prof. Mechnikoff; the theory of chlorophyll, by Prof. Wolkoff; the algolic fauna of the Black Sea, by M. Rishavy; on the laws of distribution of electricity on surfaces, by Prof. Umoff. The society has, moreover, carried on a considerable number of scientific explorations in various parts of Russia, and has continued the publication of a cryptogamic herbarium of Russia.

THE application of new materials for paper stock which has occupied so much attention of late seems to have attracted some notice in the Philadelphia Exhibition last year. From Jamaica bamboo was perhaps the most important paper-making plant exhibited. Of the young bamboo stems, which are the best for the purpose, a very large supply, it is said, could be annually, by systematic cuttings or croppings, furnished from plants flourishing in the humid parts of the island. It seems that the American paper manufacturers have also wished to make experiments with bamboo with the view, if possible, of introducing it into the American trade; so that, owing to the proximity of Jamaica to the United States, it is supposed that the supply of bamboo may eventually form an article of trade between the two countries.

THE additions to the Zoological Society's Gardens during the past week include two Pig-tailed Monkeys (*Macacus nemestrinus*) from Java, a Black Leopard (*Felis pardus*) from India, two Argus Pheasants (*Argus giganteus*), a Vieillot's Pheasant (*Euplocamus vieillotti*) from Malacca, presented by Sir Harry St. George Ord, C.B.; a North American Reindeer (*Rangifer tarandus*) from Newfoundland, presented by Capt. Edmund Fraser, 60th Royal Rifles; a Javan Chevrotain (*Tragulus javanicus*) from Java, presented by Mr. William Trent; an African Cobra (*Naia haje*) from the Cape of Good Hope, presented by Mr. Eustace Pillans; a Hawk-headed Parrot (*Deroptyus accipitrinus*) from Brazil, purchased; ten Amherst Pheasants (*Thaumalea amherstiæ*), two Temminck's Tragopans (*Ceriornis temminckii*), twenty Common Boas (*Boa constrictor*), born in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The following gentlemen were, on Saturday, June 30, elected, after open competition, to demyships in Natural Science at Magdalen College:—Mr. J. F. Heyes, of Liverpool College; Mr. R. V. Jackson, of Clifton College; Mr. G. A. Buckmaster, of Christ's Hospital and St. George's Hospital, London; Proxime accessit, Mr. A. M. Jackson, Magdalen College School, unattached student. These demyships are of the value of 95*l.* per annum, and tenable for five years from the date of election.

BRISTOL.—The first session of University College terminated on the 30th ult. without any special ceremony of prize-day or commemoration. The result of the work of this the first year must be considered very satisfactory, for in spite of several serious disadvantages, the lateness of the arrangements and appointments of last autumn, and the inconvenience of the crowded temporary premises, upwards of four hundred students have been enrolled. This number exceeds that of the first year of either the Newcastle or Leeds Colleges of Science, or of the Owens College. Lectures have also been delivered at Stroud in connection with the clothworkers' industry in the departments of textile fabrics and chemistry. Prof. Rowley has also delivered a course of lectures in literature at Bridgewater. Most of the courses of instruction only extended until Easter, when several of the temporary appointments expired. In consequence of this arrangement the numbers attending the classes in the third term has not been so great as in the preceding. The chemical laboratory has been in full swing, and evening practical classes have been added since Easter. The only reappointments hitherto concluded are the professorships of chemistry and modern literature, the lectureship in experimental physics, and the assistant lectureship in chemistry. The other reappointments are held over until the election of a principal, which will take place during the present month. It is understood that there are sixty candidates for this important post. No provision has yet been made for a lectureship in engineering.

ST. ANDREWS.—We understand that Prof. Fischer, the present occupant of the chair of mathematics in the University of St. Andrews, has made application to the University Court of St. Andrews for leave to resign his chair on a retiring allowance. As the necessary arrangements will most probably be completed during the present summer vacation, a new appointment will fall to be made before the opening of the session in the United College in November next. The patronage of the chair belongs to the Crown.

TAUNTON COLLEGE SCHOOL.—In reference to our article on Taunton College School, Mr. C. P. Bahin, of Heaton Moor, Stockport, writes that science was taught at that school before Mr. Tuckwell's time. Mr. Bahin forwards us a prospectus of the school for the year 1860, and what position was allotted to science at that time in the school may be inferred from the fact that "Physical Science" comes in as the last subject in the General Department after Fortification, that no mention is made of it in the Classical Department, and that "Monthly Lectures on General and Scientific Subjects are given during the winter season; and in summer, occasional excursions, with a view to the practically illustrating the various branches of Natural History, are taken in one of the weekly half-holidays." This is exactly the state of things we have all along protested against, and which Mr. Tuckwell has managed so successfully to remedy in the case of the Taunton School.

SCIENTIFIC SERIALS

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. x., fasc. ix. and x.—On the equation of the eikosahedron in the resolution of equations of the fifth degree, by M. Klein.—Further notices and observations on the comets of 1877, by M. Schiaparelli.—On the morphological nature of distigma, by M. Maggi.—Case of fracture of the neck and diaphysis of the femur, by M. Scarenzio.—The combustibility of tobaccos, by M. Cantoni.—Stratigraphical observations on the Province of Pavia, by M. Taramelli.—Experimental researches on heterogenesis, by MM. Maggi and Cantoni.—On the relative length of the index and ring finger of the human hand, by M. Mantegazza.

Fasc. xi.—Contributions to the morphology of Amphizonella, by M. Maggi.—On the Arachnida of Greece, by M. Pavesi.—On the tension of induced electricity, by M. Macaluso.—The albuminous matter of urine, by M. Pellogio.—On the relative and specific weight of the cerebellum and the arch of the cranium, by MM. Colombo and Pizzi.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, June 21.—Dr. Gladstone in the chair. The president announced the following grants from the Research Fund of the Society: Dr. Wright, 50*l.*; Mr. Neison, 25*l.*; Mr. C. Williams, 25*l.*; Mr. G. Harrow, 10*l.* The following papers were read: On diamyl, by H. Grimshaw. This substance was prepared by the action of sodium on amyl bromide. It boils at 160°. A chloride and acetate were obtained and investigated. By the action of caustic potash on the acetate, two alcohols were formed boiling at 202° and 212°. On oxidation acids were formed.—On the action at a high temperature of certain volatile metallic chlorides on certain hydrocarbons, by Watson Smith. *a.* The author investigates the action of antimony trichloride, and tin tetrachloride on naphthalin, benzene, and toluene, when these substances are severally passed in the state of vapour through red hot tubes. Benzene and tin tetrachloride gave a very large yield of diphenyl in one distillation. Toluene and antimony trichloride gave oils boiling at 270–320°. Naphthalin and antimony trichloride: 77 grm. of the former yielded 24·2 grm. of yellow crystalline isodinaphthyl; with tin tetrachloride, in addition to a large yield of isodinaphthyl, a reddish oil, and a citron yellow powder were obtained. *b.* Isodinaphthyl sulpho-acids and salts with certain other derivatives; the *α* and *β* sulpho-acids were prepared, also an oxydinapthyl, a nitro-substitution product, and a cyanogen derivative. *c.* A new dinaphthyl. In the purification of crude isodinaphthyl by petroleum spirit, a fine red solution was obtained; from this the author succeeded in separating three substances melting at 75°, 147°, and 253°; the latter is probably Lossen and Otto's polymeric dinaphthyl, the second is an isomeric dinaphthyl already obtained by Lossen, the first is a new isomeric dinaphthyl.—On the action of alkaline oxalates on the earthy carbonates, and of solutions of alkaline carbonates on the earthy oxalates, by Watson Smith. The author having observed that when a solution of ammonium oxalate was brought into contact with chalk or powdered marble, an ammoniacal odour at once became apparent, has carefully measured the extent of this and similar reactions.—Note on thallious platinocyanide, by R. J. Friswell and A. J. Greenaway. In 1871 one of the authors stated that the above substance was colourless, but that a compound of it with thallious carbonate crystallised in dark red needles reflecting a green metallic lustre. Carstangen having confused the two substances and stated that thallious platinocyanide crystallised in blood red needles, the authors have re-investigated the question, and fully confirmed the statements made in 1871.—On crystallised barium silicate, by E. W. Prevost. Pisani having stated that this substance crystallises in barium hydrate reagent bottles, the author has examined similar crystals, and finds that they consist of barium hydrate.—A note on anethol and its homologues, by W. H. Perkin. Methylparoxyphenylacrylic acid, when boiled in a bulb tube, furnishes a distillate, consisting of an oil with the formula $C_9H_{10}O$, which on oxidation yields apparently anisic acid. Methylparoxyphenylcrotonic acid yields anethol, methylparoxyphenylangelic acid yields a similar substance.—Note on persulphocyanic acid, by R. W. Atkinson, Japan. The author discusses the constitution of the above substance, and after investigating various silver and mercury compounds, concludes that the formula proposed by Glutz is probably correct.—On the oxidation products of the aloins, by A Tilden, D Sc. Barbaloin and socaloin when oxidised by potassium dichromate and sulphuric acid, yield a yellowish substance, which the author proposes to call aloxanthin, having the formula $C_{15}H_{10}O_6$. This substance, when treated with fuming nitric acid, yields a yellow nitro-acid, having the properties of aloetic acid.

Geological Society, June 6.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—The Rev. Charles Leach, William May, John W. Myers, and John Fletcher Pagen, were elected fellows of the Society. The following communications were read: On the rank and affinities in the reptilian class of the *Mosasauridæ*, Gervais, by Prof. R. Owen, C.B., F.R.S. The author stated that while the Mosasaurians had been originally referred to the Cetacea by Camper, then to Crocodilia by Faujas de St. Fond, and to the Lacertilia by Cuvier, Prof. Cope had recently thought he recognised in them Ophidian affinities, spoken of them as "sea-serpents," and formed of them an order called Pythonomorpha. He then discussed in detail the various characters presented by the remains of these animals. The distinctive characters did not appear to the author to be sufficient for ordinal rank, and with P. Gervais he regarded the Mosasauridæ as a family of Lacertilia equivalent to the Iguanodontidæ and Megalosauridæ in the order Dinosauria. The order Lacertilia among reptiles, being equivalent to the order Carnivora or Feræ among Mammals, the Mosasaurians would be the equivalents of the seals in the latter.—Note on the occurrence of the remains of *Hyænarctos* in the red crag of Suffolk, by Prof. William Henry Flower, F.R.S. The traces of *Hyænarctos*, described by the author in this paper, consist of a right and a left first upper molar, which were obtained from the Red Crag of Waldringfield, and are so much alike, that but for the former being rather more worn, they might have belonged to the same animal. On comparison these teeth were found to show no appreciable difference from the corresponding teeth of the original specimen of *Hyænarctos sivalensis* from the Sewalik Hills, and hence the author did not venture to regard them as representing a species distinct from the Indian one.—On the remains of *Hypsodon*, *Portheus*, and *Ichthyodectes* from British cretaceous strata, with descriptions of new species, by E. Tulley Newton, F.G.S., of H.M. Geological Survey.—On the precarboniferous rocks of Charnwood Forest, Part I., by the Rev. E. Hill, F.G.S., and the Rev. T. G. Bonney, F.G.S. The authors described a mass of slates, grits, and volcanic breccias, accompanied by some knolls and dykes of syenite, spread over a space of about fifty square miles. They showed that the patches marked on the Survey Map as greenstone of Bardon, Birchwood, and Buck Hill, except a very small portion of the latter, are really altered rock; that the syenite knoll of Bawdon Castle carries a mass of breccia in its centre; and that the area of syenite in Bradgate House Woods must be enlarged. Several writers have noticed that part of the porphyritic region of the north-west corner is altered rock. The authors showed that there is in it no igneous rock at all, and that the same is the case with every one of the smaller patches marked as porphyry on the Survey Map. All are volcanic breccias, ashes, or agglomerates, some of enormous size. The extent to which volcanic materials enter into the rocks of the district is remarkable. The authors endeavoured to correlate the stratified rocks, and adduced evidence to prove that the pebble and ash-beds of Forest Gate, the grit and pebble-beds of the Hanging Rocks, the similar beds in the grounds of Mr. A. Ellis, at Swithland, and the quartzites of Bradgate Stable Quarry, Groby Pool, and Steward's Hay Spring, form one horizon; the slate breccias of Blores Hill, Bradgate, Ulverscroft Mill, Markfield, Bardon, and High Towers, a second; the coarse ash-beds of Benscliff, Chitterman Hill, Timberwood Hill, and the Monastery, a third; and the quartzose rocks of Charley Wood, Charley, the Old Reservoir, and Blackbrook, a fourth. Hence they showed that the beds are considerably dislocated near the syenites, which removes the main objection which previous writers have urged against these being intrusive; and they described the evidence they have obtained as to this being their real nature. This evidence included the description of actual contacts of igneous and sedimentary rock seen at two points in the wood south of Bradgate House, and at a third in Bradgate Park. They propose, in a continuation of the paper, to touch upon the faults, and to describe in greater detail the microscopic structure of the rocks.

Photographic Society, June 12.—J. Glaisher, F.R.S., president, in the chair.—A paper was read by Mr. J. R. Sawyer on the action of light, temperature, and atmosphere upon pigmented sensitive tissues; showing that when once the action of light had been set up upon the sensitive pigmented tissue (used in carbon work) the same action was *continued* in darkness and exclusion from the atmosphere; by this action an increased power and facility of producing large numbers of the same subject was obtained; as also the possibility of producing different tones of coloured pigments, by this important discovery, which the paper described.—A paper by Mr. Herbert B. Berkeley—notes on the theory and practice of emulsion processes—was read, relating to the use of zinc bromide in emulsions.—Capt. Abney, R.E., F.R.S., followed with a note giving the details and results of his own investigations upon the same subject.—Mr. R. W. Thomas, F.C.S., read a note on the nitrate of silver bath.

VIENNA

Imperial Academy of Sciences, March 8.—On some reactions of amido-acids: on the copper salts of leucin, tyrosin, asparagin acid, and glutamin acid, on the dissolving power of amido-acids for cupric oxide in alkaline liquid, by M. Hofmeister.—On a modification in the determination of vapour densities, by MM. Goldschmidt and Ciamician.—Further experiments on galvanic expansion, by M. Exner. The elongations through the galvanic current were not markedly different from those which would arise from heat developed by the current; the differences did not amount to three per cent. of the whole expansion, and were partly positive, partly negative. Hence the author pronounces against a galvanic expansion.—On the teeth-apparatus in frogs and their larvæ, by M. Wajgel.—Description of a steerable flying machine in form of an eagle, by M. Grois.—The fossil bryozoa of the Austro-Hungarian miocene, by M. Manzoni.—On cosmic vulcanism, by M. Tschermak. He thinks the hypothesis most applicable to it is that which explains volcanic phenomena on the earth by gases and vapours, which have been held absorbed in the supposed liquid interior, but developed in the gradual solidification.—On point systems in rational space curves of the fourth order, by M. Weyr.—On polypes and jelly fishes of the Adriatic, by M. Claus.—On *Sagartia troglodytes* Gosse, by M. v. Heider.—On diffusion of gases through clay cells, by M. Puluj. He finds, *inter alia*, that water vapour diffuses more quickly than air (and here contradicts Dufour's statement that dry air diffuses more quickly than moist). Vapours diffuse approximately in inverse ratio of the fourth root of their densities.

PARIS

Academy of Sciences, June 25.—M. Peligot in the chair.—The following papers were read:—On the heat liberated by chemical combinations in the gaseous state; anhydrous acid and water, by M. Berthelot.—On the equivalent of organic compounds, by M. Berthelot.—On a new anthophyllite of Bamle, in Norway, by M. Des Cloizeaux. Anthophyllite (in this specimen) presents new similarities to amphiboles; like them, it may contain a large proportion of aluminium (twelve per cent.) in varieties of homogeneous appearance, and it has a marked tendency to pseudomorphism.—Reply to observations of M. Mouchez (continued), by M. Villarceau.—On an apparatus called a central obturator-inflamer, capable of adaptation to all cartridges, by M. Cosson. The inflamer is a cylindro-conical case forming an air-chamber, in which the priming explodes; the gases are distributed by slits in the top of it. The obturator is a convex sheet iron shield with serrated border, and a central hole for the inflamer. The combination is placed in the socket of the cartridge, and the obturator then flattened, so as to firmly and hermetically close the base. Among other advantages, it is claimed to improve the range, increase the penetration, diminish recoil, and preserve the cartridge-sockets intact.—H. M. Don Pedro d'Alcantara was elected foreign associate in room of the late M. Ehrenberg. He obtained thirty-nine votes, M. Van Beneden one.—On the state of the vines of Mezel, near Clermont Ferrand, according to a report of M. Truchot, by M. Aubergier.—Anthogenesis in subterranean pucerons of the Gramineæ, by M. Lichtenstein.—On a means of avoiding the resonance of the seventh minor harmonic of the fundamental in the series of grave cords of the piano, by M. Dien. The damper (which is the cause) is allowed to act in its ordinary place, but it moves, simultaneously, a lever having at its upper end a second damper, which touches the cord at a quarter of its length, and causes resonance of the double octave, destroying completely that of the defective triple minor seventh.—Historical remarks on the theory of motion of one or several bodies, of constant or variable forms, in an incompressible fluid, &c. (continued), by M. Bjerknes.—New method of elimination of arbitrary functions, by M. Minich.—On a solar spot observed during the month of June, 1877, by M. Tacchini.—On the 3rd a small spot (the only one) appeared in the east, and gradually enlarged to 40s., by the 7th continuing of this size (some small spots which presently appeared with it ever changing) till it was near the border on the 13th. On the 14th, when projection and photography revealed hardly a trace of the group, the spectroscope discovered very lively chromospheric flames; higher up, several oblique fragments, evidently from violent eruption, eruption flames on the right, and lastly, a nebulous chromospheric mass, well illuminated and slightly divergent. There was constant commotion of matter. The case is cited as against M. Janssen's view.—On a glass of phosphate of lime, by M. Sidot. It is perfectly transparent, very refringent, (index 1·525), can be cut like ordinary glass, is dissolved by oxides of cobalt and chromium, is not attacked by cold acids, but is attacked by boiling acids and by potash, is not attacked by hydrofluoric acid.—On the dissociation of carburets by means of palladium wire, and on the relation of these facts with actions of presence or catalytic phenomena, by M. Coquillion. To analyse carburets with palladium wire there must be enough oxygen to work complete combustion of the constituent carbon and hydrogen.—On the determination of potash, by M. Carnot.—On the nickelised iron of Santa Catharina, in Brazil, by M. Guignet. The bed is now exhausted; it is thought to have been a meteorite with weight not under 25,000 kilogs. (the Ovifak block was 20,000 kilogs., and so was that of Durango, Mexico, found in 1805). The last portions had very little nickel.—Description of several minerals, by M. Pisani.—Reply to objections raised by M. Naudin against the project of an interior sea in Algeria, by M. Roudaire.—Investigation of the free acids of gastric juice, by M. Richet. Pure gastric juice contains almost only mineral or similar acids; organic acids increase when, left to itself, it ferments. Aliments increase by 20, 50, and even 70 per cent., the acidity of liquids in the stomach. The mineral acid continues predominant so long as there is no putrefaction.—On lymph as an agent of propagation of vaccinal infection, by M. Raynaud. The observations are somewhat discordant, but the virulence of lymph from a vaccinated region is demonstrated.—On the pebbles of a hill near Vailly, in the department of Aisne, by M. Robert.

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THURSDAY, JULY 12, 1877

THE "INFLEXIBLE"

THE question which has been raised respecting the stability of the *Inflexible*, important as it is with reference to that ship, leads to very much wider and more general considerations. It is already known that the same principle, or want of principle, which has brought doubt upon the one ship appears also in the *Ajax* and the *Agamemnon*, and is to be reproduced in the only ship of much size or importance which the Government purpose commencing during the present year. But even in this succession of large and costly ships we see probably but the beginning of a system which, having thus received countenance and sanction in the highest quarters in this country, may not improbably become extended over the navies of the world. We propose, therefore, to explain to our readers the nature of the question itself and the manner in which it has arisen.

And as an essential preliminary we will first explain what is meant by the terms "stability" and "curve of stability." For the purpose of this elementary explanation it will be sufficient to take the case of a ship floating in still water. In Fig. 1, which represents the transverse section of a ship taken through the centre of gravity, M represents the metacentre, G the centre of gravity, and B the centre of buoyancy, W L being the water-line when the ship is in an upright position. Supposing the ship to be inclined through an angle of θ degrees from that position by an external force, and W_1L_1 to become the new water-line; she will now tend to return to the upright position with a righting force equal to her total weight or displacement, acting with the leverage of G Z, and therefore equal to W × G Z. This is obviously the case, because while the ship is held in the inclined position her weight will be acting downwards through G in the direction of G H, which is perpendicular to the water's surface, W_1 L_1, and her buoyancy, which may be supposed to act collectively through its centre of buoyancy, B_1, will be pushing upwards through the line B_1 M, and therefore the righting effect will be that of the two forces (weight and buoyancy, which are alike) acting at the opposite ends of the lever, G Z, as previously stated. It is equally certain that in all ordinary forms of ship G Z will be changed in length as the ship's angle of inclination is changed, and if we calculate its lengths for a series of angles, and set up the lengths so obtained as ordinates along a base line on which abscissæ are measured off to represent the angles of inclination, we can draw a curve line through the points so obtained, and thus form what is called a "curve of stability." The first instance on record of this being done for an actual ship or design is that given in a paper "On the Stability of Monitors under Canvas," read in 1868 at the Institution of Naval Architects, and published in their *Transactions*, and in several other places. After stating the amount of stability which certain rigged monitors would have under given conditions, and showing that the maximum stability, and even the vanishing stability was reached in them at moderate angles of inclination, Mr. Reed said: "It must be obvious that the danger to be apprehended by these monitors when under canvas is very great; and when we think that they are liable at any moment to be overtaken by sudden gusts of wind, and that if they are heeled over beyond 8 deg. or 10 deg., the further they go the less resistance they offer to being capsised, their unfitness to carry sail must be quite evident."

The curve of stability was next constructed in the case of the *Captain*, immediately before her loss, and from a report by Mr. Barnes, one of the present Admiralty constructors, we take the following:—"We assume that the side plating on the poop and forecastle has been so damaged that the ship may be considered a rigged monitor with a free-board of about 6 ft. At that draught (25 ft. and $\frac{1}{2}$ an inch) with an inclination of 14 deg., the gunwale on the immersed side is level with the water, but the stability of the ship notwithstanding goes on increasing until an inclination of 21 deg. is reached. As Mr. Reed has pointed out in his paper (quoted above) on rigged monitors, with a pressure of canvas which would incline the ship to say 8 deg., the inclination of the ship to the surface of the wave may reach about 34 deg. (in this case) before the ship would upset. As this angle is large we do not consider that even with the sides of the poop and forecastle destroyed, the *Captain* would be unsafe."

The above cases are both those of rigged ships, which the *Inflexible* can scarcely be considered, although it must be acknowledged that, as designed, she carried a considerable spread of canvas on two masts, and the present proposals—which we understand have been made—to diminish the spread of sail at all times, and to do away with it altogether in war time, are no doubt consequences of recent discoveries respecting the stability, or want of stability, of the ship with the unarmoured ends badly injured. After what has passed, however, we must accept the *Inflexible* as a mastless ship in time of war, and therefore a ship which can do with less stability than rigged ships require. In order to illustrate the nature and character of these curves we copy, in Fig. 2, a figure from Mr. Thearle's valuable work on "Theoretical Naval Architecture,"[1] in which he has grouped half-a-dozen curves which may be regarded as types of various kinds of curves of statical stability which occur in practice, viz.:—

A. A lofty-sided troop-ship, carrying sail.
B. Do. Do.
C. A broadside iron-clad frigate, Do.
D. A turret-ship with high freeboard, Do.
E. A low freeboard iron-clad gun-vessel, not carrying sail.
F. A breastwork monitor, Do.

To facilitate comparison, we have added to Fig. 2 dotted lines showing the stability of the *Captain* as ascertained at the Admiralty just before her loss. The curve marked *a* shows the stability when the ship is fully stored and provisioned, and with the proper complement of coals on board with the poop and forecastle water-tight and assisting stability. The curve *b* refers to the ship under the same conditions, except that the poop and forecastle are supposed to be so damaged as not to assist stability. It will be observed that although Mr. Barnes considered the ship safe even when she had only the stability shown by *b*, the ship actually capsised when she had the larger amount shown by curve *a*, the two curves being the same up to about 20 deg. of inclination, but the latter showing much greater stability both in amount and in range after that amount of inclination was passed. Mr. Barnes no doubt expected that the ship would never be pressed under canvas enough to endanger her, but the event showed that in matters of this kind the measure of safety must be ample, and that we must not trust to the chapter of accidents for the security of our men-of-war against capsising.

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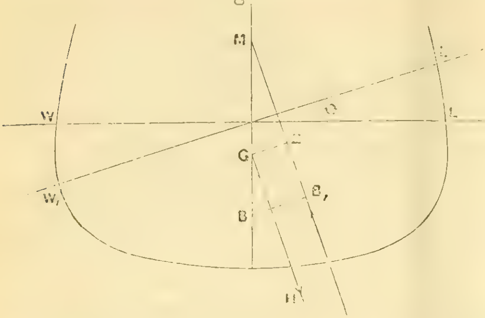
[1] Published by Collins, Sons, and Co., in the "Advanced Science Series."

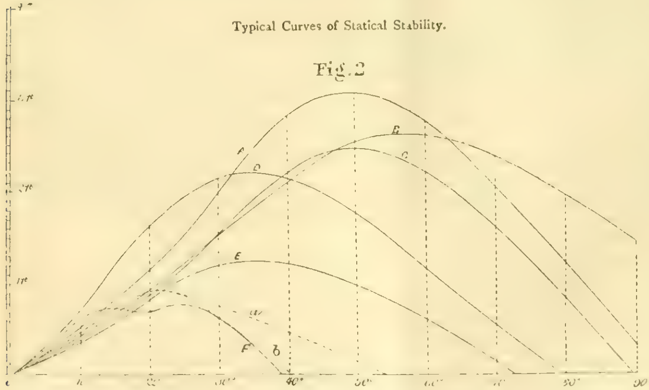
Fig. 1



We are now in a position to explain the case of the *Inflexible* up to the point at present attained in the discussion, but in order to understand it the reader must take clearly into his mind certain differences between her and all previous ships as regards the relations of the armoured and unarmoured parts. In the case of the *Captain* we had a ship with armour rising to a uniform height of six feet above the water from stem to stern, and above this armour at one end a forecastle and at the other end a poop, both of these being of thin iron and unarmoured. It was in view, no doubt, of these unarmoured ends being liable to be so injured in action as to contribute nothing to the ship's stability that the Admiralty officers calculated and stated (as above quoted from Mr. Barnes' report) the ship's stability with the poop and forecastle destroyed. But the reader should carefully observe that as these unarmoured ends were wholly situated in the *Captain* at a height of six feet above the water, their destruction to any extent whatever could not affect the ship's stability at small angles of inclination; and in point of fact by looking at the dotted curves in Fig. 2 we see that the stability is the same whether these ends exist intact or not up to an angle of about 22 deg., for up to that point the two curves are identical. At that angle of inclination the poop and forecastle enter the water, and the curve of stability declines much more rapidly when they are injured than when they are uninjured. In the cases of the *Devastation*, *Thunderer*, and some other ships, there was a different arrangement, and one less favourable to stability, for in them the forecastle (not the poop) was sunk, so to speak, down into the armour, so as to reach to within a foot or two of the water's surface. In such cases, of course, the curve of stability, with the unarmoured ends injured, begins to decline earlier than if the forecastle stood wholly above the armoured side as in the *Captain's* case; but the *Devastation* class are all unmasted ships, and therefore subject to no pressure of canvas, so that much less stability was required in them than in the *Captain* and other rigged ships.

Typical Curves of Statical Stability.

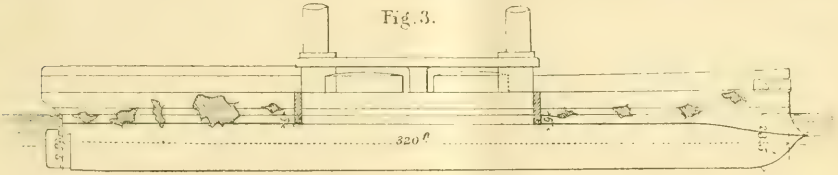
Fig. 2



The *Inflexible* class of vessels differ from all the ships previously named in a very marked manner, for in her the ends are entirely unarmoured (excepting as regards a horizontal deck lying seven feet below water), and while the forecastle, and poop, where it exists, in the other ships named are comparatively short, in her they occupy two-thirds the length of the ship—one-third at each end. The central part of the ship, one-third in length, is alone armoured with side-armour, and therefore in considering the stability during an action, we may suppose that two-thirds of the ship's length, viz., the two wholly unarmoured ends, are destroyed, destroyed that is in the sense in which Mr. Barnes obviously used the phrase in the case of the *Captain*; or, in other words, so broken and damaged by shot or shell as to let the sea flow freely in and out of them, and therefore to possess no stability to help in keeping the ship upright. In Figs. 3 and 4 we have, illustrated, the *Inflexible* injured in this manner. One of the contentions of the Admiralty is that these ends cannot be injured during an action to this extent; but whatever the Admiralty officers may now assert in this respect, it is clear that in the case of the *Captain*, they thought this amount of injury possible, and it is equally clear from the quotations given by Mr. Goschen in Parliament, that they thought the same of the *Inflexible*, when they proposed that she should be built, and thought this, notwithstanding the introduction of certain cork-filled chambers and other sub-divisions upon which they now seem disposed to rely for the ship's safety. We may add that even during the present controversy, Mr. Barnaby has published figures which assume the total annihilation of the ends, and if they can be totally annihilated it is clear they may be so far injured as to lose all buoyancy and stability. We may confidently assume, therefore, that the ends can be so far wounded and damaged as to cease to help the ship's stability, and therefore to leave her wholly dependent upon the citadel for the power of keeping from capsizing. In Fig. 3 we have shown several large injuries, such as we may assume modern shells are fully capable of inflicting, merely to help the reader to get clear ideas on the subject.

The question now at issue really is, therefore, what amount of stability has the ship (by virtue of the citadel) with the ends thus injured? The *Times* and Mr. Reed say that careful calculations which have been made show that she has none, or next to none. Hitherto the Admiralty have refrained from saying how much they claim for her. They say that the *Times* and Mr. Reed are entirely wrong in their calculations, and that the ship really has abundant stability for all purposes of safety, and they appeal to a model which is at the Admiralty to prove this. Let us say at once no model can possibly prove anything of the kind; the model must be weighted and arranged entirely to represent the results of calculations, and it is these results which should be clearly, and fully, and authoritatively stated. The Government have laid certain papers on the table of the House of Commons, but they are not yet published, and until they are in our hands it is impossible to pursue the subject further. We shall hereafter give due consideration to them. All that we can now say is that with the *Captain* case fresh in our memory, in which the Admiralty office dangerously overrated the safety of the ship in this very respect, and, remembering as we do that for a ship to be safe at sea, she should have a very large margin of stability over and above that which mere statical and smooth water conditions point to, we shall not ourselves be satisfied with less than the Committee on Designs laid down, viz., "that the angle of vanishing stability should not be fixed at less than 50 deg." Nor shall we be content with this if this range is obtained only in conjunction with a small amount of stability from point to point. Mr. Reed has pointed out, in his letters to the *Times*, the great danger of considering range only, and has attacked the dictum of the Committee on this ground. Dr. Woolley, one of its scientific members, has replied, admitting the accuracy of Mr. Reed's view, but explaining that the truth he enunciates is so elementary and obvious that the Committee thought it unnecessary to mention it, and would indeed have considered it "impertinent" (in the proper sense of the word) to state it. It is difficult to take this view of the matter, however, when we remember that the highest scientific officer of the Admiralty, in a matter affecting the safety of four of H.M. ships of the largest and newest type, has seized this dictum of the Committee as a sufficient and satisfactory guarantee of their security. We fear we must conclude that the Committee either neglected a very serious element in the calculations, or else greatly overrated the skill and discernment with which their words would be interpreted.

Fig. 3.



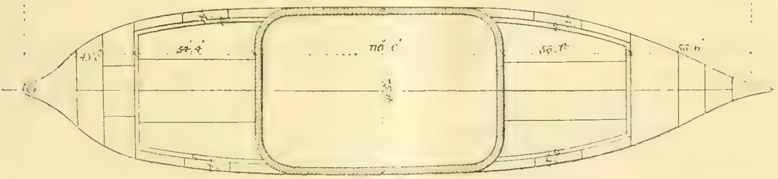


Fig. 4.

THE DEVELOPMENT OF THE OVUM[1]

Bütschli on the Earliest Developmental Processes of the Ovum, and on the Conjugation of Infusoria.

Studien über die ersten Entwicklungsvorgänge der Eizelle, die Zelltheilung und die Conjugation der Infusorien. Von O. Bütschli. (Frankfurt, 1876.)

II.

COMING now to the large and important question of the *Conjugation of Infusoria*, its nature and bearing upon the life-history of the forms, we are bound to state at once our conviction of the inefficiency of the observa-

[1] Continued from p. 180.

tions recorded on account of their discontinuity. Nothing but a close and continuous observation of the same forms extending over an entire life cycle, repeated again and again, can lead to absolute results. Errors fatal to the interests of truth inevitably arise, when minute organic forms are studied, not by continuous watching, but from inferences made from the phenomena manifest at different periods, the intervals between which are blank. Further, whilst the use of reagents on the dead forms taken at various stages is of the utmost value, when they are examined side by side with continuous observation on the living form, these may be not only not instructive but misleading when taken by themselves.

Bütschli's observations are numerous and interesting, but their value will be best estimated, by understanding briefly the nature of the hypothesis they are declared by their author to indicate. Put in its shortest form, it is that conjugation amongst the Infusoria is simply a *rejuvenescence* of the creatures which undergo it, enabling them to become "the stem ancestors of a series of generations" which propagate by fission. As yet the process of rejuvenescence has had, in biology, a limited application, being noticed in the formation of the swarm-spores of Œdogonium and other of the lowliest plants; but its connection with sexual reproduction is not clear, as no union of different elements has been made out, and it is by no means certain that the whole process of reproduction is exhausted by it. When, however, it is combined with conjugation, as in the Bacillariaceæ, it becomes plainer; although, so far as is known at present, it by no means follows that the whole generative process in these forms is known; but it is to the *Auxospores* by which rejuvenescence is secured in these forms that Bütschli appeals for the support of his theory of infusorial conjugation. Pfitzer and Schmitz have made what are at present the most complete observations of the phenomena in question; from which we learn that the customary mode of reproduction is by fission, but at each repetition the individuals dwindle in size, until they can apparently go no farther,[1] then the conjugation of two individuals takes place, the formation of auxospores being the result, that is to say rejuvenated individuals; and from these a new departure of fissiparous generations takes place, well observed by Schmitz in the case of *cocconema cistula*. There is no coalescence; the frustules simply lay themselves parallel to one another, they become surrounded by a common envelope of mucus; the protoplasm of the cells comes into contact, each frustule grows larger and becomes an auxospore. What the influence is which these frustules exert upon each other is wholly unknown; but that it has a real existence is shown in the result; each auxospore forming a stem ancestor of a new series.

This is what Bütschli extends to the infusoria, and contrary to the interpretations of Balbiani, Stein, and others, maintains that the act of conjugation so well known amongst the Paramecia, Vorticellæ, &c., is not a precursor of sexual products, but simply a means by which these forms, exhausted by continued fission, become more highly vitalised and rejuvenated, and again enter upon the process of fissiparous multiplication, which indeed becomes thus their only method of increase.

It should be noted that on the whole the facts adduced by Balbiani and Stein are admitted, but they are submitted to a wholly different interpretation; and it is specially insisted on that the forms that go into the conjugation state are of a minimum size; which fact Balbiani explains as the result of a special development for sexual purposes, but this is disallowed by Bütschli, who insists that it results from exhaustion of vitality at the terminus of a series of fissiparous multiplications. Indeed these weakened and minimised forms unite in conjugation and are neither absorbed into each other nor produce embryos, but increase in size and vitality, separate, and commence again the fission by which alone increase is effected.

The truth of this is insisted on as deriving strong support from some of the very remarkable external changes which the author has seen certain of the Infusoria undergo. In *Euplotes* and *Oxytrichineæ* a great part of the *ciliary system* is said to perish towards the end of conjugation; and afterwards, when separation takes place, to be again renewed. In *Colpidium colpoda* the entire mouth was lost in conjugation, but was renewed again after separation. So in *Bursaria truncatella*, the conjugated animals, it is affirmed, lose entirely the complex apparatus of the peristoma, which by a new growth after conjugation is restored. So also there is declared to be a complete rejuvenescence of the more important internal parts. The "secondary nucleus" in *Stylonichia mytilus*, and in *Blepharisma lateritia* and *Colpidium colpoda* the old nucleus is said to be eliminated and a new one formed. In others, part of the nucleus is thrown off, and part renewed; in others a new nucleus formed and coalesced with the old one. From these and similar observations it is inferred that the "essence of conjugation consists in the rejuvenescence of both the individuals;" and that this is chiefly centred in the "secondary nucleus" which is declared to be of the utmost importance in the life of the creature.

During the process of conjugation, also, the plasma-contents of the individuals have been seen to interchange; this especially in *Oxytrichineæ*, but also in other infusoria.

Against Balbiani's hypothesis—that the nucleus is the ovarium and the nucleolus the testis, containing spermatic elements—Bütschli affirms that in *P. aurelia* and *P. colpoda* the supposed spermatic capsule in some cases wholly disappeared without any following change in the nucleus that could be discovered, and that consequently it did not effect fertilisation. In short, he believes that the observations he has made are quite competent to overturn the sexual hypothesis in these organisms, and to establish that of *rejuvenescence* in its place.

That there is extreme ingenuity in this hypothesis we readily admit; that there is also the utmost conflict of interpretation amongst the best observers of these organisms, we admit with equal readiness. But that the author's observations give *scientific* sanction to his theory on the one hand, or either explain away or harmonise the labours of his predecessors or *collaborateurs* on the other, we are fain to dispute. The exhaustive and continuous method of observation—following a single form through all the phases of its life—has never been thoroughly adopted; and conflict of interpretation inevitably arises. Bütschli has fallen into the same groove, and his results, although valuable and full of suggestion, have no irresistible meaning. They present points of new departure for hypothesis, and nothing more.

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[1] It is impossible not to notice here the extremely interesting and certainly somewhat remarkable paper of Dr. Wallich in the February number of the *Monthly Microscopical Journal* for 1877, "On the Relation between the Development, Reproduction, and Markings of the Diatomaceæ;" for in this paper what is apparently the *auxospore* of Pfitzer and Schmitz is called the *sporangial frustule* But this, instead of having dwindled in size before conjugation appears to have become enormous in proportion, and within this the "new parents of the race arise," and from the conjugation of *these* the new forms spring as daughter frustules.

Nor can we be quite certain, from the evidence afforded, of the correctness of the larger and more important of the facts stated. We want, for example, more than a mere statement that the "ciliary apparatus" and the important organs of the *peristoma* were actually destroyed by conjugation. That they are suppressed—flattened—deranged by prolonged contact, we have observed again and again in several forms, especially *Stylonichia*, *Pustulata*, and *Mytilus;* but they rapidly regained their *normal condition*, and certainly did not *grow afresh* by "rejuvenescence" as in the cases stated by our author. And this is certainly of moment. In some important sense also this will apply to the nucleus and nucleolus themselves. Doubtless the investigations of Bütschli on the metamorphoses of these bodies, especially the latter, in such forms as *P. bursaria*, *aurelia*, *putrinum*, and others have a large importance; and if they should be confirmed by *continuous observation* on the *living form*, controlled by the evidence of preparations, made at short intervals, under the influence of acetic and osmic acids, and other reagents, not only will Balbiani's hypothesis become modified, but a sequence will be given to the successive stages, often now wanting, in the observations of Bütschli himself. It is impossible not to be struck, for example, with the minuteness of his observations, made on the nucleolus changes in *P. bursaria;* but they are utterly incompetent to accomplish his own purpose and establish his own idea. He declares that both Balbiani and Stein utterly mistook the destiny of the nucleus and nucleolus; and quite repudiates the changes said to come upon the nucleus during conjugation. But to establish his own hypothesis the whole process of morphological change in the nucleus at least should have been followed, and not once but many times. Yet the very first complete change effected in this organ could not be explained; and after following it into fission as the result of conjugation, he observed four "nucleolus capsules" as the issue, in each paramæcium. Two of these became light and clear; the other two diminished in size, and became fibrous, but on the second day they lost their fibres and became homogeneous and dark; and on the third day—*vanished!* that is to say, by the method pursued by the observer, they were lost, and "no trace of them was to be found." From this Bütschli concludes that they were "cast out," and no further concern in relation to them is evinced! Yet it must be remembered that Balbiani describes a similar condition of the same forms, and considers the granules germs or ova. To deal thus lightly with the ejection of apparently organised bodies in a set of observations designed to *prove* that what have been considered ovarian, or at least sexual, products, was erroneous, is certainly remarkable. Clearly no result can be arrived at until the manner of the vanishing of these bodies be understood; and if they were ejected, until their future destiny became known. This is all the more imperative from the fact that *after* the ejection of the "bodies," the paramæcium resumes its normal condition in size and appearance, although the method by which this conclusion is reached is by saltative inferences, and not by continuous proofs.

Again,—in *B. bursaria* and *aurelia*, two "light bodies"—definite products of the nucleolus—are repeatedly seen in successive stages after conjugation, but having been followed to a certain point we are told that "the further destiny of these two light bodies escaped me!" and yet it is assumed that the life history of the creatures is known.

Again,—in these same forms the *nucleus* broke up into a hundred spherules; and yet our author frankly declares "I am not quite certain of the destiny of the . . . fragments of the old nucleus!" This is the more important since Schaafhausen affirms that he has seen *P. aurelia* lay or deposit ova; "the organisms crammed full of egg-spheres, surrounded with clear fluid, extrudes in an hour several times one such egg."

Again,—in *Colpidium colpoda*, after conjugation, two small light spheres appear, these the author "thinks most probably" grow out of the nucleus capsules, while the *nucleus* itself is *cast out;* Bütschli followed it "for some time" and then it was lost, so he does not know its final destiny! Of what service can all the subsequent transformations of the organism itself be when this ejected organism is assumed to mean nothing? In *Blepharisma laterita* a number of "nucleolus-like bodies" were found by "squeezing and acetic acid," but their destiny was never found; while on the third day after conjugation "the *nucleus* which had been present up to this time was not to be found," and so the author meets the emergency by supposing that it was "cast out," and of course had no meaning in the history of the organism. So also in *Chilodon cuculus*, we are told that the "destiny of the original nucleus remains undetermined." In the conjugation phenomena of *Stylonichi mytilus* there is an equal or even more grave defect.

In precisely the same way in the attempt made by Bütschli to establish the position he occupies that the embryonal regions of Balbiani and others as existing in these lowly forms are to be entirely explained by the presence of swarm spores of internal parasites, there is the same want of perfect sequence, and the unscientific "no doubt" which is made to supply the place of facts.

But our space is exhausted. We have not referred to the above defects with any attempt to depreciate a valuable book. It is because it is strong enough in important facts to be a help in the unravelling of biological difficulties that we have not hesitated to point out the difference between the *theories* and the *facts* which it contains. To have attempted exhaustive criticism of such a work would have involved four or five times the space occupied by this article; but after a careful perusal and reperusal of its contents, we are obliged to admit the ingenuity of the author both in the work he has done and the method he has employed for interpreting it. But it is to the former that we attach by far the most importance; for whilst there are many missing links in evidence which make conclusions from the whole unwise, there are facts given us which must help future observers and land us nearer to the desired truth.

It may be finally observed—1. That if the theory of rejuvenescence, as put and insisted on by Bütschli, be established for any one form, conjugation should have no other meaning or place in any part of its history than rejuvenescence can explain. Now *Stylonichia pustulata* is amongst the forms the author has seen to conjugate, and as he believes, as a consequence, to become simply more vital and larger for renewed fissipartition. But Engelmann is undoubtedly right in his affirmation, that there is a conjugate state in which these organisms do *not* again separate, but the pair simply fuse together. One of the writers of this paper has observed it repeatedly under conditions which render error impossible; this is not the place to consider to what this fusion leads, but it is important as a fact, inasmuch as it throws doubt upon the *completeness* of the theory of rejuvenescence, even supposing the facts given us by Bütschli led without exception up to it. Bütschli even admits that this process of fusion may happen, but he simply dismisses it as a "very unusual one"—surely all the more important on this account, inasmuch as we know that in more highly organised creatures not only a long time, but generations may intervene between distinct acts of fertilisation.

2. It does not follow that if rejuvenescence be rejected to the extent and with the meaning Bütschli gives it, that it must be rejected altogether. He gives us many remarkable facts that deserve further experiment and research; and it may result, that what he calls rejuvenescence, is one of the many modes by which rapidity of fissiparous multiplication is in some organisms aided, and the necessity for the true act of fertilisation is made less frequent; and

3. It is clear that there are points in the theory of Balbiani which the facts given by Bütschli overturn; while there are others that certainly remain unshaken, if they be not strengthened. But it is needful to remember that if the facts given by Bütschli wholly invalidated the interpretations of Balbiani the theory advanced by Bütschli by no means follows as a consequence. In the present state of this inquiry we must seek facts industriously, and with persistent honesty, and be assured that their accumulation will lead to important issues; but we shall do well to place theory, however fascinating, in an extremely subordinate place.

W. H. DALLINGER
J. DRYSDALE

VON RICHTHOFEN'S "CHINA"

China. Ergebnisse eigener Reisen, und darauf gegründeter Studien, Von Ferdinand Freiherrn von Richthofen. Band I. (Berlin: D. Reimer, 1877.)

WE are glad to welcome the appearance of the first volume of this long-promised work from the pen of the well-known geologist and geographer, Baron v. Richthofen. We content ourselves at present with a general account of the work, hoping in an early number to be able to examine it in detail. The author has enjoyed rare facilities for the accumulation of material, and has improved them so thoroughly that the published results of his researches will assume a leading position among the late additions to scientific literature. In 1860 he accompanied Count Eulenberg on his mission to China and Japan for the purpose of closing commercial treaties between these lands and the German states. On the return of the expedition Baron v. Richthofen lingered behind, attracted by the many unsolved problems of the Celestial Empire. Up to 1872 he devoted himself to a systematic, thorough investigation of the geography and geology of China, traversing in the course of seven different journeys the whole eastern part of the empire from Canton to Corea, and penetrating westward to the sources of the Yang-tze-Kiang and the frontiers of Thibet. The essential aims of the traveller were to place on a scientific basis the geography of the land, determining the hypsometric relations, and the laws governing the conformation of the mountain-chains, to examine the general geological structure, especially in its relations to the great basins of Central Asia, and to study the laws of climatic changes. Other scientific questions received a minor consideration, and the intellectual life of the people was left entirely out of view. The present volume forms little more than an introduction to the elaboration of the immense number of observations made during the long series of years, which will form the body of the work. It is mainly occupied with an extensive and complete description of the growth of our knowledge with regard to China, forming a valuable index to the literature on this country. No small amount of space is devoted to the book, "Yü-Kung," or imperial geography, forming the sixth in the series of historical works attributed to Confucius, and covering the period 2357–720 B.C. The remaining portion of the volume is occupied with the geographical relations of China to Central Asia, and contains a most important study of the loess regions of Northern China. They are not only considered in their relations to the saline steppes of Central Asia, but are compared with all the great loess formations known, and supply the basis for an interesting theory with regard to the formation in the one case of fertile valleys, as those of the Nile and Mississippi, and in the other of sandy wastes. Scarcely less valuable is the clear and distinct picture afforded of the whole mountain system of this portion of Asia. The author finds the laws governing the conformations so simple, that less time was required to determine the system than would have been necessary for a tenth of the area in Europe. In a closing chapter on the problems of modern scientific geography, the author sharply defines the province of his science, drawing clearly the limits between it and political geography, ethnography, and kindred sciences. The method to be used in the solution of these problems he defines as "the uninterrupted consideration of the causal, mutual relations between the earth's surface from its various points of view, terrestrial physics, and the atmosphere on the one side, and between these elements and the organic world in its broadest sense on the other side." Of the three volumes yet to appear, one will be devoted to palæontology, in which the author will be assisted by Dr. Kayser, Dr. Schwager, Prof. Schenk, and other able geologists The remaining two will contain the author's extended researches into the coal-fields of China, regarded by him as more valuable than the deposits in the United States of America—the geological structure of the land, the climatic phenomena, the population as affected by these two agencies, the river system, means of transport by land and water, chief productions, mercantile possibilities, &c. A generous grant from the Emperor of Germany has permitted the publication of the work in a most sumptuous style, and the introduction of numerous carefully executed maps and illustrations wherever opportunity is offered by the text.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Soldiers' Rations

In your issue for June 28 (p. 158) Mr. H. Baden Pritchard states in his article on "Soldiers' Rations," "And yet, as we have said, with this apparently liberal feeding, our men do not receive so much actual nourishment, or nitrogenous matter, as the German soldier."

My calculations, based on Prof. Parkes' table of soldiers' rations, and Prof. Frankland's experiments on food and urea, give the following values of the several soldiers' rations:—

	Foot tons.
1. English Military Prison	4,509
2. English Soldier (Home)	3,964
3. Prussian ,, (War)	3,812
4. French ,, (Crimea)	3,683
5. French ,, (Home)	3,580
6. French ,, (War)	3,538
7. Austrian ,, (Home)	3,242
8. Prussian ,, (on the march)	3,223
Mean	3,694

As the average daily external work of a man is 353·75[1] foot tons, the efficiency of man regarded as a heat engine is 9·6 per cent. of the internal work.

An efficiency of 8·2 per cent. can be obtained by engines working at 40 lbs. pressure and steam cut off at half stroke; so that man regarded as a machine, does not occupy a very high position. The explanation of this is, that man is not a *machine*; he is a *machine maker*. The mechanism of a cat or beetle is vastly higher than that of man, and yet they are immeasurably his inferiors.

Saml. Haughton

Trinity College, Dublin, July 7

Printing and Calico Printing

In your article on the Caxton Exhibition last week, there is the remark that the beauty of execution in the specimens preserved to us of the work of the earliest known printers suggests a doubt whether the date of the actual invention must not be pushed back farther than the accepted one. But does that follow? Is not the beauty of the first printing simply the beauty of the wood engraving of the time? an art which had attained an exquisite perfection before its application to movable type.

That there should be doubt and obscurity as to the date, name, and claim of the first inventor can surprise no one who will ask himself who, for instance, was the inventor of our present mode of calico printing by roller; and, if he cannot answer, shall turn, as he has every right to do, to the current issue, ninth edition, of the "Encyclopædia Britannica" for the satisfaction of his doubt. He will find there, in that long, elaborate, and amply-illustrated article, not the bare mention of the name, even (unless I have strangely missed what I expressly looked for) of the otherwise remarkable man who conceived the idea, mechanically wrought it out for his own immediate purpose, and himself elaborated its application to the printing of calicoes—revolutionising that important branch of our industry—all well within the lifetime of men not half a century old among us! The Rev. Isaac Taylor—turning a moment from his own researches into the Etruscan mystery—should be able to tell us in what precise year it was, after 1840, that his father, Isaac Taylor, the author of the "Natural History of Enthusiasm," and a long series of subsequent works, sufficient alone for a reputation of a high and lasting order—a teacher of teachers, the depth and extent of whose influence and the fulness of whose intellectual stature have not yet been adequately recognised—engraved on the roller illustrations for his new translation of Josephus, undertaken in connection with Dr. Traill. The death of his fellow-worker cut short that enterprise, but a portion of the work appeared; and I myself, as a boy, was often in the little private workshop at Stanford Rives while this idea was struggling on the turning-lathe, through the patient genius of its author, for mechanical existence. In 1855 or 1856 I found him superintending its actual application to the printing of calicoes at Manchester. The discovery received the immediate and inevitable compliment of piracy, and brought to him and his loss instead of gain. But that within five-and-twenty years his very *name* should seem to have wholly dropped away from what was undoubtedly his own unaided invention, and one withal of so much national importance, and in an age of lime-light publicity like ours, is almost a curiosity of injustice, and throws, as I have said, a flood of light on a crowd of similar miscarriages in the indifferent past. As a hundred years hence this also may be beyond remedy, kindly assist me to arrest a moment the remorseless tooth of All-Father Time by the insertion of this contemporary note.

Henry Cecil

Breigner, Bournemouth, July 9

Stamping out Noxious Insect Life

The subject of insect and germ life in its relation to putrefaction and infectious disease is now assuming such importance from the investigations and demonstrations of Dr. Tyndall, Mr. Murray, and other scientific inquirers, that I think you may consider the following curious facts not unworthy of space in your journal.

I observe in a report of Dr. Tyndall's lecture on Germs, in Nature, he refers particularly to the varying tenacity of life which germs under certain conditions exhibit, and which he refers to the period of incubation or stage of development up to the state of emergence as complete organisms, when they are readily destroyed. He says: "We now turn to another aspect of the question; following the plain indications of the germ theory of putrefaction, we sterilise in five minutes the very infusions which, a moment ago, were described as resisting five hours' boiling. The germs are indurated and resistant, the adult organisms which spring from them are plastic and sensitive in the extreme. The gravest error ever committed by biological writers on this question consists in the confounding of the germ and its offspring. The active bacteria developed from those obstinate germs are destroyed at a temperature of 140° Fahr. Let us reflect upon these facts. For all known germs there exists a period of incubation, during which they prepare themselves for emergence as the finished organisms, which have been proved so sensitive to heat. If, during this period, and well within it, the infusion be boiled for the fraction of a minute, even before the boiling point is reached at all, the softened germs which are then approaching their phase of final development will be destroyed. Repeating the process of heating every ten or twelve hours, each successive heating will destroy the germs then softened, until after a sufficient number of heatings the last living germ will disappear. If properly followed out the method of sterilisation here described is infallible; a temperature, moreover, far below the boiling-point suffices for sterilisation."

Now as the laws of nature apply to all magnitudes alike, whether it be a grain of sand or the planet Jupiter, to the various stages of incubation of the germs of bacteria or of noxious insect life, I think I may claim some credit for having *stumbled* upon, and for having applied on a practical and large scale, a system for eradicating insect life in animals based on this law of varying tenacity of life in germs and insects. More than two years ago I advocated this system, and in September last issued a circular, in which I stated that "a short time after clip-day I dipped, by immersion, the young lambs, and I repeated the same before harvest; at the same time I made a long narrow pen alongside the stackyard fencing, into which I crammed all my old sheep as close together as possible. I then, with an ordinary watering-pan, watered them all over with diluted fluid; the latter operation was completed in half an hour, and the cost in material was less than one halfpenny per head, the proportions in both cases being 1 to 100. Now for results! I lately minutely examined the whole of my sheep, for the purpose of deciding if it was necessary to give them a final dressing before October, and I can now frankly, and without hesitation, state that in the whole flock, old and young, I could not find a single living insect, or the germ of one."

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[1] "Animal Mechanics," p. 62.

In the month of March last, agreeably to my promise, I issued a faithful report of the results of this system of stamping out insect life. The sheep cleansed on this principle were absolutely clear of insect pests, whilst others not so treated were never free. I shall send with pleasure this report to any of your readers who may desire to have it.

I believe I may say there is exact analogy between this system for exterminating insect life in animals and that adopted by Dr. Tyndall to show that the earliest eggs or germs of bacteria are extremely obstinate to kill, whilst the more fully developed are destroyed without difficulty, clearly showing that more than one treatment is necessary for the complete destruction of germ life as well as for a higher form of insect life, and that the same law applies to both alike.

I am fully convinced of the possibility of stamping out noxious insects that affect sheep and other animals, and sincerely hope Mr. Murray's suggestion at the Society of Arts of united action to effect this purpose under the direction of science and experience may be acted upon with little delay. W. LITTLE

The Hall, Heckington, Lincolnshire

Complementary Colours

IN NATURE (vol. xvi. p. 150) you give a most interesting, though very brief, account of Prof. Rood's researches on colour, the result of which you sum up in these words :—

"The mixture with white is the same as if the colours were moved towards the violet end of the spectrum."

I know of Prof. Rood's results only from your abstract, but your summary is not a perfectly accurate account of the facts which you state immediately before, unless the expression "violet end" is to be used in a new sense, or, what would be better, replaced by the expression, "violet *pole*."

The following will be found a correct summary of Prof. Rood's results :—

Let the colours of the spectrum be arranged, not in a line but in a circle, and the gap between red and violet be filled up with purple. Each colour will then be opposite to its complementary colour. Greenish yellow and violet, which are mutually complementary, are the opposite poles, and the succession will be as follows :—

Greenish yellow.

Yellow.	Yellowish green.
Orange.	Green.
Vermilion.	Cyanogen blue.
Purple.	Cobalt blue.
	Ultramarine.

Violet.

The addition of violet to any one of the colours except those at the two poles will bring that colour nearer to the violet pole ; the same addition to either of the polar colours will leave them unchanged as to their position in the circle. *The addition of white will have the same effect.*

As regards the effect of the addition of violet, this is what we might expect. Violet added to violet will only make violet. Violet added in small quantities to greenish yellow, which is its complementary, will only make it whiter without changing the colour. Violet added to any other colour will bring it nearer to violet. We might select any pair of complementaries as poles, and obtain a parallel result. But what is new, and if confirmed, most important, is that white has the same effect as violet. I can suggest no explanation of this.

For the fact that every colour in the spectrum has its complementary, see Prof. Grassmann in the *Philosophical Magazine* of April, 1854. His paper appears to be less known than it deserves. The pairs of complementaries according to him are as follows :—

Red.	Bluish green.
Orange.	Azure.
Yellow.	Indigo.
Yellowish green.	Violet.
Green.	Purple.

Most authorities say that purple is not to be found in the spectrum, but Grassman says that under favourable conditions of light it may be. I think that in any observations on the subject where great accuracy is desired, the use of sunlight is to some extent misleading, and that the true white is that of an incandescent solid or liquid at a very high temperature—that is to say the electric light. The sun's light is such a light where it leaves the body of the sun, but part of its rays are absorbed in the sun's atmosphere, and the blue in greater proportion than the red and yellow ; and a further loss of blue rays takes place in the earth's atmosphere by scattering, forming the blue of the sky. The blue light of the sky is taken out of the white light of the sun. For these two reasons the sun's light at the surface of the earth is not truly white but yellowish.

I can scarcely doubt that when the spectrum of the electric light is carefully examined, it will be found to contain purple ; and also that some simple mathematical relation will be discovered between the wave-lengths of every colour and its complementary. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, June 24

Phyllotaxis

THE theory which regards the alternate arrangement of leaves as the normal mode receives some support from the arrangement of the inflorescence of opposite leaved plants. In *Lysimachia nemorum* the leaves are opposite, inflorescence indefinite, solitary, and axillary, but it will be observed that the flowers springing from the axils of opposite leaves are never both equally developed at the same time, one will be fully expanded while the other is yet in bud, or one will be found in seed and the other in flower ; it will be further observed that the oldest or most fully developed flower appears alternately on opposite sides of the stem ; if all the leaves on this plant were separated by internodes, the arrangement would be *tetrastichous*, but owing to the suppression of the internodes between the first and second and the third and fourth leaves, the arrangement becomes opposite. The oldest of the two opposite flowers of each pair of leaves will be found to spring from the axil of the first, third, and fifth leaf, and plants with this alternate disposition of flowers may sometimes be met with ; but usually a flower originates in the axil of each leaf, and then the youngest or latest flowers spring from the second and fourth leaves of the verticil ; these latter may be looked upon as originating from arrested branches. This view is supported by the fact that plants may be sometimes found which, in place of producing the late flowers in the axils of the third and fourth leaves, produce branches from these points instead. In *Caryophyllaceæ* the opposite sides of the cymose inflorescence never exhibit an equal amount of development at the same time, proving that one of the sides is older than the other, although, owing to non-development of an internode, it is at the same level. A similar arrangement occurs in *Labiatæ* plants, but owing to the crowded inflorescence, it is not so evident, but it is very marked where branches spring from opposite leaves ; one is generally two or three times as long as the other, and by tracing the arrangement of these long branches along the stem, the normal alternate arrangement may be determined. In *Scrophulariaceæ*, where both opposite and alternate leaves are met with, all the above-mentioned modifications may be seen. *Veronica chamædrys* has opposite leaves, and when the axillary racemes are opposite, one is invariably more developed than the other ; this can be best seen by examining the inflorescence in the young state, as the dissimilarity in size disappears in the pairs of old racemes owing to the younger of the two continuing to grow until it has acquired the size of the other ; sometimes this plant may be met with bearing in the axil of one of its opposite leaves a branch, and a raceme of flowers in the other, and in such instances the branches and flowers are produced on alternate sides ; in *V. officinalis* this is the usual arrangement. The suppression of the alternate nodes of an alternate-leaved plant with axillary inflorescence would produce the arrangement seen in *Lysimachia*, inasmuch as it would bring together flowers of different ages and in different stages of expansion ; but in this instance all the flowers would belong to the same generation or be the product of the same stem, whereas in *Lysimachia* the earliest developed of each pair of opposite flowers alone belong to the stem, while the later flower of each pair belong to another generation, and spring from a branch originating in the axil of the leaf opposite (owing to non-development of an internode) to the early flower ; the branch, however, is generally arrested, and the flower alone appears, although sometimes the branch is more or less developed.

G. E. MASSEE

GEOGRAPHICAL WORK IN RUSSIA DURING 1876[1]

THE most important journeys by Russian geographers during 1876 were those of MM. Prshevalsky and Potanin in Central Asia, of Dr. Mikluho-Maclay in Polynesia, and the meteorological journey of M. Wojeikoff round the world. We have from time to time given notes of the progress made by M. Prshevalsky, and of the journeys of Mikluho-Maclay.

M. Potanin left Bulun Tokhoi (NATURE, vol. xv., p. 461) on August 20, and after having followed the eastern shore of the lake Ulungur, and crossed the deep and rapid Black Irtysh at Durbeldjin, he reached the river Kran, at Fulta, close by the Lamaïte convent, Shara Suma. The fertile valley of the Kran is the storehouse for the Southern Altai region; the Kirghises come here to purchase grain from the eastern slopes of the Altai and from the valley of Kobdo. The crossing of the Altai by the Djamaty pass, at the sources of the Black Irtysh, having been reported as very difficult, M. Potanin crossed the ridge by a more southern pass, Urmogaity, at the sources of the Kran river (9,000 feet above the sea), and entered on a wide hilly plateau covered with numerous lakes, and gently sloping to the east by a series of terraces, divided by border ridges. The easternmost of these ridges runs north and south, reaches 10,000 feet at the Terekty-asoo pass, and separates the high terrace of the Deloon river from the low tract on which the town Kobdo is built. This place was reached by M. Potanin on October 16. Rich collections of plants, insects, and birds were made during the journey, as well as a survey and a geological sketch of the route, together with determinations of latitudes and barometrical measurements of heights.

A most important work accomplished by the Russian Geographical Society during 1875 and 1876 is the geometrical levelling made along the Siberian highway, from Ekaterinburg to Irkutsk, on a distance of 2,236 miles. All meteorologists are well aware what a gap in our knowledge as to the distribution of pressure of the air upon the surfaces of large continents, arises from a want of geodetically-measured heights of meteorological stations. All attempts to trace isobars upon the Asiatic continent (one of which was made in the standard work of Mr. Buchan) have failed until now, the heights of the meteorological stations at Omsk, Tonesk, Krasnoyarsk, Irkutsk, and Nerchinsk having only now been directly measured. The Geographical Society has undertaken a geometrical levelling along the whole line, Ekaterinburg to Irkutsk, which levelling will afterwards be continued to Nerchinsk and Tashkend. This difficult enterprise, carried on with all possible accuracy, is now completed with full success, and the superintendent of the levelling, M. Moshkoff, is now busily engaged in computing the definitive results.

A yet more important undertaking, accomplished by the Russian geodesists, Col. Sharnhorst and Capt. Kulberg, during the years 1873–1876, is the precise determination of longitudes, by means of telegraphic signals, carried out along an arc of 103°, between Moscow and Vladivostok, on the Pacific. But this work is so important that we hope to be able to give a special report upon it.

We may also mention the work of Capt. Onatsévich on the shores of Russian Manchooria (NATURE, vol. xv. p. 417), and the important cartographical work of M. Sidensner between the Obi and Jenissei rivers.

Most valuable work was done also during 1876 by the Siberian branch of the Geographical Society. The measurements of depth of the Baikal were continued by MM. Dybovsky and Godlevsky, and showed that the greatest depth of the lake is to be found in its southern part and close to its north-western shore. M. Grebnitsky explored the region of the Southern Usuri and returned with valuable geological and botanical collections. M. Chersky explored the valley of the Irkut river and arrived at very important results, the chief of which are:—(1) that this valley is geologically a very old westerly extension of the Baikal trough; (2) that it contains immense glacial deposits; and (3) that the outflows of basaltic lava in the valley are, with one exception, pre-glacial. We notice also the entomological excursions and the exploration of the Kasbek (Devdorak) glacier, made by members of the Caucasian branch of the Society.

Besides these explorations, the Society has also issued some valuable publications. The most important of them are—(1) the fourth volume of supplements to Ritter's "Asia," being a description, by MM. Semenof and Potanin, of the Altai and Sayan highlands, according to works which appeared from 1836 to 1872; the names of the two authors sufficiently recommend the work; and (2) the second volume of M. Prshevalsky's "Travels in Mongolia," which contains—the Climatology and Ornithology, by the traveller himself; the Herpetology, by Prof. Strauch; and the Ichthyology, by Prof. Kessler. The seventh volume of the *Memoirs* of the Society contains the first part of the work of Prince Kropotkin, "On the Glacial Period in Finland and on the Bases of the Glacial Theory," with numerous maps and engravings. The *Isvestia* (Bulletin) of the Society contains, besides valuable small contributions, two very valuable maps of the Hissar and Koolab beckdoms, by M. Mayeff, and of the Lower Tunguska River, by M. Chekanofsky.

The scientific results of the expedition made to the Amu-daria in 1875 and 1876, will appear very soon. They contain the astronomical, magnetical, and meteorological observations made by M. Dorandt, and a thorough and elaborate hydrographical description of the Amu-daria, by M. Zuloff, with the collaboration of Col. Makshéeff. The first of these works is already printing, and contains abundance of most valuable meteorological data (pressure, temperature of air and of the soil, evaporation, level of water, variations of magnetical elements, &c.). The Meteorological Committee of the Society is engaged in preparing complete tables of the amount of snow and rain during 1872-1876, measured at the numerous stations organised by the Society. Finally, we can only mention some of the various works issued by the Society in the departments of Ethnography, Statistics, and Historical Geography; as, for instance, those on the trade in grain in Western Russia, by M. Rayevsky; the ethnographical description, with maps of South-Western Russia, by M. Gildebrand; the text to the ethnographical map of Russia, published by M. Rittich, being now at press; and many other valuable works of less importance.

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[1] "Report of the Russian Geographical Society for 1876," by the secretary, V. I. Sreznevsky.

A POCKET HAMMOCK

IN these days, when exploring tours and extended scientific excursions are so universal, it is a great advantage to be able to take up one's bed and walk, to be in short entirely independent of sleeping accommodation. Even in our own country it is often an advantage to the working geologist, or botanist, or zoologist, to be independent in this respect, and while it is sometimes no great hardship to make one's bed on the heather or grass under the lea of a broom-bush or dyke, still it is seldom advisable to do so if it can be avoided. Many of our readers will therefore be glad to know that Seydel and Co. of Birmingham have devised a handy hammock, which bears the name of the "Ashantee Hammock," from its having been found of great service during the Ashantee campaign, Sir Garnett Wolseley testifying strongly to its manifold utility. It is made of light but strong netting, and can be so folded up as to be slung over the shoulder like a bag, or even carried in a fair-sized coat-pocket. From the arrangement of the ropes, hooks, and screws, it can be used under almost any circumstances, and, as we ourselves can testify, forms a thoroughly comfortable and secure bed or lounge. Mr. Stanley, we believe, was so favourably impressed with the hammock, that he has taken a supply with him in his present exploration; and for explorers in tropical countries, we should think it would prove useful in many ways, as it can not only be used as a bed, but, mounted on a pole, as a travelling litter or palanquin. For those of our readers engaged in explorations of any kind, geological, geographical, botanical, zoological, or even in doing an ordinary tour, in remote districts, we believe the hammock would be found of real service, as it would make them quite independent of sleeping accommodation, and would not increase the weight of their *impedimenta* by very many ounces. An idea of its construction and its adaptability to almost any circumstances may be obtained from the illustrations we give. We can honestly recommend the hammock as likely to answer all the purposes for which it has been designed.

ASHANTEE HAMMOCK ON SLINGING APPARATUS.

THE SANITARY INSTITUTE

THE lecture by Dr. Richardson, published in our issue of last week, has called public attention to the Sanitary Institute of Great Britain, before which the lecture was delivered. The Institute was founded in July, 1876, at a public meeting held at St. James's Hall, and presided over by his Grace the Duke of Northumberland. The Institute has for its work a wide range of subjects. It has sprung, we may say, out of the necessities of the time, and in the first instance may be considered as a nucleus round which will cluster the many men of science who are now employed in carrying out the executive sanitary or health work of the kingdom. The various medical officers of health, the certifying surgeons under the Factory Acts, the engineers and sanitary surveyors of different localities, the mayors of municipalities, and the chairmen and presidents of local boards, all of these must needs take an interest in and in time form the body corporate of an institution framed for the purpose of becoming as it were a voluntary health parliament. In addition to these sections of the Institute there are many other sections of the community which will, we should think, earnestly join in the work. For reasons plainly stated by Dr. Richardson ladies are invited to take part in the proceedings and to help forward sanitary progress. We feel sure there will also be a large class of active men unconnected professionally with sanitary work who will be ambitious to take a part in the great practical scientific labour of the time, the only labour we may say in which science lends herself immediately to the aid and comfort of domestic life and felicity.

The detailed work of the Sanitary Institute has been in some measure projected by its founders; but it is more than probable that in the course of its natural development it will grow into something different from that which is now supposed. At the same time we are bound to say that the plan is sufficiently simple and practical to warrant the expectations of those who have mapped it out. The objects we have seen proposed are all directed to some useful and desirable end. To obtain a registration of the diseases of the kingdom; to establish communications with medical officers of health; to form local branches of the Institute throughout the kingdom; to examine and grant certificates of qualification to local surveyors and inspectors of nuisances, and to form a register of such certificated officers; to investigate the chemical aspects of the sewage question; to establish a sanitary exhibition, and to form a library of books on health subjects;—these objects, some of which must needs become a part of every sanitary organisation, are sufficiently comprehensive to cover any amount of work, and to tax any amount of industry that may be found in the best organised public body. So far the prospects of the Institute are brought beyond what is common to such undertakings in their earliest days. Members are daily being added, and an effective Council has been elected. Already one of the provincial towns, Scarborough, has invited the Institute to hold its first provincial Congress there, and in France a kindred society has been formed in sequence, and, it may be said without offence, in imitation of the one already founded in London. The visit of Dr. De Pietra Santa, of Marié Davy, and other savants from Paris to the meeting on Thursday last, is a significant sign of the good feeling with which the two rival societies have commenced their labours.

For our parts we welcome heartily both Institutes, and shall enjoy the privilege of watching their onward progress and recording their success.

ON THE SOURCE OF THE CARBON OF PLANTS

NEARLY half the dry substance of plants is carbon; and it is conclusively established that they derive, at any rate, the greater part of it, directly from the carbon-dioxide of the atmosphere, which the chlorophyll cells have the power of decomposing in sunlight, at the same time evolving oxygen. But this function of vegetation, which is so essential a complement to the processes of animal life, gives rise to many problems hitherto unsolved; and an important one is whether or not plants avail themselves of other obviously possible sources of carbon than that existing in such very small proportion, although in large actual amount, in the ambiant air.

Our knowledge bearing upon the subject as it exists in the present day, is the resultant of careful investigations by many observers. In the last century Bonnet discovered the gaseous exhalation; Priestley that the gas is oxygen; Ingenhouz that the oxygen is only evolved in sunlight; Sennebier that it is due to the decomposition of carbon-dioxide, but he believed that the carbon-dioxide is taken up in solution in water. Early in this century de Saussure carried out a long series of experiments on the relations between the carbon-dioxide decomposed, and the oxygen evolved, and on the amount of carbon-dioxide in the air compatible with the healthy development of plants. Since his time many eminent names have been added to the list of patient labourers in this field of inquiry.

Boussingault worked on the question whether the carbon-dioxide is absorbed by the leaves, or taken up by water through the roots; and by direct experiments proved that the leaves of plants do take up the carbon-dioxide, which is so sparingly, though so uniformly, diffused in the atmosphere. His researches led him to conclude that, by far the greater part, if not the whole, of the carbon which enters into the constitution of the organs of plants is derived from atmospheric carbon-dioxide; and while drawing attention to the fact that, for healthy and vigorous action, plants require large volumes of air to pass over them, and to the surprising rapidity with which they absorb the carbon-dioxide from it, he makes calculations as to the surface presented to the air by the leaves of different crops. Taking the average number of plants growing per hectare (about 2½ English acres), he estimates that:—

Artichoke	gives a surface of	142,410	square metres.
Beetroot	,, ,,	49,921	,, ,,
Potato	,, ,,	39,641	,, ,,
Wheat	,, ,,	35,490	,, ,,

Boussingault also made experiments in regard to the absorption of carbon-dioxide by plants growing under different conditions as to soil and manures. He found that a Helianthus which in twenty-four hours would, without any manure, only decompose 2 c.c. of carbon-dioxide, decomposed 182 c.c. in the same time when supplied with manure containing nitrates and phosphates, 11 c.c. when with nitrates without phosphates, and only from 3 to 6 c.c. when manured with phosphates without nitrates.

That the carbon-dioxide contained in the atmosphere is sufficient for normal vegetation is proved by the abundant growth of heath and other wild plants on sandy hills; and the numerous experiments on water-culture conclusively show that a plant may grow luxuriantly, and store up an abundance of carbon, when supplied only with mineral salts, in a solution which contains little or no carbon-dioxide.

Sachs speaks of it as an unquestionable fact, "that most plants which contain chlorophyll (for instance, our cereal crops, beans, tobacco, sun-flower, &c.) obtain the entire quantity of their carbon by the decomposition of atmospheric carbon-dioxide, and require for their nutrition no other carbon-compound from without." He goes on to say: "The compound of carbon originally present on the earth is the dioxide, and the only abundantly active cause of its decomposition and of the combination of carbon with the elements of water is the cell containing chlorophyll. Hence all compounds of carbon of this kind, whether found in animals or in plants or in the products of their decomposition, are derived indirectly from the organs of plants which contain chlorophyll."

Dr. J. Boehm made direct experiments with seedlings of scarlet-runner, growing them under glass shades, luted with potass lye, in pots containing in some cases quartz sand moistened with a nutritive solution, and in others garden-soil rich in humus. The two sets were quite equal in development and duration of life; those in the garden soil formed quite as little starch as those in the sand; and from this he concluded that the carbon dioxide yielded by the garden soil had taken no share in the growth of the plants.

Liebig had, however, supposed that plants might owe some part of their carbon to the carbon-compounds in the soil, which were absorbed by their roots, and that young plants especially drew their supply from this source. He speaks of the effect of drought as checking the supply of carbon-dioxide by the roots, and throwing the plant exclusively upon that in the air.

But the tendency of more recent investigations points to the conclusion that the atmosphere and the parts of plants living in it are solely concerned in the storing up of the carbon of vegetation.

We may pause for a moment to consider the amount of the carbon so stored up.

Liebig estimated that more than 1,000 lbs. of carbon may be harvested annually from a Morgen of surface—somewhat less than two-thirds of an English acre.

According to the estimates of Lawes and Gilbert, with wheat for twenty years in succession on the same land there was an actual yield of 2,500 lbs. of carbon, per acre, per annum, where no organic carbon compounds were added to the soil, and where these were added (in the form of farm-yard manure) the actual yield in carbon was less. With barley, for twenty years in succession, the average annual yield was 2,088 lbs. of carbon per acre; and the indication is that some other crops, under similar conditions, acquire even more.

Estimates recently made of the forest growth in Germany give as much as 2,700 lbs. In tropical climates where vegetable growth is more luxuriant the amounts are far greater; and in the West India Islands as much as from 2½ to 5 tons of carbon may be harvested per acre in the crop of sugar-cane.

With these large amounts of accumulation on the one hand, we have, on the other, an atmosphere containing carbon-dioxide in so small a proportion as 0·04 per cent.

Then we have to bear in mind the large supplies of carbon-dioxide within the pores especially of manured soils, as determined by Boussingault, and at the disposal of the roots of plants. Also the enormous quantity of water taken up from the soil and passing through plants during growth, probably at any rate more than 200 parts for every part of dry substance fixed, and the fact that carbon-dioxide is present in all natural waters would lead to the supposition that the roots would scarcely either take it up to no purpose, or act as a filter to that which constitutes so important a requirement of the plant.

Dr. Moll[1] has recently, by some interesting experiments, made a contribution to the evidence which is required to answer the question—Can leaves decompose the carbon-dioxide which is at the disposition of the roots? and argues that the proof that one part of the plant—the leaf—takes up and decomposes carbon-dioxide, is no proof that it is not taken up in another part—the root.

He quotes the experiments of Sennebier and de Saussure, but considers that they were not made quantitatively, or with sufficient exactness to solve this problem. For its elucidation he rests his methods upon Sach's theory, that the starch in the chlorophyll grains must be considered as the first visible product of the decomposition of carbon-dioxide, and that therefore, according to him, the presence or absence of starch in the leaves is the crucial test of the decomposition or non-decomposition of carbon-dioxide. In Dr. Moll's investigation of the starch contents he used Sach's modification of Boehm's method.

Five sets of experiments were made to meet the different aspects of the question.

In the first set glass shades were used, in one of which the air was kept free from carbon-dioxide by being luted with potass lye, while the other contained ordinary air, or air with an excess of carbon-dioxide, and was luted with water. The liquid lute was in porcelain dishes, made with a round hole in the middle; the central hole and outer edge being deeply rimmed. The shades, of less circumference than the dishes, were set in them, and were furnished with tubular necks, into which smaller tubes were fixed for the current of air to pass through, and for other requirements of the experiments. The exit tube of the shade in which the atmosphere was kept free from carbon-dioxide was conducted through a test-tube filled with pieces of pumice saturated with potass lye. Preliminary experiments with etiolated plants, with a watch glass containing baryta-water within the shade, satisfied the author that he secured having air absolutely free from carbon-dioxide under that luted with potass lye; and some early failures taught him how to regulate the supply of carbon-dioxide and air in the other shade, so as to grow plants as well-developed and healthy as those in the open air. With thick-leaved plants he found that it was necessary to add as much as 2 per cent. of carbon-dioxide to a volume of air supplied to them of about 2,500 c. c. daily, in order to satisfy their requirements for free growth.

Experiments were made with plants of French bean, nasturtium, gourd, and sugar-beet, growing in the open air in pots in good garden soil. From these was selected a leaf, or the upper part of a stem with several leaves, still organically united with the parent plant, which was passed through the hole in the porcelain dish, under the glass shade, and carefully secured air-tight, and from injury to itself, by cork and wadding. The plants for comparison were as nearly alike as possible in every respect, and a control plant grew in the open air between the shades. Both etiolated seedlings, which became green as quickly without carbon-dioxide as in common air, and well developed green gourd leaves, were tried. The gourd leaves, which contained starch at the beginning, entirely lost it within a day or two in the atmosphere deprived of carbon-dioxide, while those in the other shade remained still full of it. The shades, and the contents of the dishes, were then changed, so as to bring the starchless leaves into the shade containing carbon-dioxide. During the day these became again full of starch; while within twenty-four hours it had quite disappeared from the leaves in the other shade. In a similar experiment with sugar-beet the control plant in the open air was covered with a black paste-board box, and it was found that the leaves in the shade deprived of carbon-dioxide lost their starch at about the same rate as those in the dark. In no case was starch found in the leaves while they remained in an atmosphere without carbon-dioxide.

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[1] "Ueber den Ursprung des Kohlenstoffs der Pflanzen." Von Dr. J. W. Moll (Utrecht)—*Landwirthschaftliche Jahrbücher*, Band vi. Heft 2.

The second set of experiments was made with long leaves of bulrush and bur-reeds, which were etiolated, and then separated from the plants. With the same general precautions as before, the upper end of the leaf was inserted in the shade without carbon-dioxide, the lower in an atmosphere containing five per cent. of carbon-dioxide, whilst the space between was left free to the open air. This intermediate part was obscured by tin-foil, so that no starch could be formed in it at the expense of any carbon-dioxide passing through the tissues from the lower shade; and it was supposed that if such a phenomenon were possible, the spacious longitudinal air channels of these plants might be especially favourable to the transmission of the gas. These experiments usually lasted one day, and uniformly gave the same result; starch was formed abundantly where carbon-dioxide was at disposal in the air, while the excess of it in the lower shade had no effect upon the portion of leaf in the upper shade, which remained entirely free from starch.

The apparatus when arranged was always placed in a light window, shaded by gauze blinds if the sun were too hot; and in these latter experiments it was an interesting circumstance that, in the lower portions of these rather thick leaves, more starch was formed on the side next to the window; therefore, in two cases a piece of looking-glass was placed behind the shade, when, being equally illuminated, starch was formed in equal abundance on both sides of the leaf. This variation in the starch-formation, according to the amount of light, showed that that portion of leaf had not always used all the carbon-dioxide at its disposal, and that consequently there was an excess which might have passed upwards through the tissues.

The third set varied from these in having no part of the leaf exposed to free air, thus obviating the possibility of the carbon-dioxide being diffused into it in passing upwards through the plant. A glass vessel containing air without carbon-dioxide was placed within a large shade containing air with 5 per cent. of this gas; and a previously etiolated leaf, with its stem in water, was so fixed as to be partly in the one and partly in the other. After six or eight hours it was examined for starch. Without exception starch was formed abundantly in the parts in the large shade, whilst no trace of it was found in those in the inner vessel even quite close to the junction between the two.

The remaining two sets of experiments were made to ascertain whether starch formation in leaves, in the open air, is accelerated by giving an excess of carbon-dioxide, either to adjoining parts of the leaves themselves, or to the roots. In the first case leaves separated from the plant were divided lengthways. One half, with the stalk in water, was in a shade with air containing 5 per cent. of carbon-dioxide, its upper part projecting under the glass lid of the shade, which was luted with grease, into the open air. The other half of the same leaf was laid on the lid, on filter paper soaked with boiled water to keep it moist, and put as near as possible to the projecting piece of leaf. In the other cases etiolated leaves, organically united with plants whose roots were in rich humus soil, were divided lengthways; one half, quite cut off, was laid near to the other, and the two were examined and compared after some hours' exposure in sunlight. The results of both these sets of experiments were uniformly the same; careful examination showed that starch was formed as readily and plentifully in those portions of leaves excluded from any other source of carbon-dioxide than that in the air surrounding them, as in those having an excess of it at command.

From these experiments Dr. Moll concludes that starch is never formed in leaves in an atmosphere deprived of carbon-dioxide, however much of it may be at the disposa of the other, under- or above-ground, parts of the plant; nor can starch-formation be accelerated in one part of a leaf by an excess of carbon-dioxide being at the disposal of another part of it, either in the air, or through the roots.

The results of these elaborate experiments are doubtless in accordance with the direction of those of other modern inquirers on this subject. At the same time it will probably be felt, that, when long-accepted opinions, which many well-known facts seem to favour, are held to be called in question, we may still ask for further confirmation, before accepting as decisive, conclusions depending on the exact interpretation of experiments made with living organisms exposed to somewhat artificial conditions. It may be hoped, however, that this further instalment of evidence in a given sense will incite to further research.

OUR ASTRONOMICAL COLUMN

DE VICO'S COMET OF SHORT PERIOD.—It has been already remarked in this column that, according to Prof. Brünnow's last investigations relative to this comet, it appears necessary to admit a very material degree of uncertainty in the value of the mean motion determined from the observations of the year 1844, notwithstanding the comet was discovered on August 22, and followed till December 31, or for a period of more than four months, and, moreover, was observed with a degree of precision which has seldom been attained with these bodies. In Prof. Brünnow's masterly and elaborate discussion, "Mémoire sur la Comète elliptique de De Vico," which gained the prize offered by the Royal Institute of the Netherlands, in June, 1848, the planetary perturbations were calculated to the epoch of next return to perihelion in February, 1850, but in consequence of the computed positions showing that observation in that year would be quite hopeless, the calculation was continued with all possible precision to the ensuing perihelion passage early in August, 1855. The computed track in the heavens for this appearance was by no means an unfavourable one for observation; the comet would remain for a considerable period near the earth, being at its least distance on August 2, just before the perihelion passage, when it should have approached our globe, according to Prof. Brünnow's calculation, within 0·58 of the earth's mean distance from the sun. Nevertheless, it was not detected in this year—an object observed by M. Goldschmidt, not far from its track, in May, being certainly a distinct body, if the star of comparison was correctly identified. It was looked for repeatedly with the large refractors at Cambridge and Berlin. In 1860 again, ephemerides were prepared and a search was made, at least at the observatory of Harvard College, U.S., but ineffectually, indeed the chance of observing this comet when the perihelion passage falls in the winter must be but small.

The later results obtained by Prof. Brünnow, to which allusion is made above, will be found in No. 3 of his Ann Arbor *Astronomical Notices:* he there gives his reasons for concluding that he had placed too great reliance upon the value of the mean motion determined in his memoir, and while obtaining a new value (about 650″) which would assign for the period of revolution in 1844 about 1994·0 days, he intimates the necessity of searching for the comet in future on the supposition that this period may be in error ± 30 days. At this distance of time or at the end of the sixth revolution since 1844, so great an amount of uncertainty of course renders the preparation of limited ephemerides useless, but it may be observed that the period finally deduced by Prof. Brünnow would bring the comet to perihelion again in the present summer, and it will certainly be worth while to keep a close watch upon those regions of the heavens which its path must traverse on this hypothesis; we might indeed expect, if the comet continues in the same condition as in 1844, that it would not escape detection, should the perihelion passage fall between the beginning of the present month and the middle or end of October. On July 14 its orbit is thus projected on the sky, the positions consequently indicating the line in which it should then be found according to the different suppositions as to the date of perihelion passage:—

Time from Perihelion.		Right Ascension.		Declination.		Distance from Earth.		Intensity of Light.
+ 40 days	...	60°·3	...	+ 19°·3	...	1·89	...	0·17
+ 10 ,,	...	43·0	...	+ 13·4	...	1·12	...	0·56
− 20 ,,	...	21·2	...	+ 3·0	...	0·64	...	1·68
− 40 ,,	...	350·1	...	− 13·6	...	0·39	...	4·02
− 50 ,,	...	321·6	...	− 24·8	...	0·34	...	4·78
− 60 ,,	...	289·3	...	− 30·1	...	0·38	...	3·60

While it is of importance that an effort should be made to recover the comet, now to all intents *lost*, in the present year, no surprise need be occasioned if the endeavour should prove fruitless. It is quite possible that the mean motion in 1844 was of such amount as would bring the comet, with the influence of planetary perturbation into so close a proximity to Mars at the end of August, 1866, as to occasion very material changes in the elements of its orbit; and again there is the possibility that, as Dr. von Asten suspects has been the case with Encke's comet, it may have encountered one of the minor planets, and with the result of a sensible change in its motion.

And it is to be borne in mind to whatever cause or causes the circumstance may be due, that De Vico's comet has been shown by M. Le Verrier and Prof. Brünnow to be with great probability identical with the comet of 1678 observed by Lahire at Paris; yet in the long interval from 1678 to 1844 there is no record of a comet which can be identified with it, and in the early part of its appearance in the latter year it was visible to the unassisted eye. It does appear strange that in the days of Messier and Pons the comet should have escaped detection at one or other of its returns.

While writing on De Vico's comet we may mention that in heliocentric longitude 339°·6 this body approaches very near to the orbit of the periodical comet of D'Arrest, of which observations may be expected in the present year. The distance is within 0·0055 of the earth's mean distance from the sun, or about 507,000 miles, rather more than twice the moon's distance from the earth, but it does not appear likely that there has been any actual close approach of the two comets during the last fifty or sixty years.

THE LATE PROFESSOR HEIS.—We regret to record the sudden death of Prof. Edward Heis, the well-known German astronomer, which occurred on June 30 from an attack of apoplexy. Prof. Heis was born in 1806, completed his studies at Bonn in 1827, and received in 1852 a call to the ordinary professorship of mathematics and astronomy at the Royal Academy of Münster, Westphalia, which he filled until the time of his death. He was a most diligent and accurate observer in the particular branches of astronomical research to which he devoted himself. His "Atlas Cœlestis Nova" may be considered the standard work for magnitudes of the stars visible in central Europe, his acute vision enabling him to add a large number of stars of what he calls 6·7m. not included in Argelander's "Uranometria." While resident at Aix-la-Chapelle previous to his appointment to Münster he published the results of ten-years' observations upon shooting-stars which were carefully discussed. In 1875 appeared his observations on the zodiacal light, extending over the twenty-nine years, 1847-1875, and forming No. I. of *Publications of the Royal Observatory at Münster*; it is a most important addition to our collection of observations of this as yet little understood phenomenon. From 1858 to 1875 he edited the *Wochenschrift für Astronomie*, a periodical better known on the Continent than in this country. Prof. Heis was also the author of a collection of examples and problems in general arithmetic and algebra, which, we believe, has reached the forty-fifth edition in Germany. His observations of variable stars were conducted upon a system of extreme care, his researches in this direction being encouraged and guided by Argelander; he first established the variability of that irregular star ε Aurigæ, not without a long course of assiduous observation. He was an excellent draughtsman, and produced many fine pictures of nebulæ, though, unfortunately, supplied with very limited optical means.

THE CAXTON EXHIBITION

IT is not too much to say that Science has been advanced by the art of printing more than by any other of the world's inventions, for by it not only has the knowledge of scientific truth been spread throughout the world, but it has been perpetuated to all time, and the names of great heroes in science have been rendered immortal. Long after sculptured monuments, commemorative of the lives and work of great men have crumbled away, their written works remain, and the art of printing has contributed more than anything else to the bringing about of that result. The names of some of the greatest philosophers the world has ever seen would have had but a narrow and comparatively ephemeral celebrity, were it not for the record of their lives and writings which the productions of the printing press have preserved to them.

But great as have been the advantages which Science has derived from the printer's art, she has, in return, conferred as many and as important benefits upon the development of that art; and this is recorded in unmistakable language in the Caxton Collection, which, though (probably for want of space) very deficient as far as modern printing machines are concerned, constitutes a most interesting and instructive series of historical and typical forms, in which the rise and development of printing machinery may be traced from the early screw presses of wood used by Caxton and the early printers, through the Stanhope and lever presses of the last century, to the powerful steam machinery of the present day.

The principal aim of the designers of printing machinery has always been to obtain increased rapidity of working; and during the last fifty years this has been brought to an extraordinary degree of perfection. It was considered a wonderful feat when, in the year 1814, the celebrated König machine was started, throwing off 1,100 sheets of the *Times* newspaper per hour; but this number was doubled by König's second machine, which he brought out ten years after. In the year 1827, by means of Applegarth and Cowper's four-cylinder machine, the yield was raised to 5,000 per hour, and in 1848 the celebrated "Times" vertical machine was erected, which produced 12,000 single impressions per hour. The next advance was made by Richard Hoe, who, in 1857, introduced his cylinder machine into this country, where it was first employed by the proprietors of *Lloyd's Weekly Newspaper*. Shortly afterwards the proprietors of the *Times* adopted it, and by means of a ten-cylinder machine, 16,000 single impressions of the *Times* were thrown off per hour. This was till lately the most rapid printing machine ever invented, but having to be supplied with separate sheets of paper from ten different feeding-boards, it required some twenty men and boys to work it. Since that time a still further advance in the art of printing has been made by the invention of the now celebrated Walter machine, by which the bulk of the *Times* is now produced. This machine works from a continuous roll of paper, printing it on both sides and—requiring the attendance of only a man and two boys—throws off 25,000 single impressions, or 12,500 complete newspapers, per hour.

In all these rapid machines the type *formes* are cast in cylindrically-curved stereotype plates, which are produced by first setting up the matter in type by the ordinary process and then pressing the *formes* so produced into *papier maché* moulds into which the stereotype metal is cast. By this means several plates from the same mould can be produced and therefore the same number of identical sheets may be printed at the same time.

With regard to the actual operation of printing the aid that Science has given has been almost exclusively in the direction of mechanical improvement and perfection. The art of stereotyping or the reproduction of plates and blocks for illustrations has, however, been developed by discoveries in many branches of Philosophy. Electricity has long been employed in the production of copies of wood engravings by the electrotype process, which copies are now almost universally used for rapid work where fine finish is not necessary, and the many processes in which photography is combined with engraving are every day becoming more generally employed for improving and facilitating the art of printing.

It will readily be understood that notwithstanding all the improvements in printing machinery by which such rapidity as we have referred to is insured, the art of rapid printing will be most materially hampered unless the operation of type-setting or composing can be carried on with corresponding rapidity. The importance of this is shown by the attention it has received and by the many systems that have been devised for mechanical and automatic type-setting. A special feature of the Caxton exhibition is the collection of machines for that purpose. Here again Science has lent her aid, and to any one interested in the applications of Science for the assistance of personal dexterity a careful study of the various machines exhibited will be found most interesting and instructive.

One of the most beautiful of these machines is the automatic type-setter of Dr. Mackie, which we illustrate in Fig. 1, and which is a most ingenious application of the well-known principle first invented by M. Jacquard, and applied by him to the operation of weaving, and which has since been employed for telegraphic and other purposes. In this machine a horizontal wheel, carrying a number of little platforms, revolves on a vertical axis beneath a set of upright boxes arranged in a circle round it. Each of these boxes is divided vertically into eight compartments containing the types; and the platforms, during the revolution of the horizontal wheel, pass in succession below, but without touching them. Each platform is furnished with eight adjustable projecting pins, that is to say, as many as there are compartments in the boxes. The use of these pins, or "pick-pockets" as they are called, is to remove the types contained in the corresponding compartments of the boxes at the moment of passing below them; and the types so removed, resting on the platforms, are carried round with them until pushed off at another point in their revolution, where they are collected and delivered in long lines in their proper order, and evenly spaced. The pins are automatically set up or left alone by the Jacquard mechanism to be referred to presently.

Calling the compartments containing the types and the corresponding pins on the platforms by the figures 1, 2, 3, 4, &c., it might at first be supposed that if, for instance, the pins 1 and 3 were set up, they would remove types from the first and third compartments of all the boxes as they passed beneath them, but this is provided against by the platforms being hinged at one end, so as to be capable of rising and falling through a small vertical arc, and by another portion of the Jacquard mechanism each platform is raised only when it is approaching that particular box which contains the compartments to which its projected pins correspond.

The regulation of the movements of both platforms and pins is effected by a set of levers, whose movements are determined by the positions of the perforations on a continuous ribbon of Jacquard paper, which positions correspond to the letters, spaces, &c., required to be set up. This ribbon is fed into the machine at a uniform speed by a revolving spur-wheel armed with pins, which gear into a longitudinal row of holes punched along the centre of the strip of paper, and which is shown in Fig. 2, which represents a piece of the paper ribbon perforated for setting up the name of this journal, "Nature." The four lower rows, which are marked in the figure with Roman numerals, are those by which the rising and falling of the platforms are regulated, and the other eight rows, indicated by ordinary figures, correspond to the eight compartments of the boxes and control the protrusion of the pins or "*pick-pockets*." On reference to the figure it will be seen that the capital letter N is drawn from the fifth compartment of that box, under which a platform is raised by the dropping of the levers, which are controlled by the combination of the two lines of perforations marked I. and IV.; and again the small letter r is contained in the third compartment of a box whose platform is raised by the single lever corresponding to the row marked I.

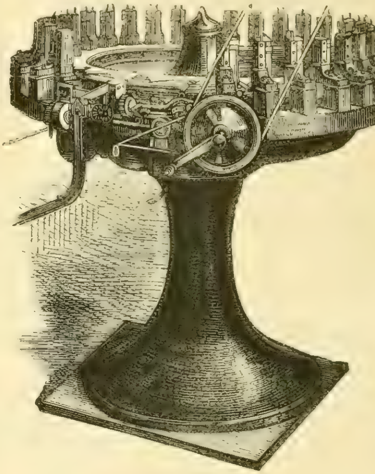

Fig. 1.

The perforation of the paper is done at a separate instrument, which, at the Caxton Exhibition, is, in external appearance, exactly like an ordinary cottage pianoforte, the keys of which are marked with the letters, figures, spaces,

&c., and which, by simple mechanism, punch corresponding holes in the paper when pressed down by the fingers. This operation being quite independent of the machine last described, can be carried on at any slack time, or when the type-setter is in use, and the prepared paper can be put away until the machine is ready to work from it. This is a special advantage of the system which printers will readily appreciate; and it possesses another of great value, and that is that parts of words of two to eight letters, and several short words, can be set up simultaneously, as the compartments are so filled that letters likely to come together are in contiguous divisions and may be released by the mechanism at the same moment. As an instance of this the eight compartments of one of the boxes are filled with types in the following order:—w i t h a t s and spaces, so that the ten words wit, with, it, that, hat, hats, at, as, is, and has, may be drawn by one operation, and the preparation of the paper for such combinations is no less simple, for it is performed by depressing several keys at once, as in playing chords in music.

By this system of type-setting, using one, two, or three perforators respectively, as many as eight, twelve, and twenty-four thousand types may be set up per hour.

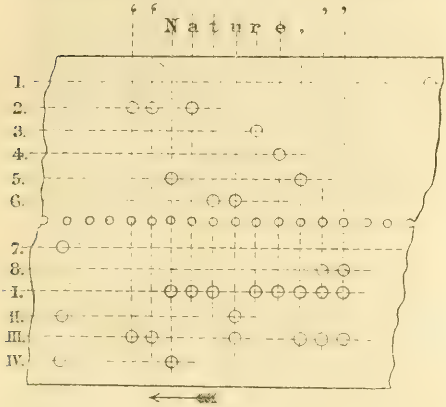

Fig. 2.

We have described Dr. Mackie's machine at some length, because it is a very beautiful application of the mathematical laws of permutations and combinations, and of mechanics to the saving of skilled labour, and is itself an interesting example of some of the services rendered by Science to the printer's art.

Another very ingenious application of Science to type-setting is the "Clowes" electrical compositor, invented by Mr. John Hooker. In this machine the types are contained in forty-eight nearly vertical troughs or reservoirs, and are pushed out through a lateral opening at the lower end by a *striker* under each trough actuated by an electro-magnet, so arranged that, when a current of electricity is sent through its coils, one type is released from its reservoir and drops out. Below the openings of the reservoirs are as many flat running tapes, and when a letter is released it drops on to the tape which is running below it, and is carried by it at the speed of seven inches per second to the edge of the table of the machine, where it is thrown on to another tape running at a quicker speed (about twenty-one and a half inches per second), and making an angle of about 150° with the set of parallel tapes before mentioned. This angle determines the relative distances of the reservoirs from the quick-running tape, and the speeds are so adjusted to these distances that the time occupied by a type in travelling from the reservoirs to collecting apparatus is exactly the same in all cases so that the types are delivered into the composing-*stick* exactly in their order of release from the reservoirs. This part of the apparatus may in telegraphic language be called the "receiving instrument."

The "transmitting instrument" consists of a series of rectangular plates of copper insulated from one another and arranged on a sloping board representing exactly the compartments in the ordinary compositor's "*Lower Case.*" Each of these plates is in metallic communication with one end of the coil of one of the discharging magnets, the other end being in connection with one pole of a voltaic battery consisting of two small Grove elements. The compositor sits in front of this set of plates, having the copy before him, and holding in his hand a copper stile or contact piece which is in connection with the other pole of the battery. Every time he touches with the stile one of the rectangular plates of copper a voltaic current is sent through the coils of its corresponding magnet and a letter corresponding to the plate touched is liberated on to the tapes and is instantly carried to the composing-stick. The collecting apparatus is extremely ingenious and is worked by a quick running cam by simple mechanism, which is a beautiful specimen of workmanship.

By this machine as many as 15,000 letters per hour may be set up; and it possesses the advantage over other systems that it can be worked by any ordinary compositor *at case*, and requires no special training for its manipulation.

Of other type-setting machines there are exhibited in the collection examples of Kastenbein's system, which is adopted in the *Times* office; the Hattersley compositor, in which the types are, by the depression of keys, shot down vertical grooves, by which they are guided to the composing frame, and by which it is said that types may be set at the rate of 8,000 per hour. Muller's machine, which is represented in the collection by a model, is a well-made apparatus, intended to set type at a speed of 5,000 letters per hour. Both this and the Hattersley machine set up the type in columns, ringing a bell at the end of each line.

Heinemann's apparatus is an exceedingly simple machine, depending upon quickness of hand and eye in aiming a pointer at the particular divisions of a comb-shaped series of guides, by which the types are withdrawn from the reservoirs corresponding to those divisions. It is a well-made machine, and its simplicity is a safeguard against its becoming deranged.

The operations of type-founding, of paper-making and folding, of lithography, and steel engraving, which are all more or less dependent upon scientific aid, are all represented at South Kensington, but we must reserve their consideration for a future notice, as well as a description of an interesting gas-engine, exhibited by Messrs. Crossley Brothers, which is admirably adapted for laboratory purposes.

From what has been said it will be seen that the Caxton Exhibition is an exceedingly interesting and instructive one, and will well repay several visits. C. W. C.

NOTES

We are glad to see that the first grants from the Research Fund of the Chemical Society have just been made. They are as follows: to Dr. C. R. A. Wright 50*l.* for the investigation of certain problems in chemical dynamics; to Mr. G. S. Johnson 25*l.* for a research on double salts with potassium tri-iodide; to Mr. E. Neison 25*l.* for a research on octyl compounds; to Mr. Carleton Williams 25*l.* for a research on hydrocarbons containing the group isopropyl twice; and to Mr. George Harrow 10*l.* for a research on derivatives of aceto-acetic ether.

THE German Astronomical Society, as it is generally called, but really the International Astronomical Society, meets this year at Stockholm, from August 30 to September 1.

NOW that the British Association meeting is again at hand, perhaps we may be permitted to urge upon the authorities the necessity for introducing some improvement in the daily programme published during the session. Last year (vol. xiv., p. 463) we noticed the handsome, full, carefully-arranged programme daily published by the American Association, and we have before us the *Tageblatt* of last year's meeting at Hamburg of the German Naturalists and Physicians. This is a quarto publication, each day's issue averaging twenty pages, and containing such important and detailed information that it is well worth binding and preserving. The rules of the Association are given in the first issue, a list of members with their addresses is given daily until complete, the arrangements for the meetings of sections and general meetings are clearly tabulated, a well-arranged general programme and list of all papers to be read each day are given, all information as to excursions, places to be visited, entertainments (including theatres), advertisements likely to appeal to members, summaries of each day's proceedings in the various sections, general meetings, dinners, &c. ; in short every kind of information likely to make the proceedings be carried on with perfect smoothness and give the members the minimum of trouble and worry. Appended is a supplement of 180 pages containing reports of lectures given at general meetings and in connection with the various sections. All this contrasts strongly with the shabby tract-like programme issued during the meetings of the British Association; if the expense is an obstacle we are quite sure no member would object to a small charge if he could only be assured of obtaining each morning a well-printed journal on a scale similar to that of either the American or the German Association.

AT the Congress on Domestic Economy to be held at Birmingham on the 17th, 18th, and 19th instant, under the presidency of Lord Leigh, Prof. Huxley will read a paper on Elementary Instruction to Children in Physiology, Mr. W. S. Mitchell one on the Practical Use of the Food Collection of the Science and Art Department, and Captain Galton on Warming and Ventilation. Many other papers bearing on the subject of the Congress will be read, and an exhibition will be organised.

MANY of our biological readers will be glad to know that Dr. George Bennett, F.Z.S., of Sydney, has arrived in this country, and that any communications directed to the office of the Zoological Society, 11, Hanover Square, will be forwarded to him.

THE Portuguese African Exploring Expedition left Lisbon for Loanda on the 7th inst.

MR. RICHARD S. FLOYD, one of the trustees of the Lick Californian estate, has been for a year past in Europe, investigating the comparative merits of reflectors and refracting lenses, for the great telescope. We are told by the *New York Tribune*, for various reasons, which he gives in detail, he decides against a large reflector, one point being that even if the extreme nicety of adjustment which the reflector requires could be attained in the new observatory, it would be liable to derangement in the high winds of a mountainous position. If a refractor is decided upon, estimates should be asked, Mr. Floyd says, from Cooke and Sons, of York, from Alvan Clark and Sons, and from Howard Grubb. He reports that the reputation of Clark's refractors and Grubb's, from all he can learn abroad, is about equal. The story of Mr. Lick's millions had preceded Mr. Floyd, and he has found it difficult to bring down the estimates of European opticians to the basis of ordinary business profit.

IN the debate on the education estimates, on Tuesday night, Sir John Lubbock, speaking on the extra subjects which had been made compulsory, said he doubted whether under any circumstances it would be desirable thus to stereotype one form of education for the whole of England; but surely we ought not to do so unless we were very clear as to what is the best system. There was, however, very great difference of opinion on this head. The first authority to which he would refer was that of a committee of that House. It was presided over by his hon. friend the member for Banbury, and after careful inquiry they reported that in their opinion "elementary instruction in the phenomena of nature should be given in elementary schools." The next authority which he would quote was the Royal Commission, presided over by the Duke of Devonshire, which unanimously recommended that more substantial encouragement should be given to the teaching of the rudiments of science in our elementary schools. In Scotland, too, great dissatisfaction was felt with the present system. At the last conference of elementary teachers, held in London, which was very numerously attended, it was resolved that the system of payment "embodied in the Code is unsound in principle and injurious to the progress of true education." The inspectors of schools differed greatly as to the most suitable subjects. Even in regard to geography they were not unanimous. It was said as a subject to lend itself very much to "cram." One of the inspectors gave in support some very amusing answers. For instance, in answer to a question of "What are mountains and rivers?" one girl replied that "Mountains in some parts of the world are very useful. In Africa, for instance, they shoot out gold." Of rivers she had not so favourable an opinion, though she thought "they were all very well in some countries where there was very little rain." He confessed, however, that he thought geography a very good subject, though he was not convinced that it ought to be continued during the whole course to the exclusion of other subjects. The mere skeleton of history taught in our elementary schools contained little more than dates, wars, and murders; but dates and crimes no more constituted the history of a nation than sinews and bones made a man. Men of science must be grateful to Sir John Lubbock for so constantly urging upon Government the importance of scientific education.

THE first practical response to the proposal for the establishment of a colonial museum in London has come from the Legislative Council of Ceylon, which has voted as its contribution a sum of 15,000*l*., to be paid in three annual instalments. The Council of the Colonial Institution is about to issue another circular on the subject to the colonial Governments.

AT the last sitting of the French Geographical Society, excellent news was received from M. de Brazza, the French explorer of the Ogové. He reached a distance of 250 miles beyond the place where M. de Compiègne was obliged to retreat hastily to save his life. He finds that Ogové does not bend towards the Zaire. If its course does not change further up, both streams may belong to a single system. It was reported, also, that M. Say, a French officer in the National Marine, had reached the Hoggar, in Central Africa, but the news requires confirmation.

THE *Bulletin* of the Paris Geographical Society for April (just issued) is mainly occupied with a long and elaborate review of the geographical work of the year 1876, by M. Ch. Maunoir. M. de Bizemont discusses some of the observations for latitude obtained by M. de Brazza during his exploration of the Ogové. In connection therewith M. de Bizemont gives a list of the instruments which he considers most useful to explorers in new countries.

AMONG the papers in this month's part of *Petermann's Mittheilungen* is one on the Cartography of the Philadelphia Exhibition; Dr. Güssfeldt contributes an important paper on the exploration, by himself and Dr. Schweinfurth, of that part of the Arabian desert between the Nile and the Gulf of Suez; and Dr. Radde a paper on the plain of the Upper Euphrates. Dr.

C. E. Jung has the first part of an important contribution on the Geographical Outlines of South Australia.

THE *Geographical Magazine* for July contains a masterly paper, with an elaborate and carefully-constructed map by Mr. Trelawny Saunders on the Himalayan system. Both article and map are evidently the result of thorough study and extensive knowledge.

FROM a Report of the Board of Commissioners of the New York State Survey, which is under the charge of Mr. J. T. Gardner, formerly of the United States Geological Survey, we learn that, although the Survey was decided on only in 1876, much has already been done in the way of commencement, and that it is likely to be carried out with a thoroughness quite equal to any of the trigonometrical surveys of Europe.

MR. LANDSBOROUGH, the well-known Australian explorer, recently read a paper at Oxley, Queensland, in which he adduces a variety of interesting facts to prove that dense forests are on the increase in Australia, that the climate is becoming moister, and therefore improving, that the country is gradually ceasing to be favourable to sheep-rearing, and becoming agricultural, and seems to hint that in course of time the great central desert may yet "blossom as the rose." Formerly when there were no sheep to keep down the grass, fires were frequent and terribly destructive to trees and all vegetation, but since the stocking of the country there is less grass for the fires to consume, and their ravages are consequently becoming limited in extent. Queensland, especially, Mr. Landsborough declares, is now so unfit for sheep-pasturing, that no one thinks of making a living by them. The observations of this experienced traveller are well worthy of attention, and it will certainly be interesting to watch the changes caused by the presence of civilised men in Australia, as we know exactly its condition at their first advent.

M. WADDINGTON, the late French Minister of Education, our readers may remember, sent out a number of men to various countries for the purpose of scientific exploration. The following is a list of these missions:—M. Masqueray in Algeria; MM. Pinard and de Cessac, North America; M. la Gaviniere, Celebes; Marignac, Antilles; Armingaud and Malard, Italy; Dr. Harmand, Cochin China; Wiener, Peru and Bolivia; Raffray and Maindron, New Guinea; Ed. Blanc, Maritime-Alps; Ratte, New Caledonia; Ujfalvy, Central Asia; Serre, Say, Sahara; Rochemonteix, Egypt; du Chatelier, Finistere (France); Abbé Ansault, Italy; Mangeot et Bersot, Japan; Mouchez, Algeria; Guizet, Japan and China.

WE have received the *Bulletin*, for 1876, of the Essex Institute (Salem, U.S.), one of the best of the many local societies of the United States. A large portion of the *Bulletin* is occupied with a valuable paper by Mr. E. W. Nelson, on the birds of North-East Illinois. We may state that this Institute is issuing a series of "Historical Collections," which are likely to be of service to those who are interested in the political history and social progress of the United States.

THE *Kansas Collegiate* is the title of a small sheet conducted by the students of Kansas State University, and contains various notes and news likely to interest those for whom it is intended. The number for May 23 contains a Scientific Supplement devoted to subjects of more or less scientific importance. The longest of these is an address by Prof. F. H. Snow, on "The Relation of Birds to Horticulture," and which contains some interesting information on the habits of many of the Kansas birds. Another paper, by Prof. G. E. Patrick, gives the results of an examination of a meteorite found at Wacender, Mitchell Co., Kansas. Prof. Snow, we notice, has formed a fine and constantly-increasing collection of the birds of Kansas.

THE *Commission Supérieure*, or governing body of the French International Exhibition of 1878, has been completed by the appointment of some influential members, among whom are the Duc d'Audifret-Pasquier, president of the French Senate, M. Andral, the vice-president of the Council of State, M. Alphaud, the chief engineer of the Paris works. Amongst the ordinary members are M. Brunet, Minister of Public Instruction, M. St. Claire-Deville, member of the Institute, and M. Rothschild the banker. M. Krantz has given a detailed report on the state of the works, which are much in advance of the specified time. The unexpected success of the exhibition in foreign countries and especially in Great Britain and the British Colonies will fill up the vacuum created by the abstention of Germany. Many nations have asked for an enlargement of the space allotted which it has been impossible to grant. The public will be admitted by tickets and not by turnstiles. The coffee-houses, balls, concerts, theatres, so numerous in the 1867 exhibition, have been abolished, but great experiments for testing the apparatus exhibited, and promoting human knowledge will be tried. China will be represented by an official commission, and Siam will make a magnificent display. Liberia, the negro republic on the Gold Coast, will exhibit for the first time in France.

THAT science in certain of its applications does pay is evident from the fact that a M. Delille, a "professor" of legerdemain, who has practised at fairs in France, and who has died at the age of eighty-eight, gained by his trade a fortune valued at several millions of francs. He began to practice at the early age of sixteen, and was seen operating at the last fair of St. Germain. He dealt largely with electricity. Here is another argument agains the Endowment of Research very similar to one which has been urged before.

FROM the prospectus of St. Thomas's Hospital Medical School, we notice that two scholarships of the value of 60*l*. and 40*l*. respectively will be awarded during the first week in October, after an examination in physics, chemistry, botany, and zoology.

THE King of the Belgians, who has been appointed a second time president of the International Association for Exploring and Civilising Central Africa, has declared that next year he will decline to continue the office. The Society is possessed of an annual revenue of 73,000 francs, principally from subscriptions obtained in Belgium, where the scheme is very popular. It has been decided by the executive committee that a station should be founded in the Trans-Tanganyika region. The head of the station and the explorer have been appointed. A depot will be formed at Zanzibar, and three others in intermediate countries; one is to be placed under a Catholic mission, and two under two Protestants, who have volunteered to help the Association. The works are to be begun without further delay.

A COMPANY is now being formed, we learn from the *Engineer*, to construct a pneumatic railway between the South Kensington Station of the District Railway and the Albert Hall. The line will rise the whole way to the Albert Hall, the ruling gradient being 1 in 48. The train will be blown through the tube by an ejector, in other words, a great centrifugal pump, two feet in diameter, fixed close to the District station, and worked by a pair of condensing engines exerting about 170 indicated horse-power. The tunnel will be of brick, and the floor will be paved. Its cross-sectional area will be 105·5 square feet; at the end of the train is fixed a screen or piston, with an area of 104 square feet, the difference being allowed for windage. The train will consist of six carriages, of very light build, the rail gauge being four feet. This train will hold 200 passengers, and the total load will be thirty-two tons, or ten tons less than the weight of a single engine on the Metropolitan Railway. The maximum resistance at twenty miles an hour will be about 2,420 lbs., requiring to overcome it a pneumatic pressure of 2·6 ounces per square inch, and 162-horse-power, assuming the useful effect to be sixty per cent.

A VERY severe thunderstorm passed over London on the evening of July 5. Between eight and nine there came a very brilliant flash of lightning, followed by a deafening peal of thunder. Many people were stunned and in several cases were found quite insensible. Immediately after it was found at Kilburn that the telegraph wires, running from the top of the Queen's Arms to a house about 300 yards higher up the Edgeware Road, were struck by the lightning, and fell in red-hot fragments, varying in length from six inches to an inch, all along the road, a great deal of yellow smoke attending the fall of the wire. In one or two houses windows were broken, and a little girl who was passing through the street had her hair singed and her jacket burnt. The instruments at the office with which the destroyed wires were connected were much agitated, and the telegraph clerk, a young lady, was much stunned.

THE fourth edition of the "Lists of Elevations principally in that portion of the United States West of the Mississippi," edited by Mr. Henry Gannett, and published in connection with Mr. Hayden's Survey, must prove of great value to the geographer and meteorologist. The first edition, published in 1872, contained only thirty-one pages, the present edition contains 164 pages. It contains, among a variety of other matter, profiles of nearly all the railroads in the part of the United States above mentioned. The results given by these profiles have been made to accord, and the heights of several thousands of points on them have been determined with an approach to accuracy. This edition contains also the heights of many thousands of points determined approximately by means of the barometer. Elevations of many thousands of mountain-peaks are given, from which very correct ideas of the ruling heights of the principal ranges may be derived. It contains also tables of the slopes of the principal streams of the west, which are of value in studying the important question of irrigation. With these various lists of elevations there is given with this edition a map of the United States, in approximate contours of 1,000 feet of vertical intervals, which, in a measure, embodies all the results of this department. Toward the improvement and ultimate perfection of this map this work is to be mainly directed in future. To express still more clearly the facts brought out by the map, it is the intention of the Survey to make shortly a relief model of the United States, on the basis of this map.

WE have on several occasions referred to the association known as the Yorkshire Naturalists' Union, composed of a large number of local scientific societies in Yorkshire. This association publishes a useful monthly journal, *The Naturalist*, intended as a general field club record. We have received the twenty-fourth number of this journal, which, besides several papers on natural history, contains reports of several of the associated societies. From a report of the third meeting of the Union held recently at Wakefield, we notice that the Bradford Scientific Association was admitted to the Union, and that a testimonial, in the shape of a microscope, was presented to Mr. J. M. Barker, late secretary of the West Riding Consolidated Naturalists' Society.

THE additions to the Zoological Society's Gardens during the past week include four Common Kingfishers (*Alcedo ispida*) European, presented by Mr. J. Lyford; two Horned Lizards (*Phrynosoma cornutum*) from Texas, presented by Mr. W. A. Bowie; a Sun Bittern (*Eurypyga helias*), a Sacred Ibis (*Geronticus æthiopicus*), bred in the Gardens; eight speckled Terrapins (*Clemmys guttata*), three Red-vented Terrapins (*Clemmys rubriventris*), two American Box Tortoises (*Terrapene carinata*) from North America, purchased.

THE INFLUENCE OF LIGHT UPON THE DEVELOPMENT OF BACTERIA[1]

WE have been engaged during the last few months on an investigation into the effect of light upon the development of bacteria in certain of those solutions in which they are usually produced.

We reserve the details for a paper which we hope to submit to the Royal Society in the course of their next session, but wish to state, in the meanwhile, that the first portion of our inquiry has led us to the following conclusions:—

1. That light is inimical to the development of bacteria.
2. That under favourable conditions it may prevent their development.
3. That under less favourable it may not prevent but only retard.
4. That for the full effect of light to be produced direct insolation is necessary.
5. That those conditions which tend to neutralise the action of light are the same which are known to favour processes of fermentation and putrefaction.
6. That the fitness of the solution to serve as a nidus is not destroyed by insolation.
7. That, so far as our investigation has yet gone, it would appear that the germs originally present in the solution are destroyed by direct insolation.

We are still pursuing the inquiry, and have devoted much time to investigating the influence of the refrangibility of the ray, but regret that at present we are not in a position to give any definite conclusions on this point.

We are endeavouring also to trace an analogy between facts which we have observed and certain vital and chemical processes, in which light is known to play a part, and are extending our observations to other phenomena of fermentation and to microscopic fungi.

That light is not essential for the development of bacteria has been long known, but that it is absolutely inimical to their production has not, so far as we are able to ascertain, been previously shown, and we are encouraged, therefore, to lay before the readers of NATURE this statement of our results.

ARTHUR DOWNES; T. P. BLUNT

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

LONDON.—The following is the list of the new associates of the Royal School of Mines:—Associates in Mining and Metallurgy—C. W. Folkard, A. K. Huntington, E. W. Voelcker; Associates in Mining—E. H. Liveing, W. H. Merritt; Associates in Metallurgy—A. C. Copeland, J. F. Hogan, C. H. Lemann, W. Leyson, E. T. McCarthy; Associate in Geology—A. R. Sawyer. The Edward Forbes Medal and prize of books was awarded to A. Heilprin; the De la Beche Medal and prize of books to E. W. Voelcker; the Murchison Medal and prize of books to F. G. Mills.

SCIENTIFIC SERIALS

Journal de Physique, June.—On the theory of electrometers, by M. Mascart.—On the dynamical theory of gases (continued), by M. Violle.—Process for measuring the index of refraction of liquids, by M. De Waha.—Application of the electric current to the study of the spheroidal state of liquids, by M. Heselius.—Temperature and humidity of the air at different heights observed at Upsala during 1875, by M. Hamberg.—Proceedings of the Physical Society of St. Petersburg.

Archives des Sciences Physiques et Naturelles, June 15.—Study on the variations of transparence of the waters of Lake Leman, by M. Forel.—On the different modes of crystallisation of water, and the causes of the varied appearances of ice, by M. Pictet.—Researches on some niobiferous and tantaliferous minerals, by M. Delafontaine.

Annalen der Physik und Chemie, No. 4, 1877.—Johann Christian Poggendorff (memoir).—New experiments on the expansion of bodies by heat, by M. Glatzel.—On the objections of Clausius to Weber's law, by M. Zöllner.—On normal magnetisation, by M. Petruscheffsky.—On stratification of the electric light in Geissler tubes after insertion of a flame and some other resistances, by M. Holtz.—On the cohesion of salt solutions, by M.

Quincke.—On the excitation of electricity through gliding friction, by M. Riess.—On unipolar induction of a solenoid, by M. Zöllner.—Remarks on Prof. Neumann's paper on the number of electric materials, by M. Edlund.

No. 5.—On the reflection of heat rays from metals, by M. Knoblauch.—On the treatment of ponderomotive and electromotive forces occurring between linear currents and conductors, according to the fundamental laws of electrodynamics, by M. Clausius.—On the tensions of vapour in dissociation of salts containing water of crystallisation, by M. Pareau.—On the coefficients of temperature of heat conduction of air and hydrogen, by M. Winkelmann.—On the phenomena of motion of electrified mercury in glass vessels, by M. Herwig.—On divergences from Ohm's law in metallically conducting bodies, by M. Braun.—On the theory of unipolar induction and Plücker's experiments, by M. Riecke.—On heat conduction in sulphate of copper, by M. Pape.—Remarks on the polarisation of the rainbow, by M. Lommel.—On the history of the invention of the areometer, by M. Gerland.—On the significance of the rhombohedric and prismatic surfaces in quartz, by M. Baumhauer.

SOCIETIES AND ACADEMIES

London

Zoological Society, June 19.—E. W. H. Holdsworth, F.Z.S., vice-president, in the chair.—The secretary read a letter addressed to him by Mr. J. M. Cornély, announcing that his female *Hydropetes inermis* had just produced three young ones.—Mr. J. E. Harting, F.Z.S., exhibited and made remarks on a variety of the common Snipe, intermediate between the usual form of that species and the so-called Sabine's Snipe.—Mr. B. Tegetmeier, F.Z.S., exhibited a specimen of a curiously malformed sternum of the Tawny Owl.—Mr. John Murray, Naturalist to the *Challenger* Expedition, exhibited and made remarks on a series of sharks' teeth, whales' ear-bones, and other specimens dredged up at great depths during the *Challenger* Expedition.—Mr. P. L. Sclater, F.R.S., read the first of a series of reports on the collection of birds made during the voyage of H.M.S. *Challenger*, containing general remarks on the collection, which was stated to consist of about 679 skins of terrestrial and 198 of oceanic birds, besides a considerable series of specimens in salt and in spirit, and a collection of eggs, principally of the oceanic species.—A communication was read from the Marquis of Tweeddale, F.R.S., containing a report on the collection of birds made during the voyage of H.M.S. *Challenger* in the Philippine Islands. Amongst them were examples of seven species new to science.—Mr. P. L. Sclater read a paper giving a description of the birds collected at the Admiralty Islands during the visit of the *Challenger* expedition to that place. Amongst these were examples of six species hitherto unknown to naturalists.—A communication was read from the Rev. O. P. Cambridge, C.M.Z.S., on some new species of Araneidea, with characters of two new genera and some remarks on the families *Podopthalmides* and *Dinopides*.—A note was read by Mr. J. H. Gurney on the breeding of the Polish swan in captivity, and on the stages of plumage of the young birds.—A communication was read from Mr. F. Moore, in which he gave a complete description of the Lepidopterous fauna of the Andaman and Nicobar Islands, so far as is yet known.—A communication was read from Mr. Herbert Druce, F.Z.S., containing a revision of the Lepidopterous genus *Paphia*, with descriptions of twenty-one new species.—A communication was read from Mr. E. J. Miers, F.Z.S., containing the description of a collection of Crustacea (*Decapoda* and *Isopoda*), chiefly from South America, with descriptions of new genera and species.—Mr. A. H. Garrod read a description of the brain of the Sumatran Rhinoceros (*Ceratorhinus sumatrensis*).—A paper by Mr. A. D. Bartlett, contained the description of a new Guinea Fowl, from Mombassa, in Eastern Africa, based on a specimen brought home by Mr. Gerald Waller, for which the name *Numida ellioti* was proposed.

Entomological Society, July 4.—Prof. Westwood, president, in the chair.—Mr. J. W. Douglas exhibited a living specimen of *Cerambyx Heros* and a young larva of the same insect, bred from a log of wood imported from Bosnia.—The president exhibited some cases composed of small semi-transparent quartz-like particles and constructed by the larva of a Trichopterous insect inhabiting Southern Europe. They had been described by Swainson in 1840 as a shell belonging to the genus *Thelidomus*.—The president also exhibited a plant-bug (*Capsidæ*) found on the leaf of an orchis which had become covered with blisters from the attack of the insect.—Mr. Jenner Weir exhibited a female specimen of a *Cicada* taken in his presence in the New Forest by Mr. Auld, who stated that he had heard it stridulating. Mr. Douglas, however, suggested that the sound had been produced by a male concealed near.—Mr. S. Stevens exhibited two living specimens of *Tillus unifasciatus* taken on a fence near Norwood.—Mr. J. P. Mansell Weale, who had just returned from South Africa, exhibited a fine collection of insects from that country and read a paper containing the results of his observations and experiments upon the breeding of *Papilio merope* and other insects.—The secretary read a letter from Dumfries stating that *Colias edusa* had made its appearance in that district in the month of June.—The president brought before the Society the recent accounts of the appearance of the Colorado beetle in Canada and in Europe.

Physical Society, June 23.—Prof. G. C. Foster, president, in the chair.—Prof. W. Grylls Adams exhibited a very complete form of optical bench, which, in addition to being provided with all the improvements introduced by Prof. Clifton, carries an arm which can be set at any angle to it and is provided with appliances for studying a beam of light or radiant heat when it deviates from the main axis of the instrument. At the base of a pillar firmly clamped in any position in the manner adopted by Prof. Clifton, is fixed a horizontal graduated circle, and a vernier, attached to a counterpoised arm, which rotates round the axis of this pillar, renders it possible to determine the angle made by the arm with the bench to one minute. At the upper extremity of the pillar is a steel pivot to which various appendages may be clamped, and immediately below this is a second graduated circle by which to determine the angular position of whatever is supported by the pillar. Mirrors, metallic surfaces, prisms, &c., may be placed on this pillar for the reflection, refraction, diffusion, or polarisation of heat and light. For radiant heat the rotating arm carries a line thermo-electric pile and a table on which absorbing media may be placed. Prof. Adams illustrated the use of the instrument by projecting on to a screen the interference bands obtained when a beam of light, after reflection from the two surfaces of a thick plate of glass, is again reflected from the two surfaces of a similar plate placed very nearly parallel to the first. A compensator consisting of two plates of glass of equal thickness is also added between the two thick plates, and an ingenious arrangement renders it possible to incline the glasses at any angle to one another, and to move them either independently or together. He also showed the effect produced in the positions of the bands when the rays from the two surfaces of the first plate traverse air of different densities before falling on the second. The adjustment of this latter was facilitated by fine screws supplemented by springs which rendered it possible to give a slight movement to the plate in any direction, by combining a motion of translation of the plate parallel to its reflecting faces with a motion of rotation about a vertical or horizontal axis.—Mr. F. D. Brown exhibited an apparatus he has arranged, in which to compare thermometers. From a brass hemispherical boiler rises a tube of the same metal two inches in diameter and about two feet long; the steam, after ascending through it, descends a metallic jacket surrounding it, whence it passes into a U-shaped condenser, and from this it is returned to the boiler. The upper end of the condenser is in connection with a large air-tight vessel forming the base of the apparatus, and in which any required degree of exhaustion can be maintained by the use of Lothar Meyer's form of pump. The thermometers are placed in tubes, which pass within the wide brass tube at its upper end, and by varying the nature of the liquid in the boiler, and the pressure to which it is subjected, the boiling point can be retained constant at any required temperature.—Dr. Guthrie and Mr. Akroyd communicated a paper on electrical selection. When a metal or other body is rubbed against some non-conducting substance like caoutchouc, electricity is developed, and the track of the metal may be readily made evident by sprinkling on the caoutchouc a mixture of red lead and sulphur. This sieving imparts negative electricity to the sulphur and positive to the red lead, hence that particular ingredient of the mixture is drawn to the metal track which possesses the opposite kind of electricity. Iron, for example, when rubbed against caoutchouc generates negative electricity, and, after sprinkling the powder, the iron track is revealed by the marked collection thereon of red-lead. A list of mixtures was given which may be used instead of the above, and it was shown that electrical selection may prove of use (1) in making an electrical diagnosis of the metals, (2) in certain experiments where the quadrant electrometer is objectionable, and (3) in teaching, where this instrument is often unavailable on account of its cost. An adjourned special general meeting of the society was then held, after which the meetings were adjourned until November next.

Anthropological Institute, June 26.—Mr. John Evans, F.R.S., president, in the chair.—Three new members, one honorary, and one corresponding member were elected.—Mr. Burt exhibited the prow of a New Zealand war canoe supposed to have been that which met Capt. Cook on his second visit to that country.—Mr. Atkinson exhibited a specimen of gold so-called Irish ring money.—A paper by Mr. Mortimer on an underground structure near Langtoft, Yorkshire, was read. Mr. Mortimer considers it is not a Botontinus; in this opinion he is opposed by Mr. J. E. Price, F.S.A.—Mr. C. H. E. Carmichael, M.A., read a paper on a Benedictine missionary's account of the natives of Australia and Oceania, in which he summarised the principal points of anthropological interest in the *Memoire Storiche dell' Australia*, by Mgr. Don Rudesindo Salvado, O.S.B., and illustrated the missionary's account by reference to the views expressed by Topinard, Virchow, and other foreign writers. Mgr. Salvado maintains the unity of the Australian race and language, and upholds the possibility of raising the aborigines to a fairly high pitch of civilisation, using their extreme quickness in learning to speak and write European languages. Two letters written in Italian by native boys brought to Europe at about eleven years of age, were commented upon by Mr. Carmichael, who laid copies on the table, and promised further investigation of the questions raised in the discussion.—The president, Mr. C. Walford, Sir John Lubbock, and others took part in the discussion.—H.I.M. the Emperor of Brazil was present at the meeting.

Vienna

Imperial Academy of Sciences, April 12.—Senile changes of the joints and their connection with *Arthritis deformans*, by M. Weichselbaum.—On a new determination of a quantity having reference to the measurement of molecules from the theory of capillarity, by M. Boltzman.—On the orbit of Dione 106, by M. Seydler.—On the decomposition of hydroxylamin by alkaline copper solution, by M. Donath.—On a method of determining the resistance of bad conductors of electricity, by M. Domalip.—Influence of temperature on velocity of evaporation, by M. Baumgartner.—On diffusion of vapours through clay-cells, by M. Puluj. The velocity of diffusion depends on the temperature in the same way as the maximum of tension. There is not direct proportionality between velocity of diffusion and difference of tension. The logarithmic function represents the connection more accurately.—A contribution to the knowledge of viscous substances, by M. Obermayer. Experiment showed that the internal friction in brittle black pitch follows the same laws as fluid friction. That in soft bodies does not exactly follow those laws.—On the internal condition and the latent heat of vapours, by M. Puschl. Very rare aqueous vapour deviates from Mariotte's law in an opposite direction to that of gases and vapours generally, and in this it behaves like very greatly rarefied atmospheric air.

Paris

Academy of Sciences, July 2.—M. Peligot in the chair.—The following papers were read:—On the generation of the meridian curve of a surface of revolution, of which the mean curvature varies according to a given law, by M. Resal.—Researches on anhydrous chloral and on its hydrate, by M. Berthelot. There is a liberation of heat in the reaction of gaseous chloral with gaseous water, with formation of a gaseous compound, gaseous hydrate of chloral therefore truly exists as a compound distinct from a simple mixture of the two vapours.—Remarks on the subject of M. Mouchez' letter of June 18, by M. Villarceau.—On the distribution of waters coming from natural slopes of the French territory, and on the amelioration of our interior navigation, by M. De Lesseps. M. Cotard has suggested the storing of water in the higher parts and distribution of it to navigation-canals giving cheap transport for materials of small value, and avoiding the formation of unwholesome marshes. M. Sibour advises the opening of a canal (seven kiloms.) between the lake of Berre and the harbour of Marseilles.—Reply to M. Roudaire's last communication on the formation of a Saharan sea, by M. Cosson. *Inter alia*, he urges that the change of the local climate would be fatal to the date, and that new plants introduced would not compensate the loss. The Artesian system is open to being greatly developed. The caravans of Central Africa would not diverge from their route to Morocco and Tripoli. The addition of so much saline matter would make the Artesian water undrinkable and unfit for irrigation. The climate would become very unhealthy from combination of moisture with great variations of temperature, &c.—M. Godron was elected correspondent for the section of botany, in room of the late M. Lestiboudois, obtaining thirty-three votes, against five for M. Duval Jouve.—Trepanation of the membrane of the tympanum, successfully performed in a case of long deafness which had resisted all treatment, by M. Bonnafont. Any deafness not accompanied by weakening of the sensibility of the acoustic nerves (ascertained by placing a watch on the cranial wall near the ear), may be cured or greatly improved (he thinks) by trepanation of the tympanic membrane. The tympanum should be anæsthetised, and the canula should be allowed to remain in the opening till it falls out naturally.—Argilo-calcareous land and phylloxera, by M. Joffroy. A vine-stock planted in such land resists the disease when the surface of the ground is sufficiently inclined from its base, and is preserved from contact with rain-water from higher ground.—Researches on the compressibility of liquids, by M. Amagat. He studied volatile liquids kept liquid by pressure at a temperature above that of their boiling-point (when, it is known, their coefficient of dilatation becomes very considerable). He gives numerical results for ordinary ether and chlorhydric ether, and will afterwards show that these numbers agree satisfactorily with deductions from the formulæ of the mechanical theory of heat.—On the state of the vines treated at Cognac with alkaline sulphocarbonates, by M. Mouillefert.—On the vapour of hydrate of chloral, by M. Troost. Fresh experiments by a method which he describes confirm his former results, which M. Wurtz had questioned.—Dissociation of gaseous iodhydric acid in presence of an excess of one of the elements, by M. Lemoine The most important result is the stability which this excess gives to the combination; in mixing iodhydric acid with increasing quantities of hydrogen the quantity of iodhydric acid dissociated diminishes about half. Still the character of the dissociation seems always to subsist, whatever the inequality in the atomic proportions. This influence of *mass* in dissociation is in accord with several other known facts.—On the dissociation of ammoniacal salts in presence of metallic sulphides, by MM. De Clermont and Guiot.—On the employment of fluoride of bromine as a dehydrating agent, by M. Landolph. He gives several examples of its action (with camphor, &c.).—On the ordinary presence of copper and zinc in the human body, by MM. Raoult and Breton. They give the results of a judicial investigation made by them in 1874. 700 grammes of (moist) liver of a man who had died after an operation for stone, gave 2 milligr. of copper and 7 mgr. of zinc; 400 grammes liver of a consumptive person gave 6 mgr. of copper and 12 mgr. of zinc. To prove poisoning, it should be shown that the quantities of copper or zinc found in a body are greater than the maxima in normal conditions.—On the determination, in weight, of atmospheric ozone, by M. Lévy. This relates to a supposed influence of platinum on arsenite of potash, which, however, was not manifested in the conditions with which M. Lévy operated.

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THURSDAY, JULY 19, 1877

THE "INFLEXIBLE"

IN our last number we sketched in outline the scientific principles and considerations which lie at the foundation of the important question now at issue regarding the *Inflexible.*

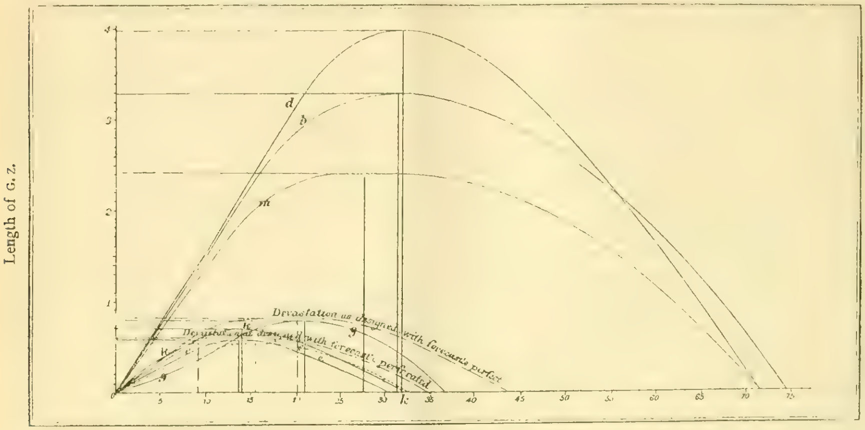
We now proceed to consider the case as set forth in the papers which have since been presented. The fact of a Committee having been appointed to investigate it is no reason for our passing over in silence these papers, which have been already laid upon the table of Parliament expressly to disseminate the information contained in them. We shall not, however, seek to trench in any degree upon the duties undertaken by the Committee.

The first question to be asked is the vital one—What stability is claimed by the Admiralty for the *Inflexible* in the condition contemplated by the *Times* and Mr. Reed, and, as is now made perfectly clear by the papers, by the Admiralty office itself when the ship was designed, a condition namely in which the unprotected ends were so far injured as to cease to contribute stability to the ship?

Has the ship that range of 50 deg. which, as we saw last week, the Committee on Designs, as a result of scientific consideration laid down as necessary for a mastless ship exposed to the action of the sea? Has she even that 39 deg. which is the very lowest amount ever mentioned as compatible with safety? And, it must be further asked (since "range" alone is a most insufficient guide to safety), is the *amount* as well as the *range* such as the settled science of the subject would demand? None of these answers lie on the surface of the Parliamentary papers, and some of them are not to be found there at all; but by what Dr. Schliemann would call "the pickaxe method" of investigation, we can get at part of them, and infer the remainder with all necessary certainty. The most difficult and unfortunate part of the inquiry is due to the fact that the *apparent* answers that do lie on the surface are not the true ones, although they bear every outward semblance of being so. The scientific world is turning to these papers for one piece of information before and beyond all others, namely, this very stability of the *Inflexible* with "unarmoured ends giving no stability;" and when they get to p. 10 there they find it plainly set forth, in tabular form, and in full detail: "Maximum stability 6,532 foot-tons; angle of maximum stability 13½ deg.; range 30 deg.;" and reading this they begin to say to themselves that although the ship is, on the showing of her constructor, below the standard of safety which everybody has either laid down or accepted, yet that she is not so alarmingly deficient as they had supposed from the pronounced tone of her critics. Give her, say they, the benefit of the cork (in which the whole Admiralty has so suddenly acquired extraordinary faith), and although she would then be somewhere near the recognised limits of safety, and would be liable to the cork being more or less rapidly got rid of under shell fire, still the case is not so very bad after all. But unfortunately on reading further they come on the following page upon an ugly foot-note by Mr. Barnaby, bearing date June 25—one week after the occurrence of the *Inflexible* debate in Parliament—and in this foot-note is disclosed the fact that the figures which we have quoted, and which are given at p. 10 as expressive of the case when the unarmoured ends give no stability, turn out to be the figures applicable to the case when the cork is in place, and when the unarmoured ends are contributing the stability which arises from the cork! This is not clearly shown on the surface of the foot-note, but it has to be discovered in a roundabout manner. It can, however, be reached in this way. The foot-note implies that the cork was allowed for in the calculations of a certain curve given on the sheet of curves appended (Curve *e* in our present diagram), and by comparing this curve with the figures given on p. 10, we find that they express the same state of things, and thus we arrive at the fact that these figures likewise include the stability due to the cork. We do not propose to offer any comments upon this, except to say that we cannot help thinking that ninety-nine persons out of every hundred who read these papers will be completely mystified and misled by such an arrangement of the information, and more than all by the distinct employment of words which indicate that the cork is unaccounted for, when, in point of fact, the cork is accounted for.

By the means thus indicated we arrive at the fact that the amount of stability previously referred to and quoted is the amount which alone the ship was believed to possess when the First Lord of the Admiralty made his statement in Parliament, and declared that the Board were in every way satisfied with the vessel. It follows that the First Lord of the Admiralty was satisfied that the *Inflexible* should have a righting lever (G Z) less than six-tenths of a foot long with the cork in place, and of course with very much less in the event of the cork being blown out. We are not able to state—for the Admiralty does not give the information—how much stability the constructors and the Admiralty actually believed the ship to possess in the case under notice (viz., the case of the unarmoured ends really contributing no stability), but it is impossible to peruse these papers and to come to any other conclusion than that unless this information was advisedly kept back it ought to have been given. We cannot ourselves, of course, profess to say how much of the measure of stability which we have quoted was due to the cork, but we are able to approximate pretty closely to it by aid of the diagram which Mr. Reed prepared, and which is printed with the papers. That diagram Mr. Barnaby has marked "wrong," and no doubt it does differ a little from Mr. Barnaby's own diagram in its estimate of the amount of stability with the cork in. Indeed Mr. Reed only put his curve of stability forward as approximate, and as being in his opinion "pretty near the truth;" but this diagram may be taken as a sufficient approximate indication of the amount of stability actually due to the cork itself, and by taking that away from the Admiralty diagram we get what the Admiralty themselves would probably allow to be the state of the ship with the cork out. We have performed this operation, and we find that the *Inflexible* is, with the cork out, left almost absolutely without any stability whatever, even when she is judged by the diagram which the Admiralty themselves supply, and upon which they rested when the first Parliamentary debate took place.

Subsequently, that is on June 25, Mr. Barnaby put forward a memorandum (the foot-note before referred to) showing that his own previous diagram might be extended a few degrees by introducing such considerations as that the four guns had been increased by 20 tons each (in a ship of over 11,000 tons !), that the armoured deck was to be broken through to let the cables be stowed lower, and that an allowance might be made for immersed materials ; and by these means some little show of a curve of stability, even with the cork out, is obtained ; but the amount of it must be exceedingly small, as the range claimed is only 17 deg., and the curves which we give in the accompanying engraving indicate by analogy how very small the *amount* of stability must in this case be with so extremely limited a range. We feel bound, however, to demur to any such change in the Admiralty calculations, both because of the doubt which must inevitably rest on the alteration of calculations made to meet a public inquiry already commenced, and because the actual changes made in this case are in themselves improper. It is improper to make new allowances now ostensibly for the enlarged guns, because still larger guns were originally contemplated in the design, as the printed Papers clearly show ; it is improper to alter the cables to meet the present state of things, as they were obviously wisely placed in the first instance ; and it is improper for the sake of a small nominal increase in the apparent amount of stability of this ship to introduce novelties of calculation which will defeat all comparison now and hereafter between the *Inflexible* and other ships, and which will absorb into the substance of the calculations for this one ship those small outlying margins which together make up the dividing ground between safety and risk. For these good and sufficient reasons we stand upon those calculations on which Mr. Ward Hunt rested when he addressed Parliament on the subject, and we are obliged to state that, according to those official calculations, the *Inflexible* is practically without stability when the unarmoured ends have ceased to furnish any.



Range of Stability

DESCRIPTION OF CURVES.

		Angle of Max. Stab.	Max. G. Z.	Range.	Metacentre above C.G.
		°	Feet.	°	Feet.
b	Ship complete, Cork in place	31·2	3·28	74·3	8·25
d	As in *b* but in light condition	31·7	3·935	71·5	8·53
e	Fully equipped ; ends riddled	13·5	·568	30·0	2·0
f	As in *e* but coal between decks (800 tons) removed ...	15·4	·534	32·2	3·09
g	As in *e* but in light condition	20·8	·794	36·8	2·22
k	As in *e* but supposing the water in ship when upright locked	13·9	·705	32·0	
m	As in *k* but supposing main deck kept free of water ...	27·4	2·42	71·5	

The next question which arises is, how is the ship circumstanced with the cork in? and the answer to that we have incidentally had before us already. Even by introducing such considerations as Mr. Barnaby adduces in the foot-note he only claims for his curve of stability with the cork in a range of 35 deg., and with the range so increased the righting lever G Z cannot much exceed six-tenths of a foot in amount. It will be easy for the reader of these remarks to imagine, without our assistance, the slight alteration of curve *e*, to which we are here pointing, and when they bring the eye down from the large levers of stability indicated by the curves *d*, *b*, and *m*, to the curve *e*, even when thus enlarged, they will see what a striking difference there is between the ship with her unarmoured ends intact and the ship with those ends broken into by the sea. The fact is that even in this state with the curve of stability extended by the devices to which we have already objected, the ship is in an unsafe condition when judged by the only standards that at present exist for our guidance, viz., those laid down in the Report of the Committee on Designs of 1871.

We have now almost exhausted all that the Parliamentary Papers contain on the essential question at issue, but before concluding this part of our subject we must advert to the case of the *Devastation*. The *Devastation*, as designed, had an unarmoured forecastle, which extended down to within about a foot of the water's surface, and with this forecastle perfect, she had a range of stability of 43½ deg. Mr. Barnaby alleges that with the forecastle perforated, her range of stability would be brought down to 35 deg., and that the maximum angle of stability would be 9 deg. It seems to us that the curves given show something more and something different to that which the table in the text gives. They show something more, because it is clear on inspecting that one which relates to the *Devastation* with forecastle perforated, that although the maximum appears to be reached at 9 deg., there is no perceptible diminution in the magnitude of the stability for nearly another 9 deg. The curve seems to be parallel to the base line, from 9 deg. up to 16 or 17 deg., and the state of the *Devastation*, therefore, is totally different from that which might be expected from the announcement in the text, that 9 deg. is the angle of maximum stability. But even allowing all the stability which the *Devastation* is shown to possess by these curves, we are ourselves disposed to believe that when this ship was altered, the alteration should have been so made as to add to the stability of the ship when the unarmoured bow is perforated. Now the Reports of the Committee on Designs show that the changes made after Mr. Reed left the service were of a kind which, while adding materially to the stability of the ship under ordinary circumstances, burdened her with a large amount of top weight, which, together with the lowering of the armoured freeboard, must have diminished the stability with the bow perforated. In view of this consideration, we should much like to have seen the *Devastation's* curve of stability as it would now be with the unarmoured bow perforated, for we are disposed to think that the production of that curve would disclose a much less satisfactory condition of things as regards stability in the *Devastation* as she now is, than existed at the time of her design; and, as we have already intimated, this ship appears to us not to have possessed even at first all the stability desirable. We are aware that the Committee on Designs expressed a contrary opinion, but we doubt if they duly considered the state of the vessel with the forecastle perforated, and we believe that if they had they would probably have looked less complacently upon the change that was made than they actually did. If we are right in this view of the *Devastation*, Mr. Reed must no doubt bear the blame of having cut this ship's stability somewhat finer than it should have been, and all that could be admitted in mitigation of the fact is that which he would probably plead, viz., that this ship was the first great monitor of the kind that had ever been designed; that calculations of stability at considerable angles of inclination had not then come into vogue; and that he left the service long before the *Devastation* was completed for sea; and therefore that it was not from his hands that she passed into actual service.

But it is no part of our business to distribute blame in such matters, and all that we can say on the point is that we are not satisfied even with the *Devastation*, and that whatever deficiency of stability she might have possessed as originally designed, that deficiency must have been made worse by the modifications which the ship has since undergone.

We must now say a few words upon the question of the cork, and how far it may really be relied upon for giving to the *Inflexible*—not a safe measure of stability when the unarmoured ends have ceased to furnish any otherwise, for we have seen that with all the cork in place the stability is still too little for safety—but some sort of chance of escaping that capsizing which must befall her at sea with the cork gone. The great advocate of cork is the present Director of Naval Ordnance, Admiral Boys; but if the arguments which he sets forth in these printed papers are in truth the foundations on which the hope of the Admiralty is based, their confidence must be very ill-grounded indeed. Admiral Boys is good enough to set forth in a formal manner the reasons which induce him to think the cork can be depended upon. There is one at least to which science at once supplies an answer.

He mentions "the difficulty of striking a ship at or below the water-line, particularly one of the *Inflexible* type, that will scarcely ever roll." Scarcely ever roll! A more astonishing, and may we say a more utterly groundless remark, was probably never made upon a serious subject. It has been established beyond all question or cavil that one of the most fruitful causes of rolling is great stability, and that mastless ships of great stability will roll more than any other. Now, in her uninjured state, which is the state in which she will enter upon an action, the *Inflexible* will have very large stability, and may be expected to roll heavily in a seaway. A glance at our engraving this week will show that the length of the righting lever, G Z, is sometimes over three feet, and in one case (with certain stores consumed) nearly four feet, whereas not one of all the typical curves given in our last number gives a G Z that much exceeds three feet, while all but one are much below that. It is true that the admission of water at the ends may have a perceptible effect in diminishing the rolling, but it would be premature to presume any great results in this respect. And apart altogether from the rolling of the ship, every seaman, including Admiral Boys, must be well aware that the ends of the steadiest ships in the world get much exposed by the mere falling away of the water in a seaway. With regard to Admiral Boys other assumptions they are more out of our province, but we may remark that the enunciation of such views by a high officer of the Admiralty is calculated to fill common minds with astonishment and apprehension. Naval architecture instead of being a complex and difficult branch of science, would be of all arts the most simple and easy, if all that had to be done were to keep safe the under-water parts of ships that never roll; to encounter projectiles that canno fulfil the object for which they are employed; and t engage vessels armed in the most suitable manner, and managed in the most suitable way, to let us escape!

The conclusions we have arrived at are, that the *Inflexible* is not a safe ship for battle in her present state that the objections brought against her have been much too lightly treated; and that the disclosure of her condition, with the circumstances that have followed it, have excited just surprise and dissatisfaction. The subject must be anxiously watched through its future stages.

THE NEW METEOROLOGICAL COUNCIL

THE final stage of the labours of the Treasury Committee, to which we have made frequent reference, has now been reached. The Royal Society has been appealed to to nominate the new council; they have done so, and the Government has accepted the nominations, which are as follows:—Prof. H. J. S. Smith, Savilian Professor of Geometry in the University of Oxford and Keeper of the University Museum (Chairman); Prof. Stokes, Lucasian Professor of Mathematics in the University of Cambridge, and Secretary of the Royal Society; Dr. Warren de la Rue, Mr. F. Galton, and Gen. Strachey, Member of the Indian Council. In addition to these there is Capt. Evans, the Hydrographer of the Navy, as an *ex-officio* member.

The new Meteorological Council, then, like the old Meteorological Committee, is composed of Members of the Royal Society, who severally hold distinguished positions in special departments of science, and who collectively represent considerable administrative ability. The addition to the new Council of two distinguished mathematicians and physicists, such as Professors Smith and Stokes, will be generally regarded with satisfaction, particularly when it is considered that it is to the mathematician and physicist that meteorologists must always look for information and guidance on many matters affecting the intricate and difficult problems with which they, in the position the science has attained, must now deal.

It is, however, matter of general surprise among meteorologists, or we should rather say of wide-spread regret, that the New Council will resemble the old Committee in having no meteorologist upon it. The omission, so far as concerned the Meteorological Committee, was a serious one, and led to mistakes; so far as concerns the new one it will be well if it does not seriously mar its usefulness and retard the foundation of the future science of physical meteorology. At the same time it is only just to point out that because the science is of the future, the choice of the Royal Society was small, and that considerations not on the surface may have had to be borne in mind. However this may be, there is no doubt that the Royal Society and the new Council have accepted a great responsibility, and that the action of the latter will be most keenly watched. The Royal Society, in a report to the Government, has stated:—

> "The Council of the Royal Society is of opinion that the most practical method of advancing meteorology is to endeavour by research and experiment to place that science on a firm basis. They are also of opinion that this can be done only by the devotion of the time of scientific men to the necessary research and experiment."

Men of science, therefore, will be justified in looking both for research and experiment from the new council in addition to the dreary piles of observations which have cumbered all scientific libraries for the last half-century.

And here is the rub. Will the busy—not to say already over-worked—members of the Council adopt this "practical method," and conduct researches? or do they propose to content themselves by going into the market with the 1,000*l.* which is given for *research*, and, be it remarked, not for mere *observations?* In the latter case it is to be hoped that their advances will be met in no narrow spirit; for if the new council only fosters research and experiment, it will be a great gain.

While, on the one hand then, we have a right to expect results of a high order from the new Council, on the other we are glad to see they are to be no longer an unpaid body. Besides the 1,000*l.* devoted to research there is another 1,000*l.* devoted to the payment of the members. This sum is to be spent partly in retaining fees and partly in payment for attendance.

The vote asked for the present year and agreed to on Tuesday is 10,000*l.*, and the Secretary of the Treasury then stated that the Committee had recommended an expenditure of 4,000*l.* a year by the Meteorological Council, and, in the judgment of the Treasury, the recommendation was one that ought to be adopted. A supplementary vote will be asked for this at an early date.

As regards the meteorological societies, on whom must devolve the practical working out of the large problem of the comparative climatology of the various districts of the United Kingdom—the working out of this problem being beyond the scope of the operations of the New Council just as certainly as it is beyond the resources originally placed at its disposal—we cannot but suppose that the Government have, in handing over the administration of the meteorological grant to the New Council, made provision that a portion of the additional 4,000*l.* will be spent in adequately aiding these societies in doing important national work which they are in a position to do so economically, and which, judging from the past, they can do so effectively.

This now seems to be the Treasury view, for in the warm debate very properly raised by the Scotch members in favour of the claims of the Scottish Meteorological Society, Mr. W. H. Smith stated that, as to the tests that ought to be applied in such cases, special regard ought to be paid to two points. The object to be attained ought to be distinctly national, and not one in which particular individuals or classes were concerned, and security ought to be taken that the persons who sought assistance were contributing largely to promote the object in view. There is no doubt that the Scottish Society satisfies both these requirements.

The Chancellor of the Exchequer also stated that it would be for the Council to consider how far they could avail themselves of the services of the Scottish Meteorological Society in the conduct of their business, and on what terms that assistance should be rendered.

The Council have lost no time in entering upon their duties, and it is devoutly to be wished that some sign may soon be given that if its constitution is not what was generally looked for, it is still well qualified to discharge its functions and to merit the confidence of meteorologists, although they have had so little to say to its appointment.

THE "POLARIS" EXPEDITION

Narrative of the North Polar Expedition U.S. Ship "Polaris," Captain Charles Francis Hall, Commanding. Edited under the Direction of the Hon. G. M. Robeson, Secretary of the Navy, by Rear-Admiral C. H. Davis, U.S.N. U.S. Naval Observatory, 1876. (Washington: Government Printing-office, 1876.)

THIS is a handsome record of one of the most memorable, and in some respects most successful of Arctic expeditions. Though dated 1876, a note dated March, 1877, is prefixed, stating that the concluding chapters have been prepared by Prof. J. E. Nourse. We have already (vol. viii., 217, 435 and *passim*) given so full details concerning this expedition that we need do little more now than notice the publication of this record by the U.S. Government. It contains a full general narrative of the expedition drawn up not only from the official records of the responsible officers, but from the diaries kept by many of the subordinate officers and men, many of the latter being unusually intelligent. Indeed it formed part of the instructions to the expedition in its outset that as many of the officers and men as were able should keep diaries, which were to be handed over to the U.S. Government on the return of the expedition, a praiseworthy feature, we think, which might be advantageously copied by all similar expeditions.

Captain Hall himself is spoken of justly in the narrative in the highest terms of praise. His enthusiasm for Arctic exploration had become almost a religion with him, and had he lived there seems little doubt that much more would have been accomplished than even there was. He had qualified himself by two long residences among the Esquimaux for enduring all the hardships incident to Polar exploration; and while his main aim was geographical discovery he had a sufficient knowledge of and love for science to induce him to do all in his power to look after its interests in connection with his expedition. To quote the work before us, Hall "possessed judgment and sagacity altogether too large and comprehensive not to be fully alive to the importance of its promotion; and not to know that every accession, whether of law or fact, to its domain, tended to the benefit of mankind." We believe that the narrative of Hall's second residence among the Esquimaux will shortly be published for the first time.

The present work will correct some misconceptions that became current at the time that the news of the fate of the *Polaris* expedition reached this country. It was stated, for example, that the ship was not well fitted for her work; but the fact is that everything was done to strengthen her and otherwise adapt her for the special work she had to do that the United States naval authorities could suggest. Hall himself said that no better equipped expedition ever set out for the Pole. The expedition was in every sense a government one, 50,000 dollars having been appropriated for it, and it was governed by the naval discipline of the United States. True, this latter point does not seem to have been rigidly carried out, Hall himself not having been a naval officer, and perhaps a little too soft to be so strict as he ought to have been. Explicit, but sufficiently elastic instructions were given him, and the scientific instructions prepared by the United States National Academy are given in the Appendix. The latter are exceedingly detailed and carefully drawn up, and embrace every department of science. The scientific results of this expedition are, we believe, of very great importance, and we are glad to learn that a portion of them, at least, have just been published by the chief of the scientific staff, Dr. Emil Bessels.

The idea of the expedition was due solely to Capt. Hall, and it was only through his enthusiastic agitation that the United States Government were persuaded to equip it. His death was a great loss to the cause of Arctic exploration, and we may say to science; and it is a relief to find that after rigid inquiry on the part of the Government it has been concluded that his death arose from purely natural causes. Our readers may remember that a handsome tablet was placed at the head of his grave by our own recent expedition.

The present volume, we have said, is a handsome one, and compares favourably with the unattractive blue-books issued by our own Government in similar cases. There are a large number of attractive illustrations and maps, the former, however, executed by a very roundabout process; they are wood-engravings painted in oil from original sketches by Mr. Emil Schumann and Dr. Bessels, photographed on wood and engraved. One may be inclined to fear that their truthfulness will be apt to suffer during this long process. A very fine life-like portrait of Capt. Hall forms a frontispiece.

POLLUTION OF RIVERS

A Treatise on the Law Relating to the Pollution and Obstruction of Watercourses, together with a Brief Summary of the Various Sources of Rivers Pollution. By Clement Higgins, M.A., F.C.S., Barrister-at-Law. (London: Stevens and Haynes, Law Publishers, Bell Yard, Temple Bar, 1877.)

THE pollution and obstruction of rivers by sewage and the refuse of manufacturing processes is, in a country like this, densely populated and depending mainly on its manufactures, a matter of the gravest importance, justifying indeed, a saying of Earl Beaconsfield's, which has met with a good deal of ridicule, that the motto of his government should be *sanitas sanitatum omnia sanitas.* We have on the one hand to preserve as rigidly as possible the purity of our streams, and on the other to interfere as little as can be with those manufactures which are so great an element in the production of our national welfare.

It is now some years since the Government issued a Royal Commission to inquire into the best means of carrying out the problem, and that the matter is now in a fair way to solution is mainly due to the labours of the late Commission, of which Dr. Frankland was the chemical member.

The five Reports presented by it to Parliament, take rank, indeed, as a classical research into the subject, and have an interest to countries other than our own. The Rivers Pollution Act of 1876 is based on the recommendations of the Commission, and it is not too much to say that without them legislation would have been impossible.

The Act in question constitutes four classes of offences as follows:—To pass into any stream (1) any solid matter so as to interfere with its due flow, or to pollute its waters;

(2) any sewage matter; (3 any poisonous, noxious, or polluting liquid from any manufacturing process; and (4) any poisonous, noxious, or polluting solid or liquid matter from any mine.

Mr. Higgins justly remarks that "the successful working of the Act will much depend upon the meaning of the word '*polluting*' as therein used, by those with whom its interpretation rests." In order to understand the drift of this remark it is necessary to observe that the Act of 1876 virtually gives no standard of purity, though the Commission of 1868 recommended an extensive and somewhat stringent list of standards. We think that on the whole the Act is right in the omission, as a suggestion made by Mr. Crookes in his evidence before the House of Lords in 1873, namely, "that the river itself should be the standard of purity, and that no liquid should be allowed to be sent into a river if the liquid contains a greater percentage of impurity than the river itself," seems to be a very feasible standard and one easily and quickly referred to. Again, as Mr. Crookes pointed out, the standard would naturally improve, as nothing worse than the river at any given point would enter it, whence in the course of nature amelioration would ensue, while the process being gradual would give the manufacturer or township time to improve their waste or sewage, and one of the most disastrous sources of trouble the injury to the water-course from the casting into it of solid refuse would be at once prohibited; as would pollution by actually poisonous matter, such as arsenical and other liquids.

It appears to us that guided by *competent* chemical evidence there ought to be no difficulty in obtaining legal decisions as to the polluting or harmless character of any liquid that may be called in question, while as to solid matters, of any kind whatever, the mere fact of their entry into a stream ought to be an offence without reference to their character. On the whole we think the act, though perhaps partaking too much of the "permissive" character, which is so prominent a feature of modern legislation, to be one which, if conscientiously used with due consideration to the facts of each individual case ought to work great good. In the race for wealth we are perhaps too little apt to think of the future. The brooks and running streams like the land we live on are not ours to do as we like with, but like an entailed estate are only held in trust for the next heir, and like national or family honour should be handed down to posterity pure and unsullied.

Mr. Higgins has devoted great care to his treatise on the Act, and his chemical training has evidently stood him in good stead, the numerous references to cases bearing on the various points show a laborious study of the legal aspects of the case and will add greatly to the value of the work in the eyes of the legal profession, for whose information it is primarily intended.

R. J. FRISWELL

OUR BOOK SHELF

The Cradle of the Blue Nile; a Visit to the Court of King John of Ethiopia. By E. A. De Cosson, F.R.G.S. Two vols. With Map and Illustrations. (London: John Murray, 1877.)

ALTHOUGH Mr. De Cosson did not go over any new ground in his tour, and although he was unable even to carry out his original plan, we are sure that most readers will find much that is new and certainly interesting in his volumes. He went slowly southward from Massowah to Lake Tzana, north-west to the lower Bahr-el-Azrek, down the Nile to Berber, and across to Saakim. He won the favour of King John, of whom he speaks as an able, well-meaning ruler, and was thus able to see much of the life of the people, and learn much of the antiquities and the character of the country that otherwise he would have missed. To any one wishing to obtain an attractive account of the past history and present condition of Abyssinia, we strongly commend Mr. De Cosson's work.

The Tiber and its Tributaries, their Natural History and Classical Associations. By Strother A. Smith, M.A. Map and Illustrations. (London: Longmans and Co., 1877.)

THE idea of this work is, we think, a happy one, and its execution successful. The object is to gather under one head everything of interest relating to the Tiber. This has necessarily involved a great amount of research, and the result will be welcomed both by the student of history, the "scholar," and the geographer. Considerable space is devoted to the inundations of the Tiber, and also to its birds and its fishes. Two nicely-coloured plates are devoted to the muræna, the mullet, the lamprey, and the sturgeon. The Tay, at Perth, we should inform Mr. Smith, is no more an "estuary" than the Thames at London Bridge, unless the word is applied to all that part of a river reached by the tide.

A Short Account of the Principal Geometrical Methods of Approximating to the Value of π. For the Use of Colleges and Schools. By the Rev. G. Pirie, M.A. (Macmillan, 1877.)

Elements of Geometry Based on Euclid. Book I. For Elementary and Middle Class Schools. By E. Atkins, B.Sc. Collins's School Series. (Glasgow: Collins, 1877.)

Takimetry. Concrete Geometry in Three Lessons. Accessible, Inaccessible, Incalculable. Translated by D. W. Gwynne, M.D., from the French of E. Lagout. (Glasgow: Collins, 1877.)

THE little pamphlet first named does not attack the problem from the circle-squarer's point of view—the use of the word "approximating" sufficiently points this out—but gives an interesting account of what was done for the question between the times of Archimedes and Huyghens. A few elementary propositions lead up to what was attempted by Willebrord Snell ("Cyclometricus," 1621) and elegantly effected by Huyghens. Mr. Pirie's object is to correct what he deems a defect in our present works on Trigonometry, and to supply a few simple propositions "on the threshold of the subject." We can recommend the book as one suitable for being put into the hands of sixth form pupils. A few references are supplied to fuller sources of information upon the quadrature of the circle.

Mr. Atkins's book seems to differ but little from the ordinary forms of Euclid as now printed. One feature is the addition of short side-notes drawing attention to the objects of the successive steps of the construction and proof. There is a short collection of sixty exercises grouped under the propositions upon which they depend. Some of these appear to us wrongly placed, and a few incorrectly printed. The work is neatly got up and of a handy form.

If all that is said of takimetry by its admirers be true a revolution in mathematical instruction may be speedily expected. "With one hundred lessons of takimetric instruction any one can very well learn geometry, algebra, arithmetic, and mechanics." "The classical geometry of Euclid *disguises its object, its utility*, and thus, for a considerable time, yields a barren and discouraging result, whilst takimetry is able, on the other hand, to produce the miracle of an astonishing progress." In the Fundamental Takimetry (introductory to Takitechny) objects are classified into square, round, pointed, and truncated forms. The three lessons of Takimetry are (1) equivalence; (2) resemblance; (3) the three squares of a right-angled triangle (*i.e.*, "Euc.," i. 47). The subject requires only three lectures, each of an hour's duration. Amongst the subjects for measurement are the accessible, the inaccessible, and the incalculable (*i.e.*, those which depend upon the circle). There is much that is good in this book, though in its present form it is overweighted with a mass of extraneous matter. By aid of the prettily-coloured figures (there are models, also, we are told, to accompany the book) a considerable knowledge of mensuration, we think, might be imparted even to dull boys. We could take exception to the translation in many places.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Museum Reform

"J. P." suggests, under the head of "Museum Reform" (vol. xvi. p. 183), the idea of a conference of museum keepers, out of which a permanent union among museum officers might result. I am of opinion that this idea is an excellent one, and that the administration of the museums of all countries would gain a great deal if an opportunity to museum officers were offered to interchange their opinions and to communicate to each other their different practical experiences. Perhaps some arrangements and rules might *generally* be accepted, as, *e.g.*, to labelling specimens, exchanging duplicates, publishing annual reports in a journal *ad hoc*, &c. A. B. MEYER

Dresden, July 9

Koenig's Tuning-Forks

THE letter of Herr Koenig inserted in NATURE (vol. xvi. p. 162) did not come under my notice till July 8. On October 27 last year I counted all the 64 sets of beats in Herr Appunn's tonometer, one through 15, the rest through 20 seconds with a pocket chronometer which was gaining less than 4 seconds a day, and found every set of beats perfectly true. The perfection of the consonances, more than 80 of which I tested mechanically, by observing the beats that arose on flattening one of the two consonant notes, seemed to me to eliminate all possible error of irregular counting. The suggestion now is that the beats were perfectly regular and uniform, and that no exception need be taken to my counting, but that Herr Appunn's pendulum was originally incorrect, to such an extent that what appeared to him 80 beats in 20 seconds, were only 79·27, and that my chronometer was not sufficient to detect the error. If this were the case all the numbers on Herr Appunn's tonometer would have to be reduced by as nearly as possible 9 in 1,000, which would make them agree with Herr Koenig's. I shall therefore have to re-test the tonometer with a larger chronometer and if possible count each set of beats for a longer time. I shall not be able to undertake this examination at present, but I shall not neglect doing so, and will inform you of the result. It is right, however, to say that on July 9 and 10 I received communications from Prof. McLeod showing that his improved instrument for counting vibrations gave results almost exactly agreeing with Herr Koenig's numbers. The marked difference of Herr Appunn's and Herr Koenig's numbers will I hope lead to such an examination of the subject as will result in an accurate determination of pitch that can be generally accepted. ALEXANDER J. ELLIS

Kensington, July 11

P.S.—I have this morning received a letter from Herr Appunn, in which he tells me that the letter of Prof. Helmholtz, quoted by Herr Koenig, was received eleven or twelve years ago, and that the error of Herr Appunn's pendulum there pointed out was corrected more than ten years ago. He also refers me to pp. 46-7 of Prof. W. Preyer's tract "Ueber die Grenzen der Tonwahrnehmung," Jena, 1876, in which, by a calculation there detailed, Prof. Preyer shows that the absolute pitch of two of Herr Koenig's forks, which should have been 128 and 256, were 129·1 and 258·2; and says that "the determination is as exact as possible, so that the first decimal place can be fully trusted." I made another fork to be 258·4, and I know by comparison of several specimens that Koenig's forks do not always agree within more than ·2 vibrations. A. J. E.

July 16

On a Fish-sheltering Medusa

WHILE collecting some three weeks since on the south shore of Killary bay in Connemara, I observed that out of a number of the common *Aurelia aurita* moving about in a rocky inlet below me, one was invariably accompanied by a small fish, of about an inch or an inch and a quarter in length, which had established itself inside of the hemispherical disc.

Occasionally the Medusa turned in its pulsations, so as to bring the umbrella undermost, when the fish would shoot hastily out, but the Medusa had no sooner righted itself, than the fish returned, and seizing its opportunity, swam in between the marginal tentacles, and close up to the fringes of the actinostome, remaining distinctly visible through the pellucid disc.

I afterwards noticed several other Aurelia similarly attended, but was not able, unfortunately, to identify the fish, which invariably darted off at the most distant approach of a landing-net—it appeared, however, so far as I could judge, to be the young of one of the larger species. Perhaps some of your readers could contribute suggestions on that point.

Associations of a similar character have, I know, been frequently observed in the case of the Physalidæ and other Acalephæ, but not, so far as I am aware, in connection with this species. E. LAWLESS

The Earth and Moon

I HAVE only now (July 12) noticed Prof. Tait's remark respecting a sentence, or rather half a sentence, which he quotes from an article of mine in the *Cornhill Magazine* for June. It runs thus: "What mathematicians call the moving force exerted by the earth on the moon is eighty-one times greater than the corresponding force exerted by the moon on the earth." This admits of an interpretation implying gross ignorance on my part—ignorance, viz., of the fact that the moon pulls the earth just as strongly as the earth pulls the moon It also admits of an interpretation accordant with fact, for the moving force exerted by the earth on each unit of mass in the moon is eighty-one times greater than the corresponding force exerted by the moon on each unit of mass in the earth. I do not think anyone is likely to believe that I made the mistake imputed to me by Prof. Tait, in this instance, any more than that I made an equally absurd blunder which he attributed to me in your columns several months ago, or that he himself made the ludicrous blunder attributed to him (in jest) by my humorous friend, Prof. Nipher, of St. Louis. But as a mere matter of fact, I may point out that the half-sentence quoted by him is completed by a half-sentence leaving no doubt as to my real meaning, and is immediately preceded by the statement that "the moon pulls the earth just as strongly as the earth pulls the moon."

London, July 12 RICHD. A. PROCTOR

Blue and Yellow Crocuses

REFERRING to Mr. W. B. Tegetmeier's letter in NATURE, vol. xvi. p. 163, I can say that I once had a pony born and bred on Dartmoor, which had never seen oats until it came into my father's stable in the fourth year of its age, and it refused them. We induced it to eat oats by mixing them with hay and gradually reducing the quantity of hay until the oats predominated.

Penzance, July 10 THOS. CORNISH

Japanese Mirrors

MORE than eleven years ago, in February, 1866, I published in *The Reader* (since extinct), a letter giving, I venture to think, a complete explanation of the phenomena exhibited by certain Japanese mirrors (through a mistake as to their nationality I called them Chinese); and as your readers appear to be unacquainted with this, perhaps I may be allowed to reproduce the substance of my former letter. In order to ascertain whether any variations in the form of the surface of the mirror, which was very slightly convex, affected the question, I looked for any distortions that might be produced in the image of the ground-glass globe of a gas lamp, as the point of incidence moved across that portion of the polished surface on the back of which was a raised figure. Let A and B be two lines on the surface immediately over the two edges of such a figure. Then as the limb of the image approached A, it became flattened; when it had passed A it expanded to more than its original size; indeed between A and B the image was sensibly larger than when viewed from any other part of the mirror. When the limb approached B it was again flattened, and beyond B it resumed its original dimensions. This clearly proves that the portion of the surface of the mirror between A and B was, if not actually plane, at least less convex than the rest of the surface; and as upon this supposition the figures when thrown upon a screen should appear *brighter* than the rest of the image, which is exactly what occurs in fact, there can be no doubt that this is sufficient to account for the peculiarity in question. In all probability the mirror had warped in cooling, except in the thicker portions where the raised figures existed. J. PARNELL

Hadham House, Upper Clapton, July 6

Printing and Calico Printing

YOUR correspondent, Mr. Henry Cecil, is under a singular misapprehension as to the inventor of cylinder machine calico printing, and the date of its first practical application. Mr. Isaac Taylor was certainly not the originator of cylinder printing; and that art was developed long before he, "in 1855 or 1856 superintended its application at Manchester." Mr. Taylor, it is true, obtained several patents for inventions connected with cylinder-printing—one, I think, for a form of pentograph, and another for the use of thin sheet copper instead of thick cast cylinders of that metal. These, so far as I know, never succeeded in practice, and it is highly probable they brought their gifted inventor loss instead of gain; but that result was not due to "the inevitable compliment of piracy." Who the inventor of cylinder printing was it would be hard definitely to determine. Nearly a century and a half ago a patent was granted for an invention which embodied the leading principles of the modern machine, and from that time downwards the apparatus gradually developed and perfected in the hands of innumerable practical inventors.

THE WRITER OF CALICO PRINTING IN THE "ENCYCLOPÆDIA BRITANNICA"

LOCAL MUSEUMS

THE importance of local museums is gradually but unmistakably forcing itself upon the country. It may take much time to foster any united action, without which any definite progress is very improbable, but year by year is adding to the ranks of those who are wise enough to see and have influence enough to advocate their value as a part of the educational stock-in-trade of the nation. We rejoice to see that Mr. Chamberlain has enrolled himself among their advocates in the House of Commons. On Monday he drew attention to the fact that the public expenditure for the promotion of science and art was exclusively confined to London, Edinburgh, and Dublin. The amount of the estimate this year, he said, for museums, art galleries, and parks in the metropolis amounted to nearly 400,000*l*., and that for Edinburgh and Dublin to nearly 50,000*l*. To those sums the provinces had to contribute twice over. Birmingham contributed about 4,000*l*., and had to find about 8,000*l*. a year besides for her own local art institutions. It might be said with truth that a national collection should be placed in the metropolis at the expense of the nation, but that argument did not apply to the expenditure on the public parks and still less to that which the Bethnal Green Museum involved. He did not complain of such expenditure. It produced most admirable results, adding as it did to the pleasure and happiness of great masses of the people, and tending to elevate and refine their minds. It was, too, in some sort a commercial investment, as it was calculated to enable artisans the better to compete with those of other nations. What he complained of was that the principle had not been carried far enough. He was anxious to see established in every one of our great centres of population and industry museums devoted to art and manufactures appropriate to each particular district. To show how highly these institutions were appreciated in the provinces, he mentioned that in Birmingham the local museum which had been established by private subscriptions was visited annually by 300,000 persons, and as the population of the town was only 370,000, the attendance was immensely greater than was shown by the returns of the number of visitors to our metropolitan institutions. Results equally extraordinary could be quoted from other provincial towns in which such museums existed. He further stated that although provincial communities were at present legally able to tax themselves to the extent of 1*d*. in the pound for the purpose of establishing museums and libraries, in Birmingham all this money went to the free library, and they had therefore no means of establishing an industrial museum.

We are glad also to see that the Government is now alive to the importance of this action, for, although Lord Sandon in his reply begged that the matter might not be pressed upon them at the present moment, he reminded the House of the great advantage which the country derived from the South Kensington Museum, which was now, in fact, a gigantic circulating museum. Almost all the principal objects in the museum, except those of great rarity or delicacy, were sent on their travels at different times through the provinces, and in this way aid was already given to local museums. The country derived enormous advantage from this vote. Local exhibitions were frequent, and loans from the South Kensington Museum for these exhibitions were very numerous. Eight museums had these objects sent to them, and a great deal had been done as the hon. gentleman wished. The South Kensington authorities were anxious to follow that course, but he could not say whether they would be able next year to do more in that direction. Their hands were to some extent tied by the necessity of economy, but the matter would receive the best attention of the Government, and he hoped that next session they might be able to go further.

Of course, neither Mr. Chamberlain nor Dr. Lyon Playfair allowed the subject to drop without pointing out that the British Museum and the National Gallery had no circulating system in operation, that in fact Lord Sandon was quite justified in adopting that line of argument with regard to the South Kensington authorities; but that many of the London galleries and museums were of no use to the provinces. The British Museum, for instance, and the National Gallery were practically of no use except to London, yet every one knew that they contained many duplicates which would be most valuable to the provinces, and the offer of some important pictures was sometimes declined on behalf of the National Gallery. Nor was this all. Dr. Playfair pointed out that in France the Minister of Education was responsible for all the museums, and constantly sent collections into the provinces; but in England, the management of the galleries was, so to say, dislocated, and not under one authority or one Minister. Why did he not go further and point out the recommendation of the Duke of Devonshire's Commission?

THE VELOCITY OF LIGHT[1]

THE correct determination of the velocity of light is a result on which so much in physical science depends that there is good reason for us to give a description of the details of the apparatus used for the purpose of obtaining the exact value. Until the time that M. Cornu undertook experiments with this object in view the generally received value of the velocity in question was 298,000 kilometres per second. This depended on the experiments of M. Foucault, who used a rotating mirror on which the rays of light from cross-wires fell, and while the mirror was in a certain position were reflected by it to a concave mirror at a distance of $13\frac{1}{2}$ feet, having the revolving mirror at its centre of curvature and so fixed as to return the rays of light to the latter, which reflected them to the point of departure. While however the rotating mirror was in rapid motion, a ray of light reflected by it to the distant mirror and back from it, would not, unless the passage of light were instantaneous, reach the rotating mirror until the latter had moved from its position of first reflection, and would not therefore return to the point of departure, but to some point near it, depending on the angle through which the rotating mirror had moved in the time between its reflecting the ray to the concave mirror and the return of that ray. By placing the cross-wires in the principal focus of a convex lens the rays proceed in a parallel beam, and on returning form an image of the wires, removed from the wires themselves, a distance depending on the angular velocity of the mirror and the velocity of light. The cross-wires and their images are rendered visible by viewing them by means of a diagonal reflector of plain glass in front, which at the same time allows sufficient light to pass through to illuminate them.[2]

In 1849 M. Fizeau devised a method differing from that just described by which he measured the time a ray took to travel from Suresnes to Montmartre and back. The apparatus consisted of a toothed wheel which could be rotated with a known velocity, and having the teeth and intervals equal in size. A pencil of rays was sent through the interval between two teeth to a reflector at Montmartre 28,334 feet distant, which caused the ray to return on itself. So long as the wheel is at rest and the rays pass through an interval, they will be returned through that same interval, but when the wheel turns with sufficient velocity a tooth takes the place of an interval before the ray has time to return from Montmartre and get through, and is therefore interrupted. By rotating this wheel faster the next interval will take the place of the preceding one on the return of the ray, which will again get through, and so on passing and being interrupted as the velocity of rotation increases.

It is obvious, then, that if we know the number of teeth on the wheel and the number of turns per second, say at the instant of reappearance of the spot of light after a disappearance, we shall know the interval between the passing away of the ray by the edge of one tooth to its return by the corresponding edge of the next; and this is the time the ray has taken to traverse the distance to the reflecting station and back, and from this the velocity of light follows. From these experiments M. Fizeau obtained a velocity of 315,000 kilometres = 196,000 miles per second.

At that time the velocity of light deduced from the observations of eclipses of Jupiter's satellites, combined with the then accepted solar parallax, was 190,000 miles per second, closely agreeing with M. Fizeau's result; later determinations of solar parallax have given a smaller result than former ones, and consequently the velocity of light deduced therefrom becomes reduced, which again closely agrees with M. Foucault's direct determination.

In the year 1874 the Council of the Paris Observatory, on the proposition of M. Leverrier and M. Fizeau, decided on the execution of experiments for the direct determination of the velocity of light, and offered the use of the scientific apparatus at the observatory for the purpose, together with funds for the construction of the necessary instruments. To M. Cornu was entrusted the execution of the operations; and after due consideration and experimental comparison, he adopted the method of M. Fizeau in preference to the revolving mirror of M. Foucault. A preliminary series of experiments were carried out in 1871 and 1872, between the École Polytechnique and Mont Valérien, a distance of 10,310 meters, giving a result of 185,370 miles per second as the velocity of light, with a probable error of less than $\frac{1}{100}$. M. Cornu then commenced more careful experiments between the Observatory and the Tour de Montlhéry, a distance of 22,910 metres. The principle of M. Cornu's arrangement we have already described, it being the same as that of M. Fizeau, but the details of the apparatus are somewhat elaborate, and in his Memoir occupy seven large sheets of plates; we can, however, to a certain extent describe them. Rays of light from a highly luminous source issuing from a small hole in a diaphragm, pass through a convex lens, and after reflection at an angle of 45 from the surface of a plain piece of glass, are brought to a focus at the circumference of the toothed wheel; the light then traverses an object-glass of fourteen inches diameter, and the parallel rays travel to the reflecting station; here they are received by an object-glass of six-inch aperture, and about six feet focal length, at the principal focus of which is a reflecting mirror of silvered glass. From this mirror the rays are returned to the toothed wheel, where an image is formed coinciding with the original image of the hole in the illuminated diaphragm, the rays if not intercepted by a tooth, pass onwards, and the greater part of them traverse the diagonal reflector of plain glass and an eyepiece beyond, through which the image formed by reflection from the distant station is viewed. So far we have given an outline of the optical part of the apparatus as well as we can without the use of the drawings by which the details can only be made intelligible. We next come to the toothed wheel, and here certain conditions must be fulfilled: first a velocity of rotation must be obtained capable of admitting a considerable number of orders of extinction; secondly, the motive power must be such that the observer can easily control the velocity of the wheel; thirdly, there must be a means of recording the velocity at any instant of time. The motive power is a weight which drives a train of wheels which rotate the toothed wheel, the latter being constructed of aluminium from $\frac{1}{10}$ to $\frac{1}{15}$ millimetre in thickness; wheels of different diameters were used varying from 35 to 48 millimetres. The velocity is recorded on the surface of smoked paper on a roller of about one metre in circumference and half a metre in length, turning on a horizontal axis. The records are made by the action of electro-magnets on light tracers pressing against the surface of the smoked paper. The velocity of the cylinder carrying the paper is such that a line 20 mm. is traced in a second, and during each revolution the tracers are moved on horizontally 15 mm. One of the tracers is put into action at every second by electric communication with a standard clock; a second is put into action at every $\frac{1}{10}$ second by a trembler governed by the pendulum of the clock; the third moves at each fortieth or four hundredth revolution of the toothed wheel, and the fourth is under the control of the observer. Each of the four tracers is continually in contact with the smoked surface of the paper, and so long as it is not moved sidewise by the electro-magnet, traces a continuous line round the cylinder, but on the passing of a current round the electro-magnet, the tracer makes a short mark at right angles to line, and a zig-zag line caused by the vibration of the tracer, back to its original position; the first two lines, therefore, show seconds and tenths of seconds, the third, the instants of completion of forty or 400 revolutions, according to the desire of the operator, of the toothed wheel; a comparison, therefore, at once gives the number of revolutions per second, while on the fourth line are marked the instants of disappearance or reappearance of the light, and the velocities at those instants are then at once known. To make an experiment the aperture in the diaphragm is illuminated by a lime-light or sometimes with sunlight by means of a heliostat. The necessary adjustments in the direction of the rays of light to the distant station are then made by bringing the distant collimator into the centre of the field of the observing apparatus, and the point of light—the luminous echo—is made to accurately coincide with its original at the circumference of the toothed wheel. Particulars of the experiment, as to number of teeth of wheel, direction of rotation, &c., are entered on the paper on the cylinder, and the latter is then set in motion; the observer then sets the toothed wheel going and watches the luminous echo, and on its disappearing touches a key which sends an electric current to the electro-magnet controlling the fourth tracer, which therefore registers the instant the velocity is sufficient to cause a disappearance. As the velocity of the wheel increases, the luminous echo again appears and the key is pressed; a further increase in velocity causes another disappearance and so on to the higher orders, each of which is registered. The velocities at the different instants are read off by a micrometer to $\frac{1}{100}$ of a second.

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[1] "Détermination de la Vitesse de la Lumière d'après des Expériences exécutés en 1874, entre l'Observatoire et Montlhéry." Par M. A. Cornu. (Paris: Gauthier Villars, 1876)

[2] From the experiments of M. Foucault in 1862 a velocity of 298,000 kilometres = 185,157 miles per second was deduced.

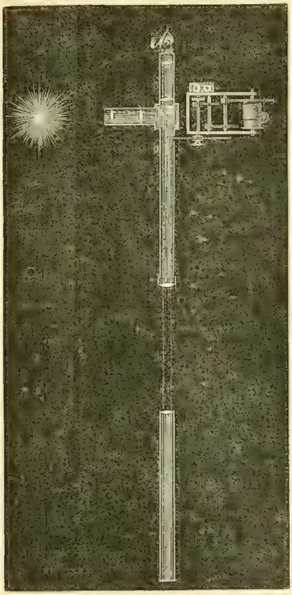

FIG. 1.—Plan of M. Fizeau's Apparatus.

It is obvious that the state of the air must have a great effect on the definition of the luminous echo, and that although the observation appears extremely simple still there may be large errors due to irregular refraction of the air, causing a motion of the point of light, and a large amount of patience must be required. Two careful surveys showed the distance between the two stations to be 22909·77 metres, and the mean velocity obtained from a large number of observations after the various corrections were made was 300,400 metres per second of mean time.

The Memoir of M. Cornu contains a large amount of theoretical matter and formulæ of corrections which of course we cannot reproduce here.

We may, however, refer to the principal causes of error. The first is a personal error depending on the sensibility of the eye of the observer in determining the disappearance and reappearance of the light at the toothed wheel, and also depending on the intensity of the luminous source; secondly, accidental inequality in the size of the teeth of the wheel; thirdly, irregularity of motion; fourthly, excentricity of wheels; fifthly, optical errors due to imperfections in the adjustment of the lenses and reflector. The first of these is small and can theoretically be reduced indefinitely by increasing the velocity of the toothed wheel and thereby observing the higher orders of extinction and reappearance of the light, but M. Cornu desires to be rigorously exact, and therefore the effect of this and the other errors is carefully calculated.

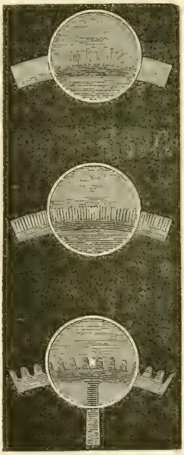

FIG. 2.—Details of Toothed Wheel.

Considerable care was exercised in the choice of stations, and those adopted were fixed upon chiefly on account of the distance between them being more easily ascertained from previous triangulation. This distance was determined by Cassini and La Caille in 1740, the result being 22910·196 metres. From the observations of Delambre the same distance was computed to be 22909·34 metres; and 22910 metres, which is nearly the mean adopted by M. Cornu.

The corrected results of the experiments gave a velocity of 300,350 kilometres a second, but this was obtained in air, and therefore 82 kilometres must be added to this result to give the velocity *in vacuo*; and as the result of his experiments M. Cornu adopts a velocity *in vacuo* of 300,400 kilometres = 186,638 miles per second of mean time, with a probable error of $\frac{1}{1000}$, or 300 kilometres.

If from this value we deduce the solar parallax, we find the latter to be 8″·881, assuming the time required for light to travel from the sun to us to be 6m. 13·2 sec., as obtained from observations on Jupiter's satellites, and the radius of the earth 6378·233 kilometres.

Again, the sun's parallax deduced from M. Cornu's values of the velocity of light in conjunction with the value of aberration is, with Bradley's estimate of 20″·25, 8″·882, and with Struve's, of 20″·445, 8″·798. These values of parallax compare favourably with determination by other methods, of which we give a few examples. The value given by the transits of Venus in 1761 and 1769 was 8″·5776 computed by Encke, but increased to 8″·891 by Mr. Stone on a redetermination. By the record of an observation of the occultation of ψ_2 Aquarii on October 1, 1672, M. Leverrier obtained 8″·866; by meridian observations of Mars at Greenwich in 1862, 8″·932; by the latitude of Venus obtained from transits of 1761 and 1769, combined with present latitudes, M. Leverrier finds 8″·853; from the discussion of meridional observations of Venus in an interval of 106 years 8″·859; by the opposition of Mars in 1860 by M. Liais 8″760; by opposition of Flora in 1873 by Prof. Galle 8″·873. Judging from these results the velocity of 186,638 miles per second is not very far from the mark, and the care in selection of methods and in computing results can scarcely be surpassed.

G. M. S.

EVOLUTION OF NERVES AND NERVO-SYSTEMS[1]

NERVE-TISSUE universally consists of two elementary structures, viz., very minute nerve-cells and very minute nerve-fibres. The nerve-fibres proceed to and from the nerve-cells, thus serving to unite the cells with one another, and also with distant parts of the animal body. Moreover, nerve-cells and fibres, wherever we meet with them, present very much the same appearances. Here, for instance, is a sketch of highly magnified nerve-tissue as we find it in the human brain, and here is one of my own drawings of nerve-tissue as I have found it in the jelly-fish; and you see how similar the drawings are—notwithstanding they are taken from the extreme limits of the animal kingdom within which nerve-tissue is known to occur.

Nerve-cells are usually found collected together in aggregates which are called nerve-centres or ganglia, to and from which large bundles of nerve-fibres come and go. These large bundles of nerve-fibres are what we see with the naked eye as nerves, permeating the body in all directions. When such a bundle of nerve-fibres reaches a ganglion, or collection of nerve-cells, it splits up like the end of a rope which has been teased out, and the constituent fibres pass into and out of the nerve-cells, so interlacing with one another in all directions, as here diagrammatically represented. More true to nature is this diagram, which represents a magnified section of human brain—the human brain being itself nothing more than a collection of very large ganglia.

To explain the *function* of nerve-cells and nerve-fibres, I must begin by explaining what physiologists mean by the word "excitability." Suppose this to represent a muscle cut from the body of a freshly-killed animal. So long as you do not interfere with it in any way, so long will it remain quite passive. But every time you stimulate it either with a pinch, a burn, or, as represented in the diagram, with an electrical shock, the muscle will give a single contraction in response to every stimulation. Now it is this readiness of organic tissues to respond to a stimulus that physiologists designate by the term excitability.

Nerves, no less than muscles, present the property of being excitable. Suppose, for instance, that this is another muscle prepared in the same way as the last one, except that together with the muscle there is cut out the attached nerve. Every time you pinch, burn, or electrify any part of the nerve, the muscle will contract. But you will carefully observe there is this great difference between these two cases of response on the part of the muscle; viz., that while in the former case the muscle responded to a stimulus *applied directly to its own substance*, in the latter case the muscle responded to a stimulus *applied at a distance from its own substance*, which stimulus was then *conducted* to the muscle by the nerve. And here we perceive the characteristic function of nerve-fibres, viz., that of conducting stimuli to a distance. This is the function of nerve-*fibres*; but the function of nerve-*cells* is different, viz, that of accumulating nervous energy, and at fitting times of discharging this energy into the attached nerve-fibres. The nervous energy when thus discharged from the nerve-cells acts as a stimulus to the nerve-fibre; so that if a muscle is attached to the end of the fibre it contracts on receiving this stimulus. I may add that when nerve-cells are collected into ganglia they often appear to discharge their energy spontaneously, without any observable stimulus to cause the discharge; so that in all but the lowest animals, whenever we meet with apparently spontaneous action, we infer that ganglia are probably present. But the point which most of all I desire you to keep well in mind this evening is the distinction which I here draw between muscle and nerve. A stimulus applied to a nerveless muscle can only course through the muscle by giving rise to a visible wave of contraction, which spreads in all directions from the seat of stimulation as from a centre. A nerve, on the other hand, conducts the stimulus without undergoing any change of shape. Now in order not to forget this all-important distinction, I shall always to-night speak of muscle as conducting a visible wave of *contraction*, and of nerve as conducting an invisible or molecular wave of *stimulation*. Nerve-fibres, then, are functionally distinguished from muscle-fibres—and also, I may add, from protoplasm—by displaying the property of conducting invisible or molecular waves of stimulation from one part of an organism to another—so establishing physiological continuity between such parts *without the necessary passage of contractile waves.*

I will now conclude all that it is necessary to say about the function of nervous tissue by describing the mechanism of reflex action. Suppose this to represent any peripheral structure, such as a part of the skin of some animal, this a collection of nerve-cells or ganglion, and this a muscle. The part of the skin represented is united to the nerve-cells composing the ganglion by means of this in-coming nerve-trunk, while the nerve-cells in the ganglion are united to the muscle by means of this out-going nerve-trunk. Therefore when any stimulus falls on the skin where this in-coming nerve-trunk takes its origin, the nerve-trunk conveys the stimulus to the nerve-cells in the ganglion. When the nerve-cells receive the stimulus they liberate one of their characteristic discharges of nervous energy, which discharge then passes down this out-going nerve and so causes the muscle to contract. Now this particular kind of response is called response by reflex action, because the stimulus wave does not pass in a straight line from the seat of stimulation to the muscle, but passes in the first instance to the ganglion, and is from it *reflected* to the muscle. This, at first sight, appears to be a roundabout sort of a process, but in reality it is the most economic process available; for we must remember the enormous number and complexity of the stimuli to which every animal is more or less exposed, and the consequent necessity that arises in the case of highly organised animals of there being some organised system whereby these stimuli shall be suitably responded to. Or, to adopt a happy illustration of Prof. Bain, the stimuli are systematised on the same principle as the circulation of letters by post is systematised; for just as in the case of the letters there is no direct communication between one street and another, but every letter passes first to the central office; so the transmission of stimuli from one member of the body to another is effected exclusively through a centre or ganglion.

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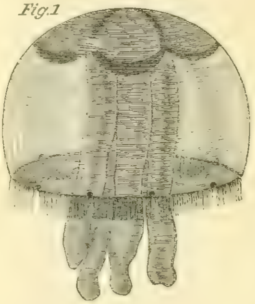
[1] Abstract of a Lecture delivered at the Royal Institution on Friday evening, May 25, 1877. By George J. Romanes, M.A., F.L.S., &c.

Those among you who are acquainted with Mr. Herbert Spencer's writings are doubtless well aware how strong a case he makes out in favour of his theory respecting the genesis of nerves. This theory, you will remember, is that which supposes incipient conductile tissues, or rudimentary nerve-fibres, to be differentiated from the surrounding contractile tissues, or homogeneous protoplasm, by a process of integration which is due simply to use. Thus, beginning with the case of undifferentiated protoplasm, Mr. Spencer starts from the fact that every portion of the colloidal mass is equally excitable and equally contractile. But soon after protoplasm begins to assume definite shapes, recognised by us as specific forms of life, some of its parts are habitually exposed to the action of forces different from those to which other of its parts are exposed. Consequently, as protoplasm continues to assume more and more varied forms, in some cases it must happen that parts thus peculiarly situated with reference to external forces will be more frequently stimulated to contract than are other parts of the mass. Now in such cases the relative frequency with which waves of stimulation radiate from the more exposed parts, will probably have the effect of creating a sort of polar arrangement of the protoplasmic molecules lying in the lines through which these waves pass, and for other reasons also will tend ever more and more to convert these lines into passages offering less and less resistance to the flow of such molecular waves—*i.e.*, waves of stimulation as distinguished from waves of contraction. And lastly, when lines offering a comparatively low resistance to the passage of molecular impulses have thus been organically established, they must then continue to grow more and more definite by constant use, until eventually they become the habitual channels of communication between the parts of the contractile mass through which they pass. Thus, for instance, if such a line has been established between the points A and B of a contractile mass of protoplasm, when a stimulus falls upon A a molecular wave of stimulation will course through that line to B, so causing the tissue at B to contract—and this even though no *contractile* wave has passed through the tissue from A to B. Such is a very meagre epitome of Mr. Spencer's theory, the most vivid conception of which may perhaps be conveyed in a few words by employing his own illustration—viz., that just as water continually widens and deepens the channels through which it flows, so molecular waves of the kind we are considering, by always flowing in the same tissue tracts, tend ever more and more to excavate for themselves functionally differentiated lines of passage. When such a line of passage becomes fully developed, it is a nerve-fibre, distinguishable as such by the histologist; but before it arrives at this its completed stage—*i.e.*, before it is observable as a distinct structure—Mr. Spencer calls it a "line of discharge."

Such being the theory, I will endeavour to show how it is substantiated by facts. And here it becomes necessary to refer to my own work. You are all, I suppose, acquainted with the general appearance of a Medusa, or jelly-fish. The animal presents the general form of a mushroom. The organ which occupies the same position as the stalk does in the mushroom is the mouth and stomach of the Medusa, and is called the polypite; while the organ which resembles in shape the dome of the mushroom constitutes the main bulk of the animal, and is called the swimming-bell. Both the polypite and the swimming-bell are almost entirely composed of a thick transparent and non-contractile jelly; but the whole surface of the polypite, and the whole *concave* surface of the bell, are overlaid by a thin layer or sheet of contractile tissue. This tissue is not exactly protoplasm and not exactly muscle, but something between the two. It constitutes the earliest appearance in the animal kingdom of anything resembling muscular tissue. The thickness of this continuous layer of incipient muscle is pretty uniform, and is nowhere greater than that of very thin paper. The margin of the bell supports a series of highly contractile tentacles, and also another series of bodies which are of great importance for us to-night. These are the so-called marginal bodies, which are here represented, but the structure of which I need not describe. Lastly, it may not be superfluous to add that all the Medusæ are locomotive. The mechanism of their locomotion is very simple, consisting merely of an alternate contraction and relaxation of the entire muscular sheet which lines the cavity of the bell. At each contraction of this muscular sheet, the gelatinous walls of the bell are drawn together; the capacity of the bell being thus diminished, water is ejected from the open mouth of the bell backwards, and the consequent reaction propels the animal forwards. In these swimming movements systole and diastole follow one another with as perfect a rhythm as they do in the beating of a heart.

The question as to whether the Medusæ possess a nervous system is a question which has long occupied the more or less arduous labours of many naturalists. Until lately, however, there has been so little certainty on the subject that Prof. Huxley—himself one of the greatest authorities on the group—thus defined the standing of the question in his "Classification of the Animal Kingdom:" "No nervous system has yet been discovered in any of these animals." The cause of this uncertainty is to be found in the fact that the transparent and deliquescent nature of the tissues of the Medusæ renders adequate microscopical observation in their case a matter of extreme difficulty; so much so that, looking to the quantity and quality of the labour which has been bestowed on the question, I doubt whether the latter would ever have been satisfactorily settled by the histological methods alone. But those of you who were present at my lecture last year will no doubt remember that by employing methods other than the histological, I was able to set this long-standing question finally at rest. For you will no doubt remember my having told you that on merely cutting off the extreme marginal rim of the bell I was surprised to find that the previously active motions of the animal suddenly and entirely ceased; the paralysis caused by this simple operation was instantaneous, enduring, and complete. On the other hand, you may remember, the severed margin which had just been taken from the swimming-bell invariably continued its rhythmical motions with a vigour and a pertinacity not in the least impaired by its severance from the main

Fig. 1



organism. For hours, and even for days after the operation, these motions persisted; so that the contrast between the death-like quiescence of the mutilated swimming-bell and the active contractions of the thread-like portion which had just been removed from its margin, was a contrast as striking as it is possible to conceive.

These experiments, then, conclusively proved that in the marginal rim of the Medusæ there is situated an intensely localised system of nervous centres, or ganglia, to the functional activity of which the rhythmical motions of the swimming-bell are exclusively due.

(*To be continued.*)

ON ELEMENTARY INSTRUCTION IN PHYSIOLOGY[1]

THE chief ground upon which I venture to recommend that the teaching of elementary physiology should form an essential part of any organised course of instruction in matters pertaining to domestic economy, is that a knowledge of even the elements of this subject supplies those conceptions of the constitution and mode of action of the living body and of the nature of health and disease, which prepare the mind to receive instruction from sanitary science.

It is, I think, eminently desirable that the hygienist and the physician should find something in the public mind to which they can appeal; some little stock of universally acknowledged truths, which may serve as a foundation for their warnings, and predispose towards an intelligent obedience to their recommendations.

Listening to ordinary talk about health, disease, and death, one is often led to entertain a doubt whether the speakers believe that the course of natural causation runs as smoothly in the human body as elsewhere. Indications are too often obvious of a strong, though perhaps an unavowed and half unconscious, undercurrent of opinion that the phenomena of life are not only widely different in their superficial characters and in their practical importance, from other natural events; but that they do not follow in that definite order which characterises the succession of all other occurrences, and the statement of which we call a law of nature.

Hence, I think, arises the want of heartiness of belief in the value of knowledge respecting the laws of health and disease, and of the foresight and care to which knowledge is the essential preliminary, which is so often noticeable; and a corresponding laxity and carelessness in practice, the results of which are too frequently lamentable.

It is said that, among the many religious sects of Russia, there is one which holds that all disease is brought about by the direct and special interference of the Deity, and which, therefore, looks with repugnance upon both preventive and curative measures, as alike blasphemous interferences with the will of God. Among ourselves, the "Peculiar People" are, I believe, the only persons who hold the like doctrine in its integrity, and carry it out with logical rigour. But many of us are old enough to recollect that the administration of chloroform in assuagement of the pangs of childbirth was, at its introduction, strenuously resisted upon similar grounds.

I am not sure that the feeling, of which the doctrine to which I have referred is the full expression, does not lie at the bottom of the minds of a great many people who would yet vigorously object to give a verbal assent to the doctrine itself. However this may be, the main point is that sufficient knowledge has now been acquired of vital phenomena to justify the assertion that the notion that there is anything exceptional about these phenomena receives not a particle of support from any known fact. On the contrary, there is a vast and an increasing mass of evidence that birth and death, health and disease, are as much parts of the ordinary stream of events as the rising and setting of the sun, or the changes of the moon; and that the living body is a mechanism the proper working of which we term health; its disturbance, disease; its stoppage, death. The activity of this mechanism is dependent upon many and complicated conditions, some of which are hopelessly beyond our control, while others are readily accessible and are capable of being indefinitely modified by our own actions. The business of the hygienist and of the physician is to know the range of these modifiable conditions, and how to influence them towards the maintenance of health and the prolongation of life; the business of the general public is to give an intelligent assent and a ready obedience based upon that assent, to the rules laid down for their guidance by such experts. But an intelligent assent is an assent based upon knowledge, and the knowledge which is here in question means an acquaintance with the elements of physiology.

It is not difficult to acquire such knowledge. What is true, to a certain extent, of all the physical sciences, is eminently characteristic of physiology—the difficulty of the subject begins beyond the stage of elementary knowledge, and increases with every stage of progress. While the most highly trained and best furnished intellect may find all its resources insufficient when it strives to reach the heights and penetrate into the depths of the problems of physiology, the elementary and fundamental truths can be made clear to a child.

No one can have any difficulty in comprehending the mechanism of circulation or respiration, or the general mode of operation of the organ of vision; though the unravelling of all the minutiæ of these processes may, for the present, baffle the conjoined attacks of the most accomplished physicists, chemists, and mathematicians. To know the anatomy of the human body, with even an approximation to thoroughness, is the work of a life, but as much as is needed for a sound comprehension of elementary physiological truths may be learned in a week.

A knowledge of the elements of physiology is not only easy of acquirement, but it may be made a real and practical acquaintance with the facts, as far as it goes. The subject of study is always at hand in oneself. The principal constituents of the skeleton, and the changes of form of contracting muscles, may be felt through one's own skin. The beating of one's heart, and its connection with the pulse may be noted; the influence of the valves of one's own veins may be shown; the movements of respiration may be observed; while the wonderful phenomena of sensation afford an endless field for curious and interesting self-study. The prick of a needle will yield, in a drop of one's own blood, material for microscopic observation of phenomena which lie at the foundation of all biological conceptions; and a cold, with its concomitant coughing and sneezing, may prove the sweet uses of adversity by helping one to a clear conception of what is meant by "reflex action."

Of course, there is a limit to this physiological self-examination. But there is so close a solidarity between ourselves and our poor relations of the animal world, that our inaccessible inward parts may be supplemented by theirs. A comparative anatomist knows that a sheep's heart and lungs, or eye, must not be confounded with those of a man; but so far as the comprehension of the elementary facts of the physiology of circulation and of respiration and of vision goes, the one furnishes the needful anatomical data as well as the other.

Thus, it is quite possible to give instruction in elementary physiology in such a manner as not only to confer knowledge, which, for the reason I have mentioned, is useful in itself; but to serve the purposes of a training in accurate observation, and in the methods of reasoning of physical science. But that is an advantage which I mention only incidentally as the present conference does not deal with education in the ordinary sense of the word.

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[1] A paper read at the Domestic Economy Congress, by Prof. Huxley, F.R.S.

It will not be suspected that I wish to make physiologists of all the world. It would be as reasonable to accuse an advocate of the "three R's" of a desire to make an orator, an author, and a mathematician of everybody. A stumbling reader, a pot-hook writer, and an arithmetician who has not got beyond the rule of three, is not a person of brilliant acquirements; but the difference between such a member of society and one who cannot either read, write, or cipher is almost inexpressible; and no one nowadays doubts the value of instruction, even if it goes no further.

The saying that a little knowledge is a dangerous thing is, to my mind, a very dangerous adage. If knowledge is real and genuine, I do not believe that it is other than a very valuable possession, however infinitesimal its quantity may be. Indeed, if a little knowledge is dangerous, where is the man who has so much as to be out of danger?

If William Harvey's life-long labours had revealed to him a tenth part of what may be made sound and real knowledge to our boys and girls—he would not only have been what he was, the greatest physiologist of his age, but he would have loomed upon the seventeenth century as a sort of intellectual portent. Our little knowledge would have been to him a great, astounding, unlooked-for vision of scientific truth.

I really see no harm which can come of giving our children a little knowledge of physiology. But then, as I have said, the instruction must be real, based upon observation, eked out by good explanatory diagrams and models, and conveyed by a teacher whose knowledge has been acquired by study of the facts, and not the mere catechismal parrot-work which too often usurps the place of elementary teaching.

It is, I hope, unnecessary for me to give a formal contradiction to the silly fiction, which is assiduously circulated by fanatics who not only ought to know, but do know, that their assertions are untrue, that I have advocated the introduction of that experimental discipline which is absolutely indispensable to the professed physiologist, into elementary teaching.

But while I should object to any experimentation which can justly be called painful, for the purpose of elementary instruction, and while, as a member of a late Royal Commission, I gladly did my best to prevent the infliction of needless pain for any purpose, I think it is my duty to take this opportunity of expressing my regret at a condition of the law which permits a boy to troll for pike, or set lines, with live frog bait, for idle amusement; and, at the same time, lays the teacher of that boy open to the penalty of fine and imprisonment if he uses the same animal for the purpose of exhibiting one of the most beautiful and instructive of physiological spectacles, the circulation in the web of the foot. No one could undertake to affirm that a frog is not inconvenienced by being wrapped up in a wet rag, and having his toes tied out; and it cannot be denied that inconvenience is a sort of pain. But you must not inflict the least pain on a vertebrated animal for scientific purposes (though you may do a good deal in that way for gain or for sport) without due licence of the Secretary of State for the Home Department, granted under the authority of the Vivisection Act.

So it comes about, that in this present year of grace 1877, two persons may be charged with cruelty to animals. One has impaled a frog, and suffered the creature to writhe about in that condition for hours; the other has pained the animal no more than one of us would be pained by tying strings round his fingers, and keeping him in the position of a hydropathic patient. The first offender says, "I did it because I find fishing very amusing," and the magistrate bids him depart in peace; nay, probably wishes him good sport. The second pleads, "I wanted to impress a scientific truth, with a distinctness attainable in no other way, on the minds of my scholars," and the magistrate fines him five pounds.

I cannot but think that this is an anomalous and not wholly creditable state of things.

OUR ASTRONOMICAL COLUMN

D'Arrest's Comet.—M. Leverrier notifies the discovery of the periodical comet of D'Arrest by M. Coggia at Marseilles, on the 8th inst., nearly in the position assigned by M. Leveau's calculations. It was also detected at Florence by M. Tempel, on the 10th.

The comet was discovered by the late Prof. D'Arrest at Leipsic on June 27, 1851, and observed till October 6. The elliptical character of the orbit was pointed out by the discoverer early in August, and his conclusions were verified by the calculations of Vogel and Villarceau shortly afterwards, the latter astronomer commencing, while the comet was yet under observation, a series of elaborate computations of the effect of planetary perturbations upon its motion, which were continued by him until taken up by Leveau. With the aid of Villarceau's ephemerides the comet was detected on its ensuing return to perihelion at the Royal Observatory, Cape of Good Hope, and observed from December 5, 1857, to January 18, 1858. Oudemans, in a memoir published by the Royal Academy of Sciences at Amsterdam in 1854, had also carried forward the elements to this appearance, his results indicating that while the normal positions of 1851 were best represented by a mean motion which would bring the comet to perihelion again on December 5, 1857, there yet remained an uncertainty to the extent of eighty-five days in the length of the revolution. Villarceau, in the *Comptes Rendus de l'Académie des Sciences*, 1852, December 6, considered the period fixed within narrower limits, one of his sets of elements assigning November 28, 1857, for the next perihelion passage, on which day the Cape observations show that it actually occurred. At the second return in the spring of 1864 the comet was not observed, and a very heavy work was involved in the preparation of an ephemeris for 1870, owing to the large perturbations due to the action of Jupiter in 1861, the comet having in April of that year approached the planet within 0·36 of the mean distance of the earth from the sun, and the two bodies remaining in proximity for a considerable time; it was therefore necessary to determine the effect of this near approach to the most powerful of the planets with every possible precision, a long work successfully accomplished by Leveau, who found on continuing the calculation of the perturbations of Jupiter, Saturn, and Mars, to June, 1870, the following material changes in the elements at the perihelion passage in November, 1857.

	° ′		° ′
Long. of Perihelion	− 4 32	Angle of Eccentricity	− 1 52
,, Ascend. Node	− 2 12	Mean anomaly ...	+ 10 10
Inclination	+ 1 43	Mean motion ...	− 15″·82

So that the period of revolution was lengthened sixty-eight days, the comet arriving at perihelion on September 22, 1870. The effect of these perturbations was to alter the geocentric place at this time, no less than 14°·6 in right ascension, and 7° 6 in declination. At all three returns the comet has been a faint object, and it was particularly so in 1870, when it was, nevertheless, sufficiently observed, Prof. Julius Schmidt, profiting by his favourable position at Athens, to follow it until nearly the end of the year.

The following are the dimensions of the orbit of D'Arrest's comet in the present year, according to the elements of Leveau.

Semi-axis major ...	3·54139	Perihelion distance	1·31809
,, minor ...	2·75651	Aphelion ,, ...	5·76469
Semi-parameter ...	2·14559	Eccentricity ...	0·6278048

The period of revolution is 2434·2 days, or 6·664 years, therefore nearly identical with that of Biela's comet up to 1852. The comet passed its perihelion on May 10. It will not arrive at its least distance from the earth until October 20, but the theoretical intensity of light diminishes from the present time, indeed has been on the decrease since the middle of May; the comet may be a test object on the borders of the constellations Eridanus and Orion in September and October.

It is probable that this comet had been revolving in its present restricted orbit for many years previous to its discovery in June, 1851. It certainly does not furnish a parallel case to that of Brorsen's comet, which was detected at its first perihelion passage after the attraction of the planet Jupiter had impressed upon it the actual form of orbit in May 1842. The nearest approach of D'Arrest's comet to Jupiter during the revolution immediately preceding discovery, took place at the end of September 1849, when the distance was 1·136.

THE BINARY STAR *α* CENTAURI.—Mr. Ellery communicates to the Royal Astronomical Society recent measures of this, the finest and most interesting of all the revolving double-stars. Taking means the following epochs result from the Melbourne measures:—

1876·72	Position	51°·1	Distance	4″·3
1877·25	,,	69·1	,,	3·13

Mr. Maxwell Hall (NATURE, vol. xv. p. 510) supplies the following:—

1877·14	Position	64°·4	Distance	3″·3

Mr. Ellery states that the distance in 1862 was 10″, but this must be an over-estimate with the meridian instrument; a mean of seventy micrometrical measures by Mr. Eyre B. Powell at Madras, gives for 1862·2, a distance of only 6″·79, a result no doubt entitled to great confidence. The first *minimum* of distance appears to have occurred in 1856. Capt. Jacob's measures for 1856·27 giving 3″·89, and a maximum of about 10″ followed in 1868-70. It is to be hoped that the star will now be frequently measured micrometrically with all possible precision, though the brilliancy and closeness of the components may render such measures difficult. A practised computer should be able to throw some light on the real nature of the orbit from the data already in our possession, but the continued regular measurement of the star at this critical period cannot fail to be of great importance in extending our knowledge of the motion in this system. The reliable estimate of its distance resulting from the observations of Henderson, Maclear, and Moesta, vastly increases the interest attaching to it.

GEOLOGICAL NOTES

GEOLOGICAL SURVEY OF THE UNITED KINGDOM.—The gradual progress of the English and Scottish Geological Surveys has brought the members of the two corps almost within sight of each other. The line of demarcation between the two kingdoms being nearer the base of operations from Scotland, has been sooner reached from that side. From Berwick-on-Tweed southwestwards the work has been carried up from the north to the English border through the range of the Cheviot Hills, and down the valley of Liddesdale to the Solway. To prevent any subsequent risk of the lines from either side not fitting accurately, the officers on the Scottish border are at present engaged in running their boundaries into Cumberland and Northumberland for such a short distance as may be required to leave them in a position where they can be easily taken up by the advanced guard of the English Survey. When this and some few isolated areas are completed, the whole of the south of Scotland between the Tay and Clyde and the English border will have been geologically surveyed, and the Scottish staff will then be engaged on both sides of the flanks of the Highlands. Already ground has been broken, and some progress has been made on the north side of the Grampian chain. On the English side the mountainous lake district is all surveyed, while the work is so well advanced in Cumberland that it may probably be completed up to the Scottish border by the end of this year. Considerable progress has likewise recently been made on the eastern side in pushing the survey northwards in Northumberland, though a considerable tract of that country still remains unmapped towards the Cheviots and Tweed. Among the south-eastern counties the survey is advancing through Norfolk, Suffolk, and Cambridgeshire, while in the south-west some of the maps which were made in the early days of the Geological Survey are being re-surveyed and brought up to date in West Somerset and Devon.

GEOLOGICAL SURVEY OF CANADA.—The Report of this Survey for 1875-76 has just arrived. In size, general interest, and geological value, it fully equals its well-known predecessors, while in regard to maps, sections, and other illustrations, it even surpasses them. Briefly told, its story is this—The Philadelphia Exhibition absorbed much of the time and thought which would otherwise have been expended on the field-work, laboratory, and museum duties of the officers. But the director need not regret this temporary suspension of the usual operations of his staff, for there can be no doubt that the display of rocks, minerals, and fossils, made by Canada at the Centennial Exhibition, so universally admired brought the mineral resources of the dominion and the skill of its geological survey before the world with such prominence as could hardly have been attained even with the ablest maps and memoirs. Mr. Selwyn's own labours from April to November, 1875, embraced an exploration of parts of British Columbia where likewise Mr. George M. Dawson, who has lately been appointed to the Canadian Survey, has been actively employed. Prof. Macour, besides his geological work, made a careful botanical survey of the region traversed, and his detailed narrative appears in this Report. Mr. Ellis was sent into the North-west Territory to make a series of borings. Mr. Bell explored the country between James Bay and Lakes Superior and Huron; while in the eastern parts of the Dominion detailed surveys were made in the coal-fields of Nova Scotia, in New Brunswick, and in Cape Breton. Besides these explorations others were continued by Mr. Vennor in Ontario. Of these Mr. Selwyn remarks that they prove the existence in Western Quebec and Eastern Ontario of a massive red orthoclase gneiss without visible stratification, lying probably unconformably under the vast crystalline masses containing *Eozoon*. He suggests that what is called Lower Laurentian may have to be termed Middle, the fundamental red gneiss becoming the Lower.

EXCREMENTITIOUS DEPOSITS IN THE ROCKY MOUNTAINS.—A recent paper to the Philadelphia Academy by Mr. Henshaw, on the excrementitious deposits in the Rocky Mountain region, sustains Prof. Cope's view that they were made by big-eared rats, a species of *Neotoma*, probably *N. cinerea*. They consist of vegetable matter, sometimes with a bitumen-like look, and varying from this appearance to that of pill-like excrements. In a crevice of the rocks one deposit had a depth of two feet, and contained also some small twigs and "birds'" feathers. "The mass was evidently the accumulation of years, and had served as a nest. Throughout was a large amount of hard droppings from which the urine had passed, and whose nature was unmistakable. The urine, charged with a certain amount of excrementitious matters, had filtered through to form the singular deposits." Water or the urine has carried the portions it could dissolve down the faces of walls, and deposited it on shelves where no animals without wings could reach, and sometimes on the roofs of cavities. All the regions where these deposits occur are inhabited by the *Neotoma*, which is essentially a vegetarian

NOTES

THE inaugural meeting of the Domestic Economy Congress was held at the rooms of the Society of Artists in Birmingham, on Tuesday last. In the absence of Lord Leigh the Mayor took the chair. Prof. Huxley gave a short address, in which he maintained that our so-called education does not fit a man for understanding his social duties. Public opinion was now beginning to take a different view of what education ought to be. Those who supported this Congress were among those who felt most strongly on the subject, and the influence on Government from discussions and meetings would be successful in the long run. The real business of the Congress commenced yesterday, when, amongst the papers read, were:—"Nursing," by Mrs. W. E. Gladstone; "Infant Life," by the Countess of Ebersburg; "Nursing in Connection with Education," by Miss Helen Taylor; "Elementary Instruction to Children in Physiology," by Prof. Huxley; and "Warming and Ventilation," by Capt. Galton. We give elsewhere an account of Prof. Huxley's paper, and we commend the latter part of it especially, not only to those who are interested in the teaching of physiology in schools, but to all who have given attention to that "burning question" of the day—vivisection.

THE annual session of the French Association for the Advancement of Science will take place at Havre on August 23 next, so that the members of the British Association will be able to take part in the proceedings, and a good attendance of these is anticipated. Members wishing to visit Havre are desired to write to the secretary of the French Association, 76, Rue de Rennes, Paris. Dr. Gilbert, local secretary in Havre, will engage rooms if required by previous notice. The president this year is Dr. Broca, Professor to the Faculty of Medicine, and Director of the Anthropological School of Paris. The maritime situation of Havre will supply every opportunity for a number of interesting excursions, especially for members of the British Association. The principal object of the deliberations of the society will be to determine how to secure a large and effective representation of science during the International Exhibition next year. A delegation of the French Society will be sent to the session of the British Association as was arranged last year.

ON Monday the 9th inst. the Haberdasher's Company voted 250 guineas as a donation to the building fund of the new City of London College. This institution is the outcome of the Metropolitan Evening Classes for young men, originally established at Crosby Hall, Bishopsgate Street, in 1848. Since that time it has pursued a most successful career, and now has more than 1,400 members.

THE Albert Medal of the Society of Arts has recently been presented to Sir George B. Airy, K.C.B., "for eminent services rendered to commerce by his researches in nautical astronomy and in magnetism, and by his improvements in the application of the mariner's compass to the navigation of iron ships."

WE understand that the International Meteorological Congress which was to have met at Rome in September, has been postponed to next year.

THE sad death of Dr. James Bryce furnishes another instance of the fact that though geology is in itself an invigorating pursuit it has its own share of risks. This long-known writer had left his home in Edinburgh on Tuesday morning the 10th inst. for a geological tour in the Western Highlands. He had reached as far as the Falls of Foyers on Loch Ness, whither he had undertaken to conduct a scientific excursion from Inverness a little later. On Wednesday, after leaving at the hotel a note addressed to his daughter announcing his safe arrival so far on his journey, he strolled out hammer in hand to make some further observations among the granitic crags of that neighbourhood to which he had already given some attention. He was never seen again alive. A few hours afterwards his lifeless body was found on a slope of *débris* at the foot of a shattered cliff of rock. His hammer lay a few yards higher up at the base of the crags. It is supposed that either from the concussion of his hammer or from some other cause a portion of the cliff had fallen away, crushing his temples, and killing him instantaneously. Dr. Bryce's early researches among the basalt of the north of Ireland, his work on Arran and Clydesdale, his papers on the Secondary rocks of the West Highlands, and his labours in connection with Scottish earthquakes have made his name familiar to geologists in this country. He was seventy-one years of age.

WE regret to announce the death, on the 15th instant, of another well-known geologist, Mr. John Williamson, of Scarborough, the discoverer of the celebrated Gresthorpe plant-beds. His labours in bygone years, when field-workers like him were very scarce, will long be borne in remembrance. He was born in 1784, so that he was ninety-three years of age.

THE Portuguese African explorers, Major Serpa Pinto and Capt. Brito Capello, have returned to Lisbon after visiting Paris and London (whither they had gone to obtain some necessary articles), and are to start for Loanda in the *Zaire*. According to the *Diario de Noticias*, they have got together a magnificent outfit, and M. de Abbadie, the eminent French explorer, pronounced this expedition the best and most scientifically formed that had yet gone out to Africa. He has given them the remarkable universal theodolite with inverse action, of his own invention, which he has called the "Abbas," and with which he made geodetic measurements in Abyssinia; which further he used in Algiers in observation of the transit of Venus. They also take a new apparatus invented by Father Perry for the study of terrestrial magnetism, one of the best equatorials of the Polytechnic School of Paris, a sextant of great delicacy, &c. M. Serpa Pinto has previously made extensive journeys in Africa, to Lake Nyassa and to near the Victoria Falls of the Zambesi.

WE note that a public meeting is to be held at the Mansion House to-day in aid of the "African Exploration Fund," recently commenced by the Council of the Royal Geographical Society to promote the continuous and systematic exploration of the interior of Africa.

AN engineer of St. Petersburg, M. de Kern, announces the discovery of a new metal, which he calls *Davyum*. It is found in the residues got from extraction of platinum. To isolate the element it is necessary, after having precipitated the ruthenium, to treat the mother lye with nitrate of ammonia. A red precipitate is produced, which on calcination yields davyum. This metal is easily attackable by aqua regia, and much less by boiling sulphuric acid; potash precipitates it in a yellow state, and sulphuretted hydrogen in a brown, passing into black through desiccation. With sulphocyanide of potassium the chloride of davyum gives a red coloration. From theoretical considerations developed by M. Mendeléeff, M. de Kern considers davyum to rank between molybdenum and ruthenium. On this supposition its equivalent should be near 100; and he proposes to test this experimentally.

M. V. OBERMAYER, of Vienna, has proved by experiments that the internal friction (viscosity) of hard black pitch is subject to the same laws as fluid friction. He determined the coefficients of internal friction by three different methods:—1. Pressure of cylindrical plates; 2. Deformation of parallelepipedal plates; 3. Distortion of cylindrical plates. No gliding of the black pitch occurs on the metal plates, between which the pitch plates are cast. For soft bodies, the internal friction appears not to follow exactly the laws of fluid friction.

ON July 13 the French Minister of Public Works visited the works of the 1878 Exhibition, which are in course of progression at the Champ de Mars. The number of workmen engaged in actual working was 1,137. Not less than 700 were employed at the Central Pavilion.

A SINGULAR accident has been recorded by the *Journal Officiel*. M. Gastard, of Paris, had placed a number of cartridges on a table. Some solar rays having been concentrated by an "eye" in the glass of a window, a terrific explosion took place. Similar catastrophes are more common than is generally supposed in summer, the windows of railway carriages, igniting sometimes overdried plants, or even leaves fallen on railway embankments. It is known also that fires sometimes occur in Algerian forests through drops of water suspended to the leaves and forming lenses.

IT is now about a quarter of a century since the first submarine cable was laid, and the telegraph system may now be said to embrace all parts of the world, offering a certain completeness as an object of study. In an interesting *brochure* recently sent us, "Recherches sur la Loi du Mouvement Telegraphique International," M. Madsen sets the problem,—Is there a determinate relation between the international telegraphic movement and the commercial traffic, and what is the mathematical expression of this relation? He arrives, from a comparison of statistics, at a law which may be approximately expressed by the equation, $T = \frac{1}{d}[\sqrt{VN} + N_1 + N_2]$; in which T denotes the number of telegrams between two countries, d the distance between their commercial "centres of gravity," V the value (in pounds sterling) of their commerce with each other, N the tonnage of the ships sailing between them, N_1 the tonnage of ships of the one country (L), but sailing between the other L_1 and other countries; N_2 the tonnage of ships belonging to L_1, but sailing between L and other countries. The law has various applications, some of which M. Madsen points out.

AN ingenious new registering thermometer devised by M. Hervé Mangon, is described in *La Nature*. A long and fine capillary tube bent on itself and containing mercury, is supported in an iron frame; it passes through the stopper of a bell jar and terminates with a fine point in a mercury dish placed in one scale of a balance; the other scale contains a vessel of glycerine communicating by glass and india-rubber tubing with another glycerine vessel on the same level in an adjoining frame. When, on rise of temperature, mercury is forced out into the vessel, the balance is depressed on one side and an electric contact made, affecting an electro-magnet in the registering apparatus, which is composed of M. Redier's double wheel-work with differential train (which we must not stay to describe). When the depression referred to has occurred a suspended float in the second glycerine vessel descends, and raises the glycerine in the first, increasing the weight in that scale. The curve obtained (from a pencil on moving paper) is of zigzag form, the wheel-work being in constant motion, now to the right, now to the left.

M. BERTRAND having lately made an appeal to possessors of letters from Gauss, with a view to publishing the complete works of the eminent geometer, the grand-daughter of Laplace has responded with five interesting letters. One of them, written in 1807, presents Gauss at the outset of his career, deprived of his fortune and threatened with extreme measures if he did not pay 2,000 francs as a war contribution to the French army occupying Göttingen. In his distress he applies to Laplace, thinking his intervention might prove effectual. At the same time he describes the equally sad position of his colleague Harding. Laplace, unable to influence Napoleon, pays the 2,000 francs, and begs his friend not to disquiet himself further. Meanwhile Gauss obtains the sum from Olber, and now he is in a position to succour Harding. Two years later he paid Laplace back the sum he had borrowed.

AN analysis has been lately made by Dr. Alder Wright (*Chemical News*) of two samples of wine, "ruby" and "white," from the Auldana vineyards, South Australia, with a view to determine the proportion of iron as a natural constituent. The average amount obtained by one method was, in both cases, 0·00130, by another method 0·00146, the iron being calculated as FeO (in the former case it is thought there may have been a little loss through incineration, &c.). Two circumstances are noted; first, that contact of the grape-juice with ironwork of any kind is studiously avoided in the manufacture; secondly, that the soil of the Auldana vineyard is exceptionally ferruginous, and as iron is taken up from the soil by vegetation, this seems a probable cause for the occurrence of iron in the finished wine. The identical character of the values, too (the wines being of different vintages) makes it improbable that the source of the iron is outside the grape-juice.

AN interesting experiment with regard to the speed of pigeon flight was made the other day. A carrier pigeon having been let off in Dover simultaneously with the starting of the express for London, reached the latter place twenty minutes in advance of the train. This corresponds to a distance-difference of eighteen miles.

WE observe that the recent enlightened decision by the Senate of London University with regard to admission of women to degrees in medicine meets with a good deal of hostile criticism from some of our leading medical contemporaries. We feel sure that no Trades' Union spirit will be allowed to prejudice what must generally be recognised as a step in the right direction.

AT a meeting of members of the Birmingham Natural History and Microscopical Society, held at the Midland Institute on July 13, a committee was appointed to make arrangements for another marine excursion, somewhat similar to the one made by this society in 1873 to Teignmouth, but this time it is to be to Arran and the Western Islands of Scotland. The dredging will be carried on in Lamlash and Brodick Bays. At the same time excursions will be made on land to Arran and the adjacent localities, all of which, we believe, yield a number of rare specimens, both botanical and geological; so that the members of each section no doubt will find this an enjoyable and an interesting excursion. It will take place in the first week in September.

A WONDERFUL white aquamarine has been found in Perthshire which, when cut, has produced one of the most brilliant gems ever seen. It is said by many competent judges to be equal to her Majesty's celebrated Koh-i-noor, its refraction being very great both by day and night. It is of a pure pellucid liquid white, and is known as the Scotch Koh-i-noor. Its hardness is 8·0, and specific gravity 2·76. Mr. Bryce M. Wright, F.R.G.S., is its possessor.

THE additions to the Zoological Society's Gardens during the past week include a Sambur Deer (*Cervus aristotelis*), a Spotted Porcine Deer (*Cervus minor*) from India, presented by H.R.H. the Prince of Wales, K.G.; a Slow Loris (*Nycticebus tardigradus*) from Malacca, a Prehensile-tailed Paradoxure (*Paradoxurus prehensilis*) from Burmah, presented by Mr. W. H. Richardson; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mrs. Shand; a Red Howler (*Mycetes seniculus*) from New Granada, four Axis Deer (*Cervus axis*) from India, a Merian's Opossum (*Didelphys dorsigera*) from South America, deposited; two Striped Hyænas (*Hyæna striata*), born in the Gardens, a Black-necked Swan (*Cygnus nigricollis*), hatched in the Gardens.

THE STÄLLDALEN METEORITE[1]

IN the Scandinavian North, so extraordinarily rich in mines and quarries, there have been found during the last few years a number of new minerals, by which many a mine and even many an inconsiderable opening scarce known in its own parish has become world-famous in mineralogical literature. Several of these *finds* are of great interest in a systematic aspect—for instance, the discovery of *barytite*, a new, exceedingly basic variety of felspar containing baryta; of *ganomalite*, the first natural silicate of lead which has been discovered; of *ekdennite*, a new mineral containing antimonic acid, from the mines of Langban; and of *homilite*, a new, beautifully crystallised silicate of boron, containing water, from Brevig. Others again give us a highly unexpected insight into the nature of the chemical forces which are in activity in the interior of the earth—for instance, the Wermland minerals, *manganosite*, or protoxide of manganese, and *pyrokroite*, or hydrated protoxide of manganese, which afford evidence of a powerful reducing action. The latter mineral has during last year been found at a new locality—the mines of Nordmark.

However important these newly-discovered minerals may be, they do not awaken so keen an interest as the stones which from time to time fall from the heavens, and afford us specimens of the matter to be found in spaces so remote that rays of light require thousands of years to reach them. A new and highly instructive contribution to our knowledge of meteorites has been obtained in Sweden through the fall of the meteorite, which took place at Ställdalen, near Nya Kopparberg, in Örebro län, on June 28, 1876, at 11·50 A.M., from a fireball which was visible over a large part of middle Sweden. In the neighbourhood of Stockholm the meteor appeared as an indistinctly-defined fireball, followed by a long streak of fire. The ball was first visible below the zenith in the north-east or north-north-east, and went from hence towards the horizon in the west, where it generally appeared to fall in the immediate neighbourhood of the spectator, sometimes with, sometimes without, the throwing out of sparks. In the town of Gefle the fire-red ball, followed by a streak of the same colour, was seen moving from north-east to south-west. At the neighbouring promontory, Harnäs, it was first seen of the size of a large star, speedily increasing, however, leaving a long streak of fire behind it, and finally disappearing without noise, falling, according to the supposition of the spectator, behind some neighbouring buildings.

At Malmköping the meteor appeared to proceed from the northern heavens towards the west, leaving behind it a fine white streak, which was distinguishable for two minutes. At a height of 25° above the horizon it disappeared without falling asunder. At Linköping the nucleus of the meteor was pear-shaped, of blinding whiteness, followed by a streak of fire which was strongly luminous notwithstanding the clear bright sunshine, and about eight times longer than the nucleus. It was first observed pretty high up in the north-east, but afterwards sank to a height of 10° above the horizon in the west, where it broke up without noise into a number of star-sparks. In Skara the meteor, followed by a beautifully luminous streak of fire, appeared to fall asunder, throwing out sparks strongly at the same time, after having gone from east to west with an apparent diameter of half that of the moon. In Hedemora two fire-balls were seen, one close behind the other, falling from the zenith towards the west, leaving behind them a light grey streak. A minute after the meteor passed from the field of view, a loud explosion was heard, which is also mentioned in reports from the town of Falun and from Gustafs and Stora Tuna parishes. In Mora no explosion was heard, but here the meteor, which left in its path a stream of fire of a deep violet colour, was seen to fall asunder in the south-south-east, with a strongly luminous fire rain, the fire drops of which, however, were extinguished before they reached the horizon. In Karlskoga a fireball of a blinding clear reddish white lustre was seen high up in the zenith. Hence it sank towards the north-north-west to a height of 30°, and afterwards parted into three or four smaller pieces, which speedily went out and resembled the stars which fall from a rocket. The meteor left behind it a white smoke, which in the calm air remained in the direction of the fall about a minute, and afterwards dispersed. In the neighbourhood of Karlstad, the meteor was thought to fall in the north east. It was compared to a falling star rocket. It was very bright, with a white nucleus, having fire-red edges, and passing when bursting asunder to a blinding white, the separate pieces being clearly visible. Its apparent size was compared to that of the full moon, and after its disappearance a white streak remained for some seconds in the sky. In Hoböl parish in Dalsland there was seen in the sky a pointed fire-ball, resembling in form a soda-water flask, at first pretty high in the heavens, afterwards approaching the earth, dividing into two parts and disappearing without any detonation after the lapse of half a minute. At Lysekil the meteor appeared to fall perpendicularly in the north-west, and spring asunder without any noise some few feet above the surface of the water. According to a statement in the newspapers the meteor in question was simultaneously seen at Christiania. In Denmark and Finland it was not visible.

From a careful and critical examination of these statements, and many others which have been collected, it appears that the meteor in question, possibly with the neighbourhood of γ Cephei as radiation point, proceeded in a somewhat oblique direction to the place where the stones fell on the meteor bursting asunder. If with a point 40 kilometres south of Ställdalen as a centre, a circle be described through Christiania, the westernmost place where the phenomenon was observed, its circumference intersects Orust in the south, the neighbourhood of Stockholm in the east, and Gefle in the north-east, and includes all the places where the meteor was visible. At Stockholm, Hedemora, Karlskoga, and Lysekil, the meteor is said to have been visible first in the north-east, somewhat below the zenith, and if the direction is noted where it disappeared in the neighbourhood of the horizon, this direction in general corresponds very well with the direction from the place of observation to the place of fall. The meteor thus went under the horizon or disappeared in its neighbourhood at Stockholm to west, at Gefle to south-west, at Mora to south-south-east, at Lysekil to north, at Malmköping to north-west, and so on.

The meteor thus reached the end of its short luminous path in the region where the fall took place. It became luminous at a height which cannot, after making allowance for errors of observation, be reckoned at less than 300 to 400 kilometres, but was probably greater. At this height the atmosphere, notwithstanding its extreme tenuity, is capable by its resistance of heating red hot a body moving with *cosmic* speed, as of 75 kilometres per second, and if the composition of the atmosphere at this height be the same as at the surface of the earth the meteor will meet with sufficient oxygen to maintain a lively combustion of the combustible matters which enter into the composition of the meteor. It appears to me that we have here an explanation of the considerable height in the atmosphere at which meteors first become luminous—an explanation which is so much the more probable as we now not only know a number of carboniferous meteorites, but also by the meteorite fall at Hessle, in Upland, have distinct proof that the common meteorites may be accompanied, and perhaps are generally accompanied, by an easily combustible carboniferous dust. Only through such a supposition can we obtain an explanation of the large size of these meteors when compared with the stones which fall, as well as of their strong illuminating power, which clearly shows that the light arises from the glowing of solid masses, and not merely from the compressed and heated gases which the meteor has collected before it.

The statements regarding the size of the Ställdalen meteor are very various. The most probable are those which give it a diameter of six minutes, which, supposing the distance to be 250 kilometres, would give the fireball a diameter of 436 metres, or nearly 1,500 feet. In comparison with this size the stones that have fallen are surprisingly small, which yields a further support to the supposition that the main mass of the meteor consisted of substances which had already high up in the air been dissipated in the form of gas or undergone combustion. In the case of the Ställdalen meteor there is also the exceedingly remarkable circumstance that the fireball was not visible in the region where the path of the meteor struck the earth and where the meteorites fell, although this place lay nearly in the centre of the area where the meteor was visible as a luminous fireball, and although the sky here too was clear and cloudless with the exception of the little dark cloud which the meteor collected before it in its path through the air. It was probably this cloud which prevented it from being seen in the region which lay in the direction of the fall. Although no fireball was seen here, loud detonations were heard and some light streaks of cloud were visible in the zenith, from which, according to some, faint flashes of fire resembling lightning were seen darting. Whistling, rumbling, and rattling noises were also heard. The sound was thought, for the most part, to come from the west or south-west. It was not heard in Karlskoga, which lies to the south, but far to the north and north-west. At Falun it was supposed that a fall of rock had taken place in a mine, and at Grandgrufvan, at Ludvika, the sound was heard as of a peal of thunder at a depth of sixty metres underground. At other places a dynamite magazine was thought to have exploded, or it was taken for a loud clap of thunder.

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[1] Abstract of an Address by Prof. Nordenskjöld at the Anniversary of the Royal Swedish Academy of Sciences.

In the neighbourhood of a workman who was cutting trees in a wood several branches of a tree were broken off by a stone weighing nearly a kilogram, in a way which clearly showed no great falling velocity, which was further confirmed by the stone making a hole in the ground only a decimetre in depth. Another person saw a stone fall close beside him, and immediately took it up. It was not at all warm. A girl saw a stone weighing two kilograms fall to the ground "so that the earth smoked." Several fell in the Lake Björken or were picked up in the neighbourhood soon after. One weighing 8½ kilograms fell in a rye-field. In falling it had gone in two pieces and made an eight-inch deep hole in the cultivated soil. The largest stone weighed 12½ kilograms.

The number of the stones that have been found, however, amounts only to eleven, with a total weight of thirty-four kilograms. They were scattered within an oval two kilometres broad, whose larger axis had a length of eight kilometres. The largest stone was found in the south-west end of the oval, in a meadow surrounded by wood. It is probable that larger stones have fallen farther into the wood, and thus escaped observation. The stones are of very irregular form, and on their surface are full of the depressions peculiar to meteorites. On the surface they are, as usual, covered with a blackish fused crust of very variable thickness, being so thick on some of the fractured surfaces as to completely conceal the colour and inequalities of the main mass, and on other similar surfaces so thin that the colour and crystalline structure of the main mass may be clearly distinguished. Sometimes the crust is completely wanting, so that the surface of the stone, with the exception of an inconsiderable blackening, resembles a fresh fracture. The stones are thus fragments which have been formed at different times, and exposed for different periods to the action of the glowing envelope. The largest stones are covered in many directions with black friction surfaces which are more clearly marked on these meteorites than on any I know. These too have probably been formed in our atmosphere, and show that with the great pressure produced by the resistance of the air, cracks have been formed in the meteorite along which its different parts before springing asunder rubbed against each other during the rotation of the irregularly-formed mass, whereby the uneven surfaces have been smoothened, and coloured black by the heat developed during friction, the projecting metallic particles flattened, &c. On breaking in pieces the meteorites in question, they are found to consist of a coarse breccia-like mixture of grey and of nearly black portions, little differing from each other in chemical composition. It is remarkable that the grey mass when heated becomes dark, and thereby in appearance quite like the black, which appears to show that some of the breccia-like pieces found in the stones had been heated, while this does not appear to have been the case with the other part. Different pieces of the Ställdalen meteorites thus appear to have been exposed to the action of very different temperatures before they were united into the mass, hard, tough, and difficult to break up, which formed the meteorite.

The stones that fell at Ställdalen have been carefully analysed by Mr. G. Lindström, assistant in the mineralogical department of the Riks Museum, who found them to consist of nickel-iron; a silicate decomposed by acids, chiefly olivine; a silicate indecomposable by acids, probably bronzite; magnetic pyrites, and inconsiderable quantities of phosphide of nickel-iron; of a phosphate, and of chloride of iron. The first-named substance, a metallic alloy of ninety per cent. iron and ten per cent. nickel, is not known of terrestrial origin, but distinguishes most meteorites, and makes it possible to separate with certainty the meteorites which have fallen at Ställdalen from all other minerals occurring in the quarter. The two other main constituents again, olivine and bronzite, are also wanting in our granites, gneisses, and common slaty rocks, but are found commonly entering into the composition of a number of rocks which by most of the geologists and mineralogists of the present day are considered to be of plutonic origin. Many circumstances, however, indicate that these rocks, which in remarkably regular layers cover extensive regions of the earth's surface, *often, but not always*, consist of stratified tuff-like formations which during the enormous duration of geological periods have assumed a crystalline structure. The resemblance between them and various constituent parts of the meteorites is so striking that the question must be seriously and impartially discussed whether a part of the plutonic rocks are not of cosmic origin. By this I mean that it gradually fell to the earth even after its surface formed an abode for animals and plants, and that under favourable circumstances it collected so as to form proper stratified so-called plutonic rocks, in which, through subsequent chemical changes, so great a development of heat has sometimes taken place that volcanic and plutonic incandescent craters have arisen in the interior of the earth.

Many observed facts may be quoted in support of this view, if it for the present appears very strange on account of the great changes it would bring about in the prevailing ideas of the history of the formation of the heavenly body which we inhabit. We have perhaps here the true solution of the many disputed questions raised by the discovery of meteoric iron at Ovifak, in Greenland, a simple explanation of the abundant occurrence of magnesia in certain geological formations, and of many other geological phenomena difficult of explanation according to theories now prevalent.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Mr. W. N. Shaw, B.A., Emmanuel College, 16th Wrangler, 1876, and 1st Class Natural Sciences Tripos (Distinguished in Physics), 1876, has been elected to a fellowship in his College.

LONDON.—The following have passed the recent examination for the degree of Doctor of Science in the branches specified:—

Branch IV.—Inorganic Chemistry.—J. M. H. Munro, Royal College of Science, Dublin.

Branch VI.—Electricity (treated experimentally).—O. J. Lodge, University College.

Branch VIII.—Physical Optics, Heat, Acoustics (treated mathematically).—J. F. Main, Trinity College, Cambridge.

Branch X.—Comparative Anatomy.—A. M. Marshall, B.A., St. John's, Cambridge, and St. Bartholomew's Hospital.

Branch XIV.—Geology.—W. Saise, Royal School of Mines.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 20.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Messrs. George Alexander Gibson, Henry P. Gurney, John Higson, and Francis Stevenson, were elected fellows of the Society.—The following papers were read:—On a hitherto unnoticed circumstance affecting the piling up of volcanic cones, by R. Mallet, F.R.S.—The steppes of Southern Russia, by Thomas Belt, F.G.S.—The glacial period, by J. F. Campbell, F.G.S.—The action of coast-ice on an oscillating area, by Prof. John Milne, F.G.S., of the Imperial College of Engineering, Tokio, Japan.—On points of similarity between zeolitic and siliceous incrustations of recent formation by thermal springs and those observed in amygdaloid and other altered volcanic rocks, by Prof. A. Daubrée, F.M.G.S.—On the cretaceous Dentaliadæ, by J. S. Gardner, F.G.S.—On a number of new sections around the estuary of the Dee which exhibit phenomena having an important bearing on the origin of boulder-clay and the sequence of glacial events, by D. Mackintosh, F.G.S.—Discovery of silurian beds in Teesdale, by W. Gunn, F.G.S., and C. T. Clough, F.G.S., of H.M. Geological Survey.—On the superficial geology of British Columbia, by George Mercer Dawson, F.G.S., Assoc. R.S.M., of the Geological Survey of Canada.—The exploration of the ossiferous deposit at Windy Knoll, Castleton, Derbyshire, by Rooke Pennington, F.G.S., and Prof. W. Boyd Dawkins, by Prof. W. Boyd Dawkins, F.R.S.—Description of the fossil organic remains from Bendigo, by M. Carl August Zachariæ, communicated by the president.—Notes on some recent discoveries of copper ore in Nova Scotia, by Edwin Gilpin, F.G.S.—Glacial drift in the North-eastern Carpathians, by R. L. Jack, F.G.S., and John Horne, F.G.S., of the Geological Survey of

Scotland.—On terminal curvature in the south-western counties, by W. A. E. Ussher, F.G.S., of H.M. Geological Survey.—On the chronological classification of the granitic rocks of Ireland, by G. H. Kinahan, M.R.I.A., communicated by Prof. Ramsay, F.R.S.—The Cambrian rocks of South-east Ireland, by G. H. Kinahan, M.R.I.A., communicated by Prof. Ramsay, F.R.S.

PHILADELPHIA

Academy of Natural Sciences.—A valuable list of the fresh-water fishes of Northern Indiana, by Dr. D. S. Jordan, is published in the *Proceedings* for 1877, with remarks on many forms of novelty or interest. This is followed by a critical account of the genera of North American fresh-water fishes, by Dr. Jordan and Mr. C. H. Gilbert. One list gives the whole of the genera in the order of their original description, with full references.

VIENNA

Imperial Academy of Sciences, April 19.—Contributions to the cosmic theory of meteorites, I. Proof of identical meteorite paths, by M. Niessl. Two detonating meteorites, on April 10, 1874, in Bohemia, and April 9, 1876, in Hungary, had apparently the same point of emergence, and observations gave for both a velocity corresponding to a hyperbolic path.—On the action of alcoholic caustic potash solution on ether-like nitro-bodies, by MM. Hess and Schwab.—On the application of the microscope to quantitative determinations, by M. Jonstorff.—On the history of creation of our planetary system, &c., by M. Sedlitschka.—On some remarkable phenomena in Geissler tubes (fourth paper), by MM. Reitlinger and Urbanitzky. Seeking the causes and laws of the repulsions and attractions, they experimented with various gases rarefied in Wüllner's cylindrical tubes (without capillary part), noting simultaneously with a multiplier the changes in the induced current. They were led to the conviction that it is a case of reciprocal action between accumulation of static electricity on the approximated conductors and current electricity in the tubes, and that the chemical character of the gases has a great influence on the apparent progress of the phenomenon.

April 26.—On iron cyanide compounds, by M. Skraup. This relates to superferrid-cyanide of potassium.—On a new derivative of sulpho-urea, sulphydantoinic acid, by M. Maly.—Theory of circular polarisation, by M. V. Lang.—On *Phymatocarcinus speciosus*, Reuss, by M. Bittner.—A geological profile from Osmanich am-Arcer, on the Sveti Nikola-Balkan, to Ak-Palanka, on the Nisava, by M. Toula.

PARIS

Academy of Sciences, July 9.—M. Peligot in the chair.—The following papers were read:—On the alcoholate of chloral, by M. Wurtz. The dehydratation of crystallised oxalate of potash occurs in vapour of the alcoholate as easily as in air; not so in vapour of hydrate of chloral (proving that the latter contains water).—Reply to M. Roudaire's last note on the Algerian inland sea, by M. Naudin. He insists specially on the erosive force the current would have both in its primary state and in time of flooding. The troubled water of the coast, too, would enter and deposit much sediment.—On electric transmission through the ground by means of trees, by M. Du Moncel. Trees are all, more or less, conductors, their conductivity depending on the quantity of liquids in them. The roots act as electrodes. The resistance of a tree, commencing with its leaves, and supposing contact only with a few of them, varies from 200,000 to 400,000 kilometres of telegraph wire (in round numbers). That of the trunk, at a height of 7 to 8 m. hardly exceeds, in strong trees, 3,000 kil. in connection with the ground, and varies from 2,000 to 7,000 kil. between small metallic electrodes. Thus, contact of telegraph wires with leaves need not give much anxiety. The resistance of ordinary houses being about sixteen to twenty times that of trees, the latter, if not under the former in height, may be considered a protection, but as rain usually falls in thunderstorms and diminishes the difference of conductivity between trees and house, a protective effect of trees may only lie in their superior height.—Treatment by sulphocarbonates of vines of Orleans and Saint Jean-le-Blanc, by M. Gueyrand.—On the quasi-circular movements of a point subject to the attraction of a fixed centre, by M. Boussinesq.—On the diamagnetism of condensed hydrogen, by M. Blondlot. Palladium charged with hydrogen M. Blondlot finds to be *less* magnetic than palladium uncharged; which accords with the facts that palladium is weakly magnetic and hydrogen diamagnetic. Graham's opposite experience is thought due to some disturbing cause, probably impurity of the acid used in charging the palladium by means of electrolysis; the least trace of a ferruginous body gives a deposit on the palladium, which would explain Graham's results.—Photometric researches on coloured flames, by M. Gouy. If the quantity of salt introduced into the flame be doubled, the increase of brightness of each line is at most equal to what would be produced by doubling the thickness of the flame, and it is nearly always inferior.—On a new metal, *davyum*, by M. Kern (see p. 236).—On the oxidability of sulphide of manganese, by MM. De Clermont and Guiot.—On a new general method of synthesis of hydrocarbons, acetones, &c. Third note by MM. Friedel and Crafts.—Action of bromine on pyrotartaric acid; second memoir by M. Bourgoin.—On the determination of carbonic acid in blood serum, by M. Fredericq.—Researches on bitter almonds, by M. Portes. Young bitter almonds contain amygdaline; they have always a different composition from sweet almonds; the embryo alone contains emulsine, and it appears pretty late; the amygdaline is localised in the teguments of the seed; its origin is still unknown; by degrees it quits the teguments and penetrates into the cotyledons by the radicle.—On the nickelised iron of Santa Cattarina, by M. Lunay.—On some physiological facts observed in Drosera, by M. Ziegler. It has been observed that Drosera is sensible to the physical action of salts of quinine after excessive indirect animal contact. Many other bodies have this property, and among them is *urea*. Like quinine, urea does not cause any action in normal droseras, but on being united with certain other bodies, it produces contraction (*e.g.*, granules made of a mixture of urea and iron with white wax give contraction; but granules of wax with urea alone, or with iron alone, have no effect).—Comparative study of cupric preparations introduced into the stomach and the blood, by MM. Feltz and Ritter. Insoluble albuminate of copper ingested into the stomach in considerable quantity has hardly any effect; soluble albuminate causes disorders at least as grave as the ammoniacal sulphate in distilled water. Sulphate of copper dissolved in syrupy glycerine is much more poisonous than in aqueous glycerine.—Treatment of rheumatism, gout, and various nervous states, with salicylic acid and its derivatives, by M. Sée. It seems beneficial in some cases.—On testing for salicylic acid, by M. Marty.—On external use of salicylic acid, by M. Grellot.—The advantages of immediate and early trepanations, by M. Gross.

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THURSDAY, JULY 26, 1877

A MANCHESTER UNIVERSITY

THE movement of the authorities of Owens College, represented in their memorial to the Duke of Richmond on Friday last, is certainly one of the healthiest important educational advances of recent years. It is an outcome of an impatience of pure examination and a revived belief in the educative influences of association of young men with each other and with masters of the subjects in which they happen to be interested, which have been gradually but surely growing up among educationalists.

Like a few hundreds of other institutions, including Oxford, Cambridge, and the Scotch Universities, Owens College is affiliated to the University of London. Twenty years ago this meant that only students of these institutions could present themselves for London degrees. But their members multiplied—and every weaker member added to the list of affiliated colleges supplied a new reason for still farther widening the bounds of the University. It was natural that in time the outsiders educated at home, with private tutors, or in unrecognised and unaffiliated institutions should knock at the doors of [the university and demand admission. After they did, it was found in the end impossible to refuse them. Since that date the University examines everybody, wherever he has been educated, and its influence is to assimilate what one may call organised institutions of individuality and character to the unorganised or semi-organised types of the crammer's school or the private student of cram books. For the school or the private student it has been and still is of the utmost value. For higher colleges it is a great centralisation, objectionable only because it is not complete, not having really swallowed up the Scotch and Irish universities as well as Oxford and Cambridge. Mr. Lowe's ideal doubtless is that the most intelligent, impartial, perhaps bloodless of examiners, selected from all the world, should prescribe the subjects of examination, and should thereby dictate from the standpoint of the highest human omniscience, the courses of all teaching institutions, and should, like Minos, Æacus, and Rhadamanthus, decide in final award the character and place in the universe alike of teacher and of taught. This ideal has been realised in the Chinese empire, and many excellent arguments can be stated in its favour. It is inadequately realised in this country, because all incorporated universities have been left practically outside of the scheme. Owens College is the first of the non-incorporated actual or possible universities which asks also to be released.

It is possible to offer the petitioners something they do not seek. Owens and other residence colleges object to be controlled by a mere examining board. But there are Universities like Oxford and Cambridge which are not mere examining boards, which have resident students, where the examiners are in frequent and living contact with the students, from whom, indeed, they are often removed only by a few additional years. Five years before, the men who preside over the triposes may have them selves sat for examination. Might not Oxford or Cambridge supply a less narrow and technical examination than London? Could these universities not affiliate Owens College to themselves in some more living bond than has now become possible elsewhere?

There is an excellent and all but final answer. Oxford was asked to do it for King's College, London, and after mature deliberation it deliberately declined. Probably it could not see its way to work out this more living association. There is one kind practicable—such as Oxford and Cambridge practise between themselves. A man may count a certain number of terms at Cambridge for his degree at Oxford, just as he may count so many sessions at Edinburgh for his degree at Glasgow. There is another kind conceivable. The teachers of both places may be associated with each other, with or without outside examiners, for the examination of the students of both, and the programmes of examination, and to some extent of teaching, may thus be settled in common. Oxford was not willing to associate itself with King's College in either of these ways, and there is no reason to believe that it would care so to associate itself with Owens College or any other institution. Such an association would imply an equality which the older universities can scarcely be asked to admit; and the second mode of association would institute a sort of outside interference with them which they would never allow. The simplest and most satisfactory university is self-contained, teaching and examining its own men under the stimulus of rivalry and public criticism, and with the help, perhaps, of outside examiners. Oxford and Cambridge see no reason why they should descend from their own secure and satisfactory position and tempt the dangers of confederation.

It remains to ask what good it can and what harm it might do to grant the prayer of the petitioners. The first question need scarcely be answered. It is an obvious advantage to every district to have a great centre of high thinking and noble living planted in its midst. But there is an equally obvious disadvantage in the undue multiplication of universities. There may be serious objections, as Prof. Huxley puts it, to any official system of branding our young herrings with B.A. or M.A. But so long as we keep to the system of branding we ought not to be too free with our brands. There ought not to be too many, and there are a good many in the United Kingdom with no means of telling which is a good brand and which is a bad one. There is no doubt that, as far as it goes, the objection is sound, and that we ought not to have too many degree-granting bodies. The question is whether we have—whether the precise limit at which we ought to stop has been reached—whether the new claimant is not as fully entitled as some of the old-established institutions to an independent status and existence. In England we have very few branding bodies, and every guarantee is offered by the Manchester authorities that their brand will be of the very first quality. They propose to have outside assessors to help them to see to it; the one thing they ask is that having the responsibility of teaching they shall have an equal share in directing the examinations. It seems a reasonable and modest request.

THE KINETIC THEORY OF GASES

A Treatise on the Kinetic Theory of Gases. By Henry William Watson, M.A., formerly Fellow of Trinity College, Cambridge. (Oxford : Clarendon Press, 1876.)

THIS book does not profess to treat of all that has been written about the kinetic theory of gases. It discusses the ultimate average condition of a material system, consisting of a very great number of parts in motion within a confined space, and it follows for the most part the methods of investigation given by Boltzmann. The discussion is arranged in the form of thirteen propositions, in which the different cases are considered in the order of their complexity. In the earlier propositions the moving bodies are supposed to be rigid-elastic spheres acting on each other only by impact, afterwards external forces are introduced, and finally the bodies are supposed to be material systems, the parts of which are held together by any system of forces consistent with the principle of the conservation of energy.

The ultimate average condition of such a system is investigated in a very satisfactory manner in this book. No part of mathematical science requires more careful handling than that which treats of probabilities and averages. Mathematicians, whose competence to deal with other questions is undoubted, have fallen into errors in treating of probabilities, and even the validity of certain methods of proof is still apparently an open question.

Besides this, some of the consequences to which these theorems lead us are so startling that we are not prepared to admit them without an unanswerable proof, and of the investigations already given, some are so short and incomplete, and others so long and roundabout, that it requires no ordinary exercise both of penetration and of patience to find out whether they are proofs at all. Mr. Watson has conferred a great benefit on the students of the kinetic theory by placing before them in a series of distinct propositions, none of them too long for the mind to grasp, all the necessary steps leading to the result, and none of the superfluous evolutions in which the mental energy of the student is so often dissipated. The book, as we have said before, is confined to the investigation of the ultimate average condition of the system, and does not discuss the processes of diffusion by which that ultimate condition is attained, such as the inter-diffusion of gases, the diffusion of momentum by viscosity, and the diffusion of energy by thermal conduction. These have been recently treated in a larger work,[1] to which we may have occasion to refer.

There are two very different methods of defining and investigating the state of a complex material system. According to the strict dynamical method the particles of the system are defined in any sufficient manner, as, for instance, by their co-ordinates at a given epoch, and the position of any particle at any other time is then defined by its co-ordinates, expressed as functions of the time, the form of these functions being different from particle to particle, and not necessarily continuous in passing from one particle to another which was contiguous to it in the initial configuration.

According to this method our analysis enables us to trace every particle throughout its whole course, and therefore we can apply the laws of motion in all their strictness. The application of this method to systems consisting of large numbers of bodies is out of the question. We therefore make use of another method which we may call the statistical method, on account of its analogy with the methods employed in dealing with the fluctuations of a large population.

We divide the bodies of the system into groups according to their position, their velocity, or any other property belonging to them, and we fix our attention not on the bodies themselves, but on the *number* belonging at any instant to one particular group. This number is, of course, subject to change on account of bodies entering or leaving the group, and we have therefore to study the conditions under which bodies enter or leave the group, and in so doing we must follow the course of the bodies according to the dynamical method. But as soon as the process is over, when the body has fairly entered the group or left it, we withdraw our attention from the body, and if it should come before us again we treat it as a new body, just as the turnstile at an exhibition counts the visitors who enter without respect to what they have done or are going to do, or whether they have passed through the turnstile before.

The first mode of grouping the bodies of the system is to class those together which, at a given time, are in a given region of space. This is called grouping according to configuration, and what we learn from it is the distribution of the positions or co-ordinates of the bodies in space.

The second mode of grouping is that according to velocity. The best way to understand this is to suppose a diagram of velocities constructed by drawing from a given point as origin a system of vectors representing in direction and magnitude the velocities of the different bodies. The extremities of these vectors are called the velocity-points of the bodies to which they correspond, and by grouping the bodies according to the regions of the diagram in which their velocity-points lie, we learn from the numbers in the groups the distribution of velocities among the bodies.

In like manner we may form groups defined in any other way, as, for instance, those pairs of bodies whose distance from one another lies between given limits, and by confining within sufficiently narrow limits the values of all the properties of the bodies which form the group, we may consider all the bodies belonging to the group as practically in the same state. Whether at a given instant any body actually belongs to the group is, of course, another question.

The object of study in the statistical method is the probable number of bodies in each group. We may get rid of the idea of probability by supposing the system to continue under the same conditions for a very long time. During this time many bodies will enter the group, stay in it for a certain time, and then leave it. If we add together the times of residence within the group af all these bodies, and divide the sum by the whole time of observation, we obtain a numerical quantity which we may call the average number of bodies in the group. The longer the time of observation, the nearer does this number approach to what we have called the probable number of bodies in the group.

The average number of bodies in a group depends on the limits which define the group, being, of course, greater when these limits are wide than when they are narrow. But it also depends on the character of the group, that is to say, the particular set of mean values of the conditions which entitle a body to be ranked in the group.

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[1] "Die kinetische Theorie der Gase in elementarer Darstellung, mit mathematischen Zusätzen. Von Dr. Oskar Emil Meyer, Professor de Physik an der Universität Breslau. (Breslau, 1877.)

It appears from the investigation that if ϕ be any property of a body, such that if ϕ_1 and ϕ_2 are its values or two bodies before an encounter, and Φ_1 and Φ_2 its values after the encounter, and if under all circumstances $\phi_1 + \phi_2 = \Phi_1 + \Phi_2$, and if the number of bodies in each group varies as $e^{-h\phi}$, then the distribution of the bodies in the groups will not be altered by the encounters between the bodies.

Now if we make ϕ equal to the sum of the kinetic and the potential energy of each body, the quantity $\phi_1 + \phi_2$ is not altered, either by an encounter between the two bodies or by external forces acting on them; so that a distribution according to the values of the function $e^{-h\phi}$ will satisfy the condition of permanence.

The most general case is that given in the seventh proposition. The bodies are no longer supposed to be smooth rigid-elastic spheres, but molecules, that is to say, material systems consisting of any number of parts acting on each other with forces of any kind consistent with the principle of the conservation of energy. The molecules of any one kind are supposed to have m degrees of freedom, this number being, in general, different in different kinds.

It is also assumed in the enunciation that all the forces in the system are either forces tending to fixed centres and functions of the distances from these centres, or else forces acting between the parts of the same molecule, thus excluding forces acting between one molecule and another except during the encounter of two molecules. This restriction, however, does not appear necessary, and indeed it is easy to remove it.

For the result of the proposition is to prove that if we define the group (A) of molecules as consisting of those whose generalised co-ordinates (q) are between certain limits (q and $q + dq$), and whose generalised momenta (p) are between certain other limits (p and $p + dp$), then the average number of molecules in the group is—

$$A e^{-h(\chi + T)} dp_1 \dots dp_m\, dq_1 \dots dq_m,$$

where A is a constant which is the same for all groups of molecules of the same kind, but is different for different kinds of molecules in the same mixture, but h is the same for all kinds of molecules. χ is the potential energy, and T the kinetic energy of a molecule when in the state (A), and $dp_1 \dots dp_m$ are the differentials of the components of momentum, and $dq_1 \dots dq_n$ the differentials of the co-ordinates. The continued product of these differentials specifies the extent of the group.

By integrating this expression with respect to any one of the variables, we may ascertain the average number of molecules in a larger group, in which that variable does not form a ground of subdivision. For instance, if we integrate with respect to all the co-ordinates, we arrive at a group consisting of all the molecules whose momenta are between certain limits, or by integrating with respect to the momenta we form a group of molecules whose configuration lies within certain limits.

In this way we obtain two very important results:—

1. The average kinetic energy of a molecule is $\frac{m}{2h}$ where m is the number of degrees of freedom of the molecule. This is independent of the position of the molecule.

2. The average number of molecules whose configuration lies between certain limits is—

$$A e^{-h\chi} dq_1 \dots dq_n,$$

where χ is the potential energy of the molecule, arising from forces either internal to the molecule or tending to fixed centres, but (according to Mr. Watson) excluding intermolecular forces.

But as our definition of a molecule is of the most general kind, nothing is easier than to take into account any intermolecular forces by simply including within our "molecule" all those molecules between which intermolecular forces are exerted.

For instance, there is nothing to prevent us from defining as a molecule a material system consisting of one atom in Sirius, another in Arcturus, and a third in Aldebaran. If the universe is supposed to have attained that condition of thermal equilibrium to which alone these propositions apply, the average kinetic energy of each of these atoms will be $\frac{3}{2h}$, because each has three degrees of freedom.

That of the system of three atoms will be the sum of the kinetic energies of the three atoms, namely, $\frac{9}{2h}$. We might obtain the same result from the consideration that this system has nine degrees of freedom.

The centre of mass of the three atoms is a mathematical point at an immense distance from any of them. It has, of course, three degrees of freedom, and the kinetic energy of a material particle whose mass is the sum of the masses of the atoms and which moves as the centre of mass does is $\frac{3}{2h}$.

The value of the kinetic energy of the centre of mass will be the same for any system of atoms provided that every atom of the system is liable to encounters with atoms not belonging to the system. Of course if we take into our "molecule" all the atoms of a material system unconnected with any other system, its centre of mass will not be agitated at all by the mutual actions of the atoms during their encounters.

And here we must notice a point to which Mr. Watson has adverted only in a note at the foot of p. 20—the definition of the motion of the medium as distinguished from the motion of agitation of the molecules. For this is connected with the weak point of the demonstration and shows us the way to strengthen it.

The weak point of the demonstration is the tacit assumption that the sum of the potential and kinetic energies of a pair of molecules is the only function which does not change during their encounter. For there are other quantities which are not altered by the mutual action of two bodies, such as their masses themselves, the sum of their momenta resolved in any given direction, and their angular momenta about any fixed axis.

Hence if instead of $T = \frac{1}{2} M (u^2 + v^2 + w^2)$ we write in the expression for the distribution of velocities—

$$\mathfrak{T} = T - M \; \{Uu + Vv + Ww + p\,(wy - vz) + q\,(uz - wx) + r\,(vx - uy) + CM,$$

the distribution of velocities will still be a permanent one.

In this expression the quantities U, V, W, p, q, r, may have any values provided they are the same for the whole system of bodies, but C may be different for different bodies, because it is multiplied by the mass of the body, which is invariable. But we arrive at the same expression by substituting in T for u, v, and w the quantities—

$$\begin{aligned} u &- U + qz - ry \\ v &- V + rx - pz \\ w &- W + py - qx, \end{aligned}$$

or, in other words, by substituting for the absolute velocities of the bodies their velocities relative to a system of axes moving in the most general manner possible; that is to say, the components of velocity of the origin being U, V, W, and the components of the velocity of rotation being p, q, r, and at the same adding to T the quantity

$$\tfrac{1}{2} M \left\{ (qz - vy)^2 + (rx - pz)^2 + (py - qx)^2 \right\} - C',$$

which depends on the co-ordinates only and not on the velocity of the body.

We now see that the most general case of permanent distribution is when the system of bodies is contained in a vessel of invariable form which moves with constant velocity along a screw, that is to say, in which one point is moving along a straight line with constant velocity, while the vessel rotates about an axis passing through this point with constant angular velocity.

When there is rotation, we must subtract from the potential energy a term depending on the co-ordinates, which shows that the rotation produces an effect similar to that of a centrifugal force at right angles to the axis of rotation.

Returning to the general expression for the number of molecules in a group, we may make it yield us information of other kinds. Thus, if we wish to know the density of a particular gas at any given point in the mixture, we have only to make the limits of the group those of an element of volume, and we find the density proportional to $e^{-h\chi}$, where χ is that part of the energy of a single molecule which is due to external forces, such as gravity. In the case of gravity, χ is equal to mgz, where m is the mass of a molecule, g the intensity of gravity, and z the height. This leads to the ordinary expression for the density of a gas of uniform temperature in a vertical column, and it shows that in the ultimate distribution of a mixture of gases the density of each gas diminishes with the height according to its own law, that is to say that of the heaviest gases diminishes most rapidly, so that the proportion of the heavier gases diminishes with the height.

This law of the distribution of gases was asserted by Dalton as a consequence of his theory of gases, and numerous experiments have been made on air collected at different heights in the atmosphere in order to detect a difference in their composition, but we cannot say that such a difference has as yet been satisfactorily established.

The atmosphere, in fact, is eminently unfitted for testing the theory of the ultimate state of a mixture in equilibrium, for the inequalities of temperature in so large a body of gas produce powerful currents which continually carry masses of the mixture from one stratum into another. This tends to produce a uniformity of composition and a variation of temperature which are both of them contrary to our theory of the condition of equilibrium, and which seem to favour certain other theories.

Nor is the case much improved if, instead of the open atmosphere, we substitute a mixture of gases contained in a vertical tube. For in order to obtain a difference of composition at the top and bottom of the tube large enough for experimental verification, the tube must be at least 100 metres high, and it would take more than a year for the contents of such a tube to approximate by one half to their final distribution. In the mean time the slightest difference of temperature in the sides of the tube would produce currents which would tend to equalise the composition of the mixture. To verify the other result of our theory—the uniformity of temperature in the ultimate state of a vertical column—would be attended with still greater difficulties.

But it would be quite within the powers of experimental methods to verify the law of distribution of a mixture of gases in a rotating vessel. Let two bulbs be connected by a wide tube, say 10 cm. long, and let them be filled with equal volumes of hydrogen and carbonic acid, well mixed together. Let this apparatus be placed on a whirling machine, so that one bulb shall be close to the axis, while the other is moving at the rate of fifty metres per second. The same degree of approximation to the final state, which would take years in the long tube, will be effected in minutes in this small apparatus, and the proportion of carbonic acid to hydrogen will be about $\frac{1}{140}$ greater in the bulb furthest from the axis.

The clear demonstration of this proposition given by Mr. Watson is of great scientific value, for almost every one of those who have attacked the question with insufficient methods of investigation have come to the conclusion that the temperature would diminish in a vertical column as the height increases; and those who regard gaseous diffusion from a chemical rather than a dynamical point of view would probably expect the composition to be uniform at all heights.

But the profound scientific value of this proposition becomes more manifest when we make use of it in establishing the definition of temperature and the law of volumes of gases.

In Prop. II. of this book, which corresponds to the original form of the theorem, as I gave it in the *Philosophical Magazine*, January, 1860, two sets of spheres are completely mixed up together in the same vessel, and it is proved that the average kinetic energy of a sphere is the same for either set. We may then assert, as I did, that the two gases are at the same temperature because they are thoroughly mixed together. But this assertion has no scientific meaning, because we cannot test its truth by putting a thermometer first into the one gas and then into the other.

But if we now call to our aid a system of forces acting on the molecules and tending to fixed centres, we may obtain a result capable of experimental verification; for though we are not acquainted with natural forces acting exclusively on one kind of gas, we can calculate the effects of such forces.

Let us assume, then, that the forces are such that the potential energy of a sphere of the set N is much greater in one part of a vessel, which we shall call B, than when it is in the part A, these two parts being separated by a stratum, C, within which the potential varies continuously. The medium consisting of the spheres N will be dense in A, it will become rarer in the stratum C, and there will be hardly any of these spheres in B.

Now let the potential energy of a sphere of the set N' be much greater when it is in A than when it is in B, and let it vary continuously from the one value to the other in the stratum C. Then the spheres of this set will be thickly scattered in B, will thin out in the stratum C, and will be very rare in A.

The two sets of spheres are thus kept in great measure separate in A and B, while free to exchange their kinetic energy by collisions within the stratum C.

Now by definition, the temperatures of two bodies are equal if, when the two bodies are placed in contact, their thermal state remains the same. We cannot apply this definition to the two sets of spheres in Prop. II., for they were inextricably mixed up together, but we have now got them almost completely separated from each other into two distinct regions. They are therefore practically distinct bodies, and we can test their temperatures separately.

Hence the statement, that the temperatures of two gases are equal when the kinetic energy of the centre of mass of a molecule is the same in each, is true, not only of gases mixed together, but of two pure gases in different parts of the same vessel.

If we assume that a powerful external force acts on each molecule tending to a fixed centre belonging to that molecule, each molecule will always remain very near its own fixed centre of force, and the assemblage of molecules will behave like a solid body. But forces of this kind are included among those considered in Prop. IV., so that the relation between temperature and the kinetic energy of the centre of mass of a single molecule must be extended even to solids.

Returning once more to the general expression for the average number of molecules in a group, we may make it yield us information with respect to the average number of sets of molecules which, at a given instant, are in a given configuration with respect to each other.

For instance, if two molecules act on each other, and if χ is the potential energy due to this action corresponding to a distance r, then the number of pairs of molecules whose distance is between r and $r + dr$ will be proportional to $r^2 e^{-h\chi}$. In the case of attraction, χ is negative, so that there will be a greater number of pairs of molecules within these limits of distance than there would have been if they did not attract each other. In the case of repulsion, χ is positive, so that the repulsion diminishes the number of pairs within the distance of repulsion. If the potential energy of a pair of molecules rapidly increases to an enormous value when the distance between their centres becomes less than a given quantity, the number of pairs which are within the given distance will be practically zero, and the molecules will behave like smooth rigid-elastic spheres.

By making the "molecule" include three or more molecules, and making χ the potential energy of this system, we may extend the theorem to the simultaneous encounter of three or more molecules, so that these cases, which were formally excluded in the earlier propositions, do not in any way interfere with the absolute generality of the final result.

The clear way in which Mr. Watson has demonstrated these propositions leaves us no escape from the terrible generality of his results. Some of these, no doubt, are very satisfactory to us in our present state of opinion about the constitution of bodies, but there are others which are likely to startle us out of our complacency, and perhaps ultimately to drive us out of all the hypotheses in which we have hitherto found refuge into that state of thoroughly conscious ignorance which is the prelude to every real advance in knowledge.

If we know from observation either the specific heat of a gas at constant pressure, or the ratio of its specific heats at constant pressure and at constant volume, we can determine the ratio of the rate of increase of its total energy to the rate of increase of the energy of agitation of the centres of its molecules. Now if the molecule has m degrees of freedom, its total kinetic energy is to the energy of agitation of its centre of mass as m to 3. It is probable that the internal potential energy of the molecule increases as the temperature rises, and this would make the ratio of the whole energy to that of agitation of centres greater than that of m to 3, so that if we know this ratio by experiment, we can assert that m cannot exceed a certain value.

For chlorine, ammonia, and sulphuretted hydrogen, m cannot exceed 6; for hydrogen, oxygen, nitrogen, air, carbonic oxide, nitrous oxide, and hydrochloric acid, it cannot exceed 5, and for mercury gas, according to the experiments of Kundt and Warburg, it cannot exceed 3.

Now Boltzmann has pointed out in a paper: "Über die Natur der Gasmolecüle" (Vienna Acad., December 14, 1876), that if the molecules were rigid-elastic bodies of any form, m would be 6, that if they were smooth figures of revolution, the velocity of rotation about the axis of figure would not be affected by the collisions, so that m would be 5, and that if they were smooth spheres, the three component velocities of rotation would each of them be independent of collisions, so that m would be reduced to 3, and these values are in striking agreement with the phenomena of the three groups of gases.

But before we accept this somewhat promising hypothesis, let us try to construct a rigid-elastic body. It will not do to take a body formed of continuous matter endowed with elastic properties, and to increase the coefficients of elasticity without limit till the body becomes practically rigid. For such a body, though apparently rigid, is in reality capable of internal vibrations, and these of an infinite variety of types, so that the body has an infinite number of degrees of freedom.

The same objection applies to all atoms constructed of continuous, non-rigid matter, such as the vortex-atoms of Thomson. Such atoms would soon convert all their energy of agitation into internal energy, and the specific heat of a substance composed of them would be infinite.

A truly rigid-elastic body is one whose encounters with similar bodies take place as if both were elastic, but which is not capable of being set into a state of internal vibration. We must take a perfectly rigid body and endow it with the power of repelling all other bodies, but only when they come within a very short distance from its surface, but then so strongly that under no circumstances whatever can any body come into actual contact with it.

This appears to be the only constitution we can imagine for a rigid-elastic body. And now that we have got it, the best thing we can do is to get rid of the rigid nucleus altogether, and substitute for it an atom of Boscovich—a mathematical point endowed with mass and with powers of acting at a distance on other atoms.

But Boltzmann's molecules are not absolutely rigid. He admits that they vibrate after collisions, and that their vibrations are of several different types, as the spectroscope tells us. But still he tries to make us believe that these vibrations are of small importance as regards the principal part of the motion of the molecules. He compares them to billiard balls, which, when they strike each other, vibrate for a short time, but soon give up the energy of their vibration to the air, which carries far and wide the sound of the click of the balls.

In like manner, the light emitted by the molecules shows that their internal vibrations after each collision are quickly given up to the luminiferous ether.

If we were to suppose that at ordinary temperatures the collisions are not severe enough to produce any internal vibrations, and that these occur only at temperatures like that of the electric spark, at which we cannot make measurements of specific heat, we might, perhaps, reconcile the spectroscopic results with what we know about specific heat.

But the fixed position of the bright lines of a gas shows that the vibrations are isochronous, and therefore that the forces which they call into play vary directly as the relative displacements, and if this be the character of the forces, all impacts, however slight, will produce vibrations.

Besides this, even at ordinary temperatures, in certain gases, such as iodine gas and nitrous acid, absorption bands exist, which indicate that the molecules are set into internal vibration by the incident light.

The molecules, therefore, are capable, as Boltzmann points out, of exchanging energy with the ether.

But we cannot force the ether into the service of our theory so as to take from the molecules their energy of internal vibration and give it back to them as energy of translation. It cannot in any way interfere with the ratio between these two kinds of energy which Boltzmann himself has established. All it can do is to take up its own due proportion of energy according to the number of its degrees of freedom.

We leave it to the authors of the "Unseen Universe" to follow out the consequences of this statement.

J. CLERK MAXWELL

OUR BOOK SHELF

Report on the Progress and Condition of the Royal Gardens at Kew during the Year 1876. (Clowes and Sons.)

SIR JOSEPH HOOKER'S annual report on the Royal Gardens, Kew, for 1876, has just been issued. It is a pamphlet of some thirty-three pages, and is a considerable improvement on the reports of former years. It deals most fully with new plants of economic interest, whether such have been actually received or sent from the Royal Gardens, or have formed the subject of correspondence with foreign or colonial governments. It is eminently satisfactory to know that such useful plants as the Para rubber (*Hevea brasiliensis*), the ipecacuanha (*Cephælis ipecacuanha*), the Liberian coffee (*Coffea liberica*), and others, have been most successfully introduced into India and other countries, through the instrumentality of Kew. Of the 70,000 seeds of the *Hevea* received at Kew about the middle of June last year, all of which we are told were at once sown, and though closely packed together, covered a space of over 300 square feet so soon as August 12th following, upwards of 1,900 living plants, raised from these seeds, were transmitted to Ceylon in thirty-eight Wardian cases, 90 per cent. of the whole consignment reaching Dr. Thwaites in excellent condition. So rapid was the germination of these seeds at Kew that some had actually started into growth on the fourth day after sowing, and many in a few days reached a height of eighteen inches. It has been arranged that these young plants shall "be nursed and established in Ceylon for subsequent transmission through the Indian Gardens to Assam, Burma, and other hot damp provinces of India proper." Besides those sent to India, smaller quantities of plants have also been despatched to the west coast of Africa, Burma, Dominica, Jamaica, Java, Queensland, Singapore, and Trinidad. With regard to ipecacuanha, though Dr. King reports that he fears it cannot be grown so far north in India as Bengal, it is nevertheless in some situations capable of rapid and extensive cultivation, and the roots grown in India have been proved to be quite as efficacious in a medicinal point of view as those from the best districts of South America. In the matter of Liberian coffee, the wide and general extension of this new kind in coffee-growing countries bids fair, in many parts, to entirely supersede the old and better known *Coffea arabica*. Sir Joseph Hooker reports the receipt of numerous favourable notices of the plant, and quotes "two from opposite sides of the world," namely Ceylon and Dominica. With reference to diseases affecting coffee plants—which it is hoped the more sturdy habit of the Liberian kind will help it in some measure to resist—a very exhaustive notice is furnished, which is not only of much interest in a scientific point of view, but cannot fail to be valuable to coffee-planters themselves. It will, moreover, no doubt be the means of causing more careful observations to be made by residents on coffee estates or in coffee-growing countries into the nature and habits of diseases which are still more or less obscure.

Considerable additions are reported to the Museums and Herbarium, the new building for the accommodation of the latter collection being now in a very advanced state. The new Laboratory, which has been erected at the expense of T. J. Phillips Joddrell, Esq., is reported as having been completed during the year, and though not fully provided with the necessary equipment at the time the report was written, has been already, as our readers are aware, used by Dr. Tyndall in several of his recent experiments and researches.

Two new features of the report which we have not already mentioned are—first, the introduction of plates, one being a figure of the new Liberian coffee plant, and the other a view and ground-plan of the Laboratory; and second, the publication of the report, at a charge of sixpence, by Messrs. Clowes and Sons.

Natural History Transactions of Northumberland and Durham, vol. v., part 3. (Williams and Norgate, 1877.)

THIS part is by no means the least valuable of these transactions; on the contrary, it will rank high, owing to the contributions of Dr. Embleton and Mr. Atthey on the structure of the Labyrinthodonts, and the eight excellent plates by which their papers are illustrated. The illustrations of Loxomma and Anthracosaurus are as complete and instructive as any that have yet been published of British Labyrinthodonts. The authors, however, do not recognise the articular surfaces on the exoccipitals of Loxomma as the two condyles; and they speak of a concave articular surface as taking the place of a condyle or condyles on the basioccipital bone. The condyles in all Amphibia are produced by the exoccipital bones, and such a character is not a special evidence of the affinity of Loxomma with fishes. The number also contains several interesting papers on local natural history and antiquities, and the address of the president, the Rev. G. Rowe Hall.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The "Inflexible"

MY attention has been called to an article on the *Inflexible*, in NATURE (vol. xvi., p. 201), and I shall be much obliged by your inserting a few remarks, which I shall make as short as possible. On the general subject of the article I do not propose, nor would it be proper for me, to say a word. I am concerned only with the concluding remarks of the writer on a letter of mine to the *Times*. Nothing has appeared to me more astonishing than the use, or rather abuse, which is occasionally made of the report of Lord Dufferin's committee on ships' designs. If their authority can be claimed for any statement, I see on all sides a readiness to claim it. Should anything they have said militate against a favourite view, their authority is depreciated, and a comparison is sometimes invidiously drawn between the supposed opinion of the majority and that of an intelligent minority. Now if I be included with the unintelligent majority, I am quite content to find myself in such good company; but if, on the contrary, I am included in the minority, I utterly and absolutely refuse a compliment at the expense of my distinguished colleagues, with whom I shall always esteem it no small honour to have served. In fact I am not aware of any scientific point on which that committee was not unanimous. The writer of the article in question in common with many others, seems to have entirely mistaken the position of that committee. He seems to think their duty was to make their report a treatise on Naval Architecture. The absurdity of such a notion is apparent on the face of it. In fact they were required to give an opinion on certain designs of ships submitted to them as to their being in accordance with the latest developments of the theory of naval architecture. It was no part of their duty to descant on the principles which were successfully applied in such designs; but undoubtedly if they observed that in any direction caution was necessary, they were bound to remark it. In none of the designs was there any indication of a tendency to curtail initial and maximum stability of their due proportions; had there been they would certainly not have failed to call attention to it. But while they found the design of the *Devastation* in all respects sound, they yet thought it desirable that the range of stability should in future designs be somewhat enlarged. In recommending such enlargement they by no means committed themselves to any such absurd *dictum* as the writer imagines—that range of stability is all that is requisite for the safety of a ship. But as I have already said, to have laid down all the other requisites of a good ship would have been to write a treatise.

Again, whatever credit according to some, or discredit according to others, is due to the design of a ship like the *Inflexible* with an armour-plated central citadel with unarmoured bow and stern, that credit or discredit cannot be justly imputed to the committee. Mr. Reed, in his evidence, had brought a design with some of the leading features of such a ship before them, and it occupied a considerable share of their attention. Now what do they say on this subject?—"It is not by any means certain that some method may not be devised of securing the requisite reserve buoyancy by other means than armour plating." And after giving a sketch of what such a ship would be, they conclude thus:—"In the absence of any practical experience of the effect of large shells or of torpedoes upon such a structure as we have in view, it is impossible to say with confidence that the object aimed at would be thus attained, but, *if it were*, consequences of so much importance and value would follow that we think it right to *indicate this line of inquiry as worthy of experimental investigation.*"

How far such a bare suggestion of experimental inquiry is from the recommendation of such a structure for adoption must be evident to your readers without further comment.

United University Club, Pall Mall, JOSEPH WOOLLEY
July 20

[The above letter from Dr. Woolley is what might have been expected from a man of his eminence in the science of naval architecture, writing under the restraint of his nomination by the Government to a membership of the Committee which is to report upon the stability of the *Inflexible*. It is no doubt to the concluding words of our first article on this subject (NATURE, vol. xvi. p. 203) that Dr. Woolley's letter refers, and we at once admit that there is very great force in the argument which he now employs. The particular point in question is a very simple one. In his letter published in the *Times* of July 19, Mr. Barnaby wrote:—"According to our estimate the ship, when fully armed, stored for fighting, and manned, will have, independently of the unarmoured ends—*i.e.*, supposing them not to exist—a range of stability of 48 deg. The Committee on Designs considered that 40 deg. was sufficient range for a seagoing unmasted ship." On the following day a letter appeared from Mr. Reed commenting on the impropriety of assuming the *non-existence* of the ends, pointing out that it was 50 deg. and not 40 deg. that the Committee spoke of as the *minimum* angle of vanishing stability, and adding that when the Committee put forward "*range* of stability" as "the one measure of safety" to be considered, "they stated the most dangerous doctrine which probably has ever been propounded in connection with the science of naval architecture." Now, on reconsidering the whole question, we are inclined to think that these words were not, in point of fact, quite fair to the Committee, because there was probably no member of the Committee who would have asserted or admitted that "range" was the one and only measure of safety to be considered. Dr. Woolley, Mr. Froude, Sir W. Thomson, and probably some other members of the Committee, doubtless knew perfectly well that the length of G Z from point to point was not only as important as "range," but far more important in all cases of limited range; and it is now obvious, with the present letter of Dr. Woolley before us, that the absence of any reference to the fact is attributable to the limited extent of the Committee's inquiry. There is great force in the remark that it was no part of the duty of the Committee to compose a treatise on naval architecture. On the other hand we are bound to deny that our remarks were penned under a contrary impression. Our view is that the use to which Mr. Barnaby has put the Report of the Committee proves that the scientific men who composed it would have done well to have employed more guarded language, and to have recognised in some manner the insufficiency of range only as a measure of safety. When they are found speaking of a certain angle of vanishing stability as being "sufficient to ensure the safety" of ships, it must be admitted, even by Dr. Woolley and his colleagues, that some risk of misconstruction was incurred. That misconstruction, or perhaps we ought in this case to say misuse—or even "abuse," as Dr. Woolley expresses it—has occurred in the present case is manifest, because Mr. Barnaby seized hold of the Committee's dictum as to range, and ignored altogether the very serious question of the amount of the stability. What makes the matter more important than usual in the present case is that the curve of stability due to the citadel of the *Inflexible* only is, no doubt, a low and flat curve, G Z being everywhere so small that in order to bring the stability up to a safe amount its range would have to be very greatly extended. It was, no doubt, improper of Mr. Barnaby to make the use he did of the Committee's words, and Mr. Reed took no pains to credit the Committee with anything beyond what was written; but Dr. Woolley is, we think, a little forgetful of the fact that what Mr. Reed wrote, and what we have since written, has had to be said in presence of the circumstance that in a matter of the gravest public importance a free use of the Committee's words has been made by a high authority for the purpose of claiming for the *Inflexible* public confidence in her stability on the ground of range only. It is satisfactory to learn, however, on the undoubted authority of Dr. Woolley that the Committee, whatever its language, not only inetnded to give no countenance to the doctrine that the *Inflexible* would be proved safe if only she were shown to possess sufficient range of stability, but individually and collectively would consider such a doctrine as altogether absurd.

On the second portion of Dr. Woolley's letter, we entirely concur with him. We have read over again both the evidence and the reports of the Committee of Designs, and we cannot find the smallest justification for the assumption that, right or wrong, the Committee on Designs is responsible for this ship's design. The case to the contrary is absolutely clear and unquestionable. Mr. Reed placed before the Committee the outline ideas of a ship of this description, but making it a *sine quâ non*—let it in justice to him be said—that the ship should not depend "in the least degree" upon the ends, and that the stability of the citadel should be so ample as "to make it a matter of perfect indifference how much the ends might be knocked about by shot and shell." He spoke of the ends as being filled with water, and thus converted into a sort of tanks, and it most naturally occurred to the committee to suggest whether cork or some metallic cellular material, might not with advantage be employed to take in some degree the place of water, a proposal which Mr. Reed thought well worth consideration and trial. Beyond this the Committee did not go in their report, as the quotations cited by Dr. Woolley clearly show; on the contrary, by recommending the course of experimental investigation which they advised they plainly showed that, in their opinion, sufficient grounds for depending upon cork, &c., for stability did not exist, and could not be shown to exist except by large and well-considered experiments. Mr. Barnaby roundly asserts that the Committee "did not agree with Mr. Reed's view as to the necessary dependence of the ship upon her armoured citadel for her floating power;" but the extracts from the Committee's Report which he adduces in support of the statement by no means bear it out. The Committee, for some reason or other, advert to Mr. Reed's plan without mentioning his name, but, while nowhere implying any dissent from his main principle, they plainly enough indicate that armour should be employed to sufficiently protect buoyancy and stability, unless "other means than armour-plating" could be found and proved effectual.

We shall defer to Dr. Woolley's very proper wish to restrict his remarks to the two points above considered, and shall in no way seek to connect them with the general question upon which he has been appointed a judge. We may be permitted to observe, however, that whatever the result may be, it is a satisfaction to us to find that the Committee consists of gentlemen who are in large part not merely masters of the science of the stability of floating structures, and raised high by their individual repute above the suspicion of partisanship, but who also, by serving on the Committee of Designs of 1871, acquired very special fitness for promptly considering the *Inflexible* case. They will know how to go directly to the questions at issue, and after ascertaining what stability the ship actually possesses without aid from cork, or canvas, or other devices, and what she possesses with such aid, they will be able to declare with scientific confidence and precision whether it is or is not sufficient, for they are themselves the authors of the very standards by which that issue must be decided. Nor will they forget that whatever demands for stability existed in 1871, still greater demands now exist when we have the First Lord of the Admiralty, in his place in Parliament, claiming for this very ship the ability to float and fight even after three successive blows from Whitehead torpedoes. If the result should be a disproof of our views of the subject, we know that that disproof will be based upon scientific grounds that will commend themselves to impartial minds. If the result should be to require that additional stability shall be provided in such ships, a great public good will have been accomplished. We need not say which result we anticipate.—Ed. Nature.]

The Manufacture of Leading Articles

There is a good old story told of a country editor who once met a pressing demand for copy in a singularly ingenious manner. At the moment of going to press, it was found to the consternation of the printer that a whole column was lacking. What was to be done? The whole staff was in confusion at the unexpected discovery; the editor alone preserved his wonted coolness. Sending for a copy of the *Times*, he clipped therefrom one of the leaders and ordered it at once to be set up in type, prefaced by the words "What does the *Times* mean by this?"

This story recurred to me with some force on reading on the front page of *Land and Water* last week, an article on Soldiers' Food in War; for the original, bearing my signature, appeared on the front page of Nature (vol. xvi. p. 157). In this case, however, my other self seems to have had more time on his hands than the country editor, since the article in question has been paraphrased in parts, still with such care as not to destroy the identity. H. Baden Pritchard

July 24

The Fish-sheltering Medusa

Probably the species of fish to which Mr. Lawless refers as seeking shelter under the swimming-disc of *Aurelia aurita* (Nature, vol. xvi. p. 227) is *Merlangus carbonarius* (Cuv.), popularly called boat-fish. At least I have seen the fry of this species behaving as Mr. Lawless describes.

The observation stated in the following words appears to me one of great interest:—"Occasionally the Medusa turned in its pulsations, so as to bring the umbrella undermost, when the fish would shoot hastily out, but the Medusa had no sooner righted itself, than the fish returned." Now, if this occasional turning on the part of the Medusa was not merely accidental, but, as Mr. Lawless implies, a reflex act performed with the view of escaping from the irritation occasioned by the fish, the fact would show that the marginal ganglia of *Aurelia aurita* are so far co-ordinated in their action as to enable the animal to steer itself in any required direction. For my own part, I have not as yet been able to satisfy myself that such ganglionic co-ordination occurs in any species of covered-eyed Medusa; so it would be well worth while if Mr. Lawless could repeat his observation a sufficient number of times to exclude the supposition of the somersaults being merely fortuitous.

I may take this opportunity of saying that the cut which illustrates the abstract of my lecture on p. 232 of the same issue of Nature as contains Mr. Lawless's letter, is intended to represent the species of Medusa to which he refers, viz., *Aurelia aurita*. The cut is about $\frac{1}{8}$ natural size.

George J. Romanes

Phyllotaxis

I have noticed in the laurel and the Spanish chestnut species, in which the leaves have normally a distichous arrangement, that when a vigorous shoot takes a vertical direction—for example, after the stock has been cut down near the ground—the leafage of such a shoot is often quincuncial. The phenomenon suggests three possible interpretations. Is this to be regarded as a fixed adaptive habit, the spiral phyllotaxis being the fittest for the upright, the two-ranked for the more numerous lateral twigs? Or are the exceptional instances endeavours after greater economy of space in the packing of the buds? Or, finally, ought we to discern in the peculiarities of the more vigorous shoots a reversion towards some ancestral condition? W. E. Hart

Drumaweir, Greencastle, July 20

Printing and Calico-Printing

As all that I am ever personally concerned to know is the truth of a matter, I am glad to stand corrected by the writer of the article on Calico-Printing in the "Encyclopædia Britannica." The claim I made, however, for the author of the "Natural History of Enthusiasm" was not my own invention; and it would be of interest, I think, to the many who must still, even in our day, revere his memory, to know more fully and accurately what it was that engrossed so many years of his valuable life, and what, if any, have been the practical results.

Bregner, Bournemouth, July 23 Henry Cecil

THE VISIT OF THE BRITISH ASSOCIATION TO PLYMOUTH

THERE are very good reasons for anticipating that the Plymouth meeting of the British Association will be at least up to the average in interest and success. Indeed, in some respects it is anticipated that it will be unusually attractive; and the attendance is likely to be much larger than from the distance west was at first thought probable. And as the details of the local arrangements approach completion so do the outside attractions, rendered available for the pleasure or information of the visitors, increase and multiply. The Government authorities have kindly consented to render every facility in their power for the inspection of the great establishments which constitute Plymouth and Devonport one of the chief arsenals in the world. The dockyard and Keyham yards will be freely open; those who desire to inspect the famous biscuit machinery at the Royal William Victualling Yard will be enabled to do so; there will be gunnery practice and probably torpedo practice also to be witnessed on board the *Cambridge*. The Breakwater of course can be seen at any time from Plymouth Hoe, and visited whenever weather will permit, which unless a gale is blowing it always does. The Breakwater Fort, reared on an artificial island of stone in the Sound immediately within the Breakwater, granite-capped and iron-plated, is by far the most interesting of the great chain of forts wherewith the Three Towns are girdled, and this too, it is hoped, will be open to inspection. The Eddystone will be visited on the Saturday, as already stated; but it must be understood that it is by no means certain that a landing can be effected. In fair weather, even, there is at times such a swell there as to render landing difficult, and even dangerous, while in rough weather it is impossible.

The excursion arrangements have been somewhat extended since our previous notice. On the Saturday, in addition to the excursions to the Eddystone, Lee Moor, and Iskeard for the Caradon Mines, it is now proposed by the citizens of Exeter to invite a large party of the members to this famous city, which abounds in objects of antiquarian interest, and which is noted for the hospitality with which it receives its guests. Iskeard too, is moving in the same direction; and the proprietors of Lee Moor Clay Works, Messrs. Martin, intend to make provision also for their visitors. After the clay works have been seen and justice done to the luncheon, there will be ample opportunity for a delightful ramble on Dartmoor. Shell Top and Pen Beacon, with their magnificent views and prehistoric remains, are within very easy distance, and good walkers will have the opportunity of enjoying some of the most romantic scenery in Devon, in the valleys of the Plym, and other moorland rivers.

The excursion arrangements for the Thursday remain unchanged; but there has been a considerable addition to the list of available attractions. The engineers of the party, through the kindness of Mr. Margary, engineer of the Great Western Railway for the district, will be enabled to inspect the Royal Albert Bridge to their heart's content—even to a scramble through the tubes. The great granite works and granite quarries of Messrs. Freeman at Penryn; the mines of Dolwath, Tincroft, and Carn Brea (by the kindness of Capt. Josiah Thomas and Capt. Teague); the pneumatic stamps of Mr. Husband, at the Hayle Foundry; the tin smelting works of Messrs. Bolitho, at Penzance, will all, by the courtesy of their proprietors, be available to be visited by members of the Association. And as the Earl of Mount-Edgcumbe has kindly opened his magnificent park and his romantic mansion of Cotehele, so Sir John St. Aubyn permits his famous and historic residence, St. Michael's Mount, to be visited by those members of the Association who may find their way so far west.

The public museums of the two counties will, we believe, be all open to the members. The chief are that at Exeter, that of the Royal Cornwall Institution at Truro, and that of the Royal Cornwall Geological Society at Penzance. Mr. C. C. Ross, of the latter town, has one of the best private collections of minerals in the West of England, and will gladly show it to all who feel interested in mineralogy. Then there are the museums of the Torquay Natural History Society, the specialty of which is its Kent's Cavern collection, which will form one of the attractions of the Torquay excursion, and the Museum of the Plymouth Institution, in which will be found a magnificent collection of flint implements and weapons lent and arranged for the occasion by Mr. Brent.

There will be several local papers contributed to the various sections, but the list is hardly likely to be so long as at the meeting of 1841, when the local contributions were unusually numerous.

The Pharmaceutical Society will, as usual, hold their meeting immediately prior to the meeting of the British Association at Plymouth, and the Mineralogical Society and the Society of Public Analysts, will also meet at Plymouth during the Association week.

THE GORILLA

SINCE Monday last the young gorilla from the Berlin Aquarium has been exhibited, during most hours of the day, at the Westminster Aquarium, in company with a chimpanzee. This is the first occasion on which a living gorilla has been publicly exhibited in this country as such, an earlier specimen some years ago, in a travelling menagerie, having passed for a chimpanzee during its life-time.

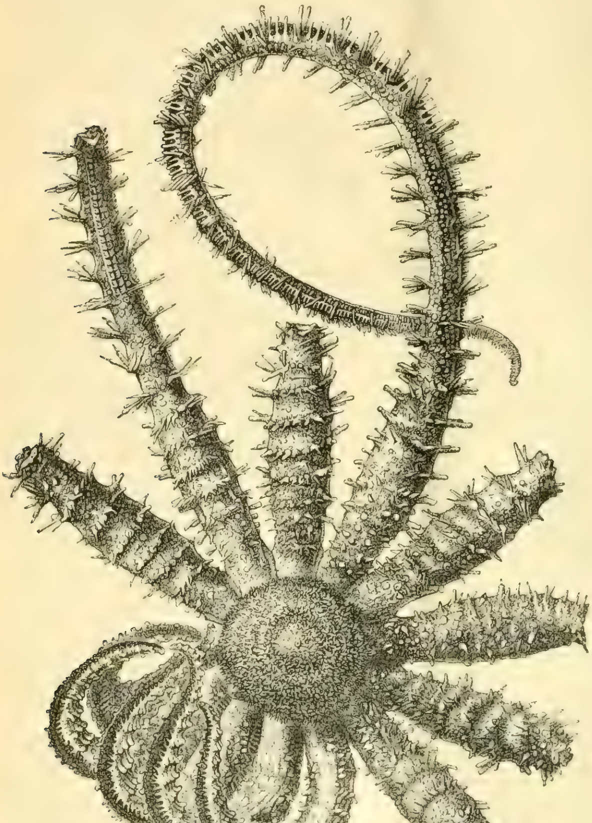
The gorilla, which is about three years old, appears in excellent health, and differs most strikingly from its companion in the blackness of its face and extremities, the smallness of its ears, the shortness of its muzzle, the great development and breadth of the alae of its nose, the shortness and softness of its thick-set body-hair, the presence of a frontal hair-tuft, the breadth and flatness of its back, which is also capable of greater backward bending, the smallness of the four outer toes, which are free for but a short distance, the breadth of its hands, and the massiveness of the nape of the neck. The conjunctiva is black, and the eye intelligent. We think that no one interested in natural history should lose the opportunity of seeing this particularly interesting Anthropoid ape.

BRISINGA

NEARLY a quarter of a century ago the celebrated Norwegian poet and naturalist, P. Chr. Asbjörnsen, was dredging in the interior of the picturesque Hardangerfjord, when, at a depth of about 200 fathoms, the dredge brought up a wonderful new star-like Echinoderm, quite unlike any form that had been up to that moment described. From a little circular disc of about an inch in diameter there issued eleven spreading arms or rays upwards of a foot each in length. These were armed along the edges with several rows of long spines; these arms, while standing near together at their base, generally taper away gradually to their tips. The colour, though variable, was, on the upper or dorsal surface, of a more or less red hue and paler, often to whiteness, on the under surface. On the lower surface of the disc, and occupying the central space, is seen the mouth-like aperture of the alimentary system, and spreading away from it along the centre of each ray-like arm, are the deep ambulacral furrows, so called because from these furrows issue the ambulacra or water-feet. These form two uninterrupted rows, and are flanked by several palisades of strongly-developed spines, the outer ones being the longest. All these spines are enveloped in an integument which is covered with strange-looking Pedicellariæ.

Only an instant's glance at this brilliant novelty was vouchsafed to the poet-naturalist; for beneath his glance the star-fish, thus brought up to quarters new to it, threw off all its arms, and what was once a thing of beauty became now a tangled mass of writhing arms moving away from the disc that had so long borne them company.

From living in great darkness and in the tranquil depths of the ocean's bosom, the being brought so suddenly up into the bright sunlight and to the agitated movements of that ocean's surface was too great a change and too severe a shock, and the catastrophe just mentioned was the consequence. To Asbjörnsen, however, this thing of beauty seemed like a link in the chain of the past. In its unlikeness to most recent forms of star-fishes he saw its connection with certain fossil forms, and in its brilliant sun-like form he was reminded of the "Brising" which, according to ancient Norwegian tradition, was concealed by Loke in the abyss of the primeval ocean, but which had so long served as the ornament to cover the breast of the god Freya, and he gave the name of Brisinga to the new genus.



Brisinga coronata, G. O. Sars. Natural size.

From the number of its arms Asbjörnsen called this new species *B. endecacnemos*, and until quite recently it was the only species known. In one locality of the fjord—Hesthammer—its occurrence cannot be considered as very rare, it has only been met with on rocky bottoms and at depths of from 200 to 400 fathoms. It has also been dredged by Prof. G. O. Sars some miles north of Bergen; by Sir Charles Wyville Thomson in the North Atlantic, and off the west coast of Portugal by Mr. Gwyn Jeffreys, an account of the finding of these latter specimens will be found in Prof. Sir C. W. Thomson's most interesting work on the "Depths of the Sea."

A second species of this genus was in 1869 and 1870 brought up by Prof. G. O. Sars at the fishing-place Skraaven, in Lofoten, from a depth of 300 fathoms, and this species has also since been found in the great depths of the Atlantic Ocean; for this species the name *B. coronata* has been selected by Sars, and this name has been adopted by Thomson, from whose work the accompanying beautiful woodcut of this species has been borrowed. It represents the animal as seen from above; five rudimentary arms in one series take the place of those that have been lost, for in this species the number of arms varies from nine to twelve.

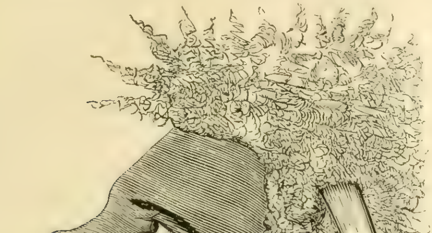
This new species has been made the subject of an elaborate memoir by Prof. G. O. Sars, in which memoir the structure and affinity of the genus is also fully discussed. It has been published as the University of Christiania's programme for the latter half of 1875. With the aid of a tolerably large number of fresh specimens and by repeated careful dissections, Sars has been enabled to ascertain most of the points in connection with its histology, and seven plates assist in illustrating the structures described. He considers the functions of the remarkable little organs called Pedicellariæ, which occur in most surprising number in both species of Brisinga, as that of seizing and holding fast the objects which come in contact with them, those that are found on the dorsal surface, thus acting as protectors to the thin skin; and those on the lower surface acting in the service of alimentation.

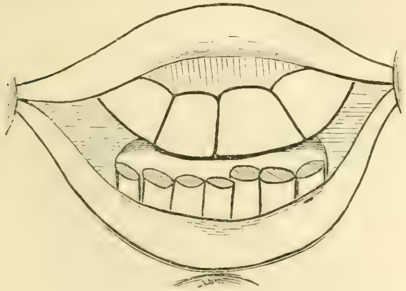
Among living star-fishes Brisinga seems to stand isolated, coming perhaps nearer to Pedicellaster; with the oldest known fossil star-fish Protaster, it shows close affinities, but would seem to be older and less specialised, and if so it would then be the most primitive as well as oldest form known of Star-fishes. It must therefore be kept in a family of Echinoderms by itself which may be called *Brisingidæ*.

E. P. W.

A REMARKABLE DEFORMITY OF THE TEETH AMONG THE INHABITANTS OF THE ADMIRALTY ISLES[1]

THE Russian traveller, M. Miklucho-Maclay, in the course of recent travel in Melanesia, has noticed among the natives of the Admiralty and Hermit Isles a remarkable peculiarity in the teeth, the upper incisors projecting "shovel like," almost horizontally, and to such a degree as to extend even beyond the lips when the mouth is closed. The breadth, moreover, of one of these teeth is at times so great as to equal its visible length; being in the specimen figured as much as 19 millimetres to 16 mm. of length, measured, of course, from the edge of the jaws, not from the extremity of its fang. As all the teeth have a blackish polish, due to the prevailing habit of betel-chewing, the mouth presents a somewhat ghastly appearance. M. Miklucho-Maclay has nowhere else met with a similar deformity of the teeth, but heard of such, when on the peninsula of Malacca, the race in which it occurs being called "orang-gargassi."[1]





J. C. G.

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[1] See a note contributed to the *Illustrirte Zeitung* of Leipzig by M. Miklucho-Maclay.

[1] *Man-devil. Orang* is the usual Malay word for man, while *gargassi* is equivalent to the German *Quälgeist*, tormenting spirit.—J. C. G.

RAINFALL AND SUN-SPOTS

WE have received the following communications having reference to Gen. Strachey's paper on the above subject, which we printed some little time back.

The conclusions of Mr. Meldrum as to a relation between the amount of rainfall and the frequency of sun-spots have become a subject of much interest with reference to the possibility of being prepared for such a deficiency of rain in India as may result in a failure of crops and consequent famine. That the varying yearly rainfall at Madras showed on the whole a rather marked agreement with the sun-spot period, has been known to me for some time, and Dr. Hunter has lately made an examination of the amounts relatively to an eleven-yearly cycle which has excited marked attention on account of its possible practical application. Gen. Strachey has made a discussion of the Madras observations in a paper read before the Royal Society, a full abstract of which has appeared in NATURE (vol. xvi., p. 171). He has sought to show that there is no evidence in the Madras observations of periodicity at all; and that if the rainfalls for each of the sixty-four years were written on slips of paper and drawn from a bag, so that the first amount drawn should be placed to the first year (1813), the second to the next year (1814), and so on, as well-marked a result would be obtained as is shown by the quantities observed in their respective years. This conclusion he founds on the following method, which he terms a "true criterion of periodicity."

If the differences of the rainfall for each year from the mean of the whole sixty-four years be taken, and the mean of all these differences (without respect to sign) be called the *general* mean difference; if we arrange the yearly rainfalls in horizontal series of eleven successive years (there will be six such series nearly), and the means for the first, second . . . . years of the series be taken, these quantities (periodic means) will show the mean variation in the period of eleven years, if any such exist. If now the differences of the yearly rainfalls from the periodic means for the corresponding years be obtained, the means of these, irrespective of sign, may be called the *periodic* mean differences. In the case of the Madras rainfalls Gen. Strachey finds—

The *general* mean difference = 12·4 inches,
The *periodic* ,, ,, = 11·2 ,,

and his true criterion of periodicity, though not so definitely stated as might be wished when so important a rule is proposed, appears to be that if there be no periodicity the variation in the periodic means will tend to disappear in a sufficiently long series of observations and the *general* and *periodic* mean differences be identical. It seems to me that the disappearance of a variation in the periodic means is here the true criterion of no-periodicity; but though a very large variation exists in the case of the Madras observations, yet Gen. Strachey concludes that since the *periodic* and *general* mean differences agree so nearly, there is no tendency to periodicity shown in the Madras observations.

As an illustration of the true criterion, Gen. Strachey takes the two hourly observations of the barometer for five successive days at Madras, and shows that—

The *general* mean difference = 0·030 inches,
The *periodic* ,, ,, = 0·014 ,,

Here, he says, a true period existing, the *periodic* mean difference becomes much less than the *general* mean difference.

I shall now venture to show that this is no criterion of periodicity. If we represent variations of any quantity for a given time by a curved line, and if we have several such lines of exactly the same form placed one over the other, a straight line passing through the curves, with as much space between the straight and curved lines above as below, will represent the *general* mean. In a simple curve of two branches the *general* mean difference will be nearly one-fourth of the amplitude of the oscillation; while, as all the oscillations agree with each other, and therefore with the mean oscillation, the periodic mean difference will be zero. If, however, we displace the individual curves so that as many shall be above as below their mean, both the *general* and the *periodic* mean differences will increase, and the difference between these quantities will diminish, till the individual curves are so separated from the mean that none of them is cut by it, when the two mean differences will be *equal:* between this case and that of general coincidence the two mean differences will have values which will differ more or less from each other, according as the individual curves are nearer to, or more remote from, the mean; and the ratio of the one mean difference to the other will tend to a constant value as the number of cycles increases, a ratio which will depend for its value on the mode of distribution of the individual curves and of the irregular deviations from the mean.

Gen. Strachey's illustration is from a case approaching coincidence; hundreds of cases, however, may be found of the other class, especially when, as in this instance, only a few periods are in question. Thus, taking two hourly observations of the barometer at Simla during six days in the beginning of January, 1845, I find—

The *general* mean difference = 0·0634 inches,
The *periodic* ,, ,, = 0·0615 ,,

and if the last day of the six be omitted so as to have an *odd* number of days, I find—

The *general* mean difference = 0·0656 inches.
The *periodic* ,, ,, = 0·0634 ,,

Gen. Strachey's conclusion from the Madras rainfall observations is in fact that because the *periodic* mean difference was only one-tenth less than the *general* mean difference, there was no evidence of periodicity whatever; here we have a large and regular semi-diurnal period (the whole mean range being 0·070 inch) where the *periodic* is not one-thirtieth less than the *general* mean difference.

I may add that when the true sun-spot period of ten and a half years is employed for the Madras rainfall observations, I find—

The *general* mean difference = 12·4 inches,
The *periodic* ,, ,, = 10·2 ,,

quantities which differ by five times as much as those found for the true periodic variation of the barometer at Simla.

I have taken the variation chosen by Gen. Strachey to illustrate this question, but the fact that the difference of the *general* and *periodic* mean difference is no criterion of periodicity might have been shown equally well with cases more resembling that of the rainfall, where the irregular variations are large compared with those following a known period; I cannot here, however, enter into details and notice only the objections offered by me to Gen. Strachey's paper when it was read before the Royal Society.

It would be easy to show that the Madras rainfall observations, *taken alone*, give results which are remarkable in several respects. Thus, 1st, They show a *mean* oscillation larger for about ten (nine to eleven) years than for any other duration. 2nd. When the mean variations for a period of ten and a-half years are represented by a function of sines they give the yearly *mean* rainfall (y) in the period, $y = 6{\cdot}2 \sin(\theta + 310^\circ)$, showing the large range of 12·4 inches. 3rd. This representative equation gives the epochs of maximum rainfall in the years of maximum sun-spots, or as nearly so as would be given by the mean sun-spot areas represented by a similar expression.[1]

On the other hand, the irregularity in the amount of rainfall from year to year is so great that the probable error of the periodic means is too considerable to give any great weight to this result alone.[2] When observations during a sufficiently large number of cycles have been obtained, so as to make the probable error of the means small compared with the range of the periodic variation, then there will be a general acceptation of Gen. Strachey's remark: "It is hardly conceivable that there should be a coincidence with the sun-spot period, such as is supposed to be found at Madras, based on any physical cause which should not in some way be discernible in the rainfall at Bombay and Calcutta" (NATURE, vol. xvi. p. 172). He has then taken *five* cycles of eleven years' rainfall at Bombay, and *four* cycles at Calcutta, and testing them by his criterion he obtains results quite similar to that for Madras.

I have only the periodic *means* for the five eleven-yearly cycles at Bombay now before me, but seeking from these the representative equation of sines as for Madras, and repeating the latter for comparison, I find—

Bombay ... ... $y = 6{\cdot}1 \sin(\theta + 316^\circ)$.
Madras .. ... $y = 6{\cdot}2 \sin(\theta + 310^\circ)$.

Both equations give almost exactly the same range of the oscillation and nearly the same epochs of maximum and minimum as the sun-spots.[3] This result, which was wholly unexpected by me, is all the more remarkable that the two places are on the opposite coasts of India, and have their rains from different quarters. Calcutta, with a sufficiently large number of cycles, might also have agreed with Bombay and Madras, which is not the case, however, with four cycles only. In each case the criterion would show that no periodicity exists.

I cannot, then, agree with Gen. Strachey as to his test of periodicity nor to the conclusions he has deduced from it. I will not enter here into the consideration of the weights which may be given to results founded on the known principles of the calculus of probabilities, nor into the question whether the rainfall, not at one or two stations only, but over a country or the whole globe, may not show some relation to the sun-spot period as Mr. Meldrum believes, and as I think quite possible, judging from other results of solar actions. This relation, however, it appears to me has still to be proved, though the observations considered by Gen. Strachey are, on the whole, so much in its favour as to encourage further investigation.

JOHN ALLAN BROUN

Lyndhurst, New Forest, July 18

In the paper read by Gen. Strachey before the Royal Society, May 24 (see NATURE, vol. xvi. p. 171), "On the alleged Correspondence of the Rainfall at Madras with the Sun-spot Period, and on the True Criterion of Periodicity in a Series of Variable Quantities," certain conclusions are arrived at which render it desirable to test the value of the criterion of periodicity employed. This is the more necessary when it is considered not merely that the principle, if a sound one, must be of extensive application in physical research, but also, and more immediately that the conclusion arrived at seriously affects the great modern problem of Indian administration, viz., the food-supply of the people. The conclusion is that in the case of the rainfall observations of Madras, which have been discussed by Dr. Hunter, the Director-General of Statistics to the Government of India, the evidence is not sufficient to establish either any periodicity or a correspondence such as Dr. Hunter points out. The correctness or incorrectness of Gen. Strachey's views will, it is evident, materially influence the line of action taken by the Government in dealing with the disastrous famines consequent on the recurring droughts of Southern India.

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[1] The years given by the equation, the series commencing with 1813·5, are 1817·8, 1828·8, 1838·8, 1849·8, 1859·8, 1870·8. The condition that an oscillation should agree in its epochs of maximum and minimum with those of a known phenomenon (a very weighty one when the chances are to be considered) has been neglected by Gen. Strachey altogether.

[2] This refers to the periodic means deduced from the observed quantities; the above equation for Madras gives the observed means with a probable error of less than 3 inches.

[3] The first cycle at Bombay begins with the same year, (1824·5) as the second cycle for Madras.

The criterion of periodicity brought forward by Gen. Strachey will appear from what follows. The mean rainfall at Madras for the sixty-four years is 48·51 inches; if we take the difference between this mean and the rainfall of each individual year, and average the results, we obtain 12·40 inches as the arithmetical mean of the sixty-four differences. If we now partition the sixty-four years' rainfall into six sun-spot cycles of eleven years each, and take the difference between each individual year's rainfall and the mean of that term of the sun-spot cycle in which that year's rainfall stands, and then average these sixty-four new differences, we obtain 11·20 inches as the arithmetical mean. Since 12·40 inches, the mean difference of the individual observations from the mean of the whole series, is, by the latter process, only reduced to 11·20 inches, or about 10 per cent., Gen. Strachey concludes that the supposed law of variation obtained from the means of the six eleven-year cycles hardly gives a closer approximation to the actual observations than is got by taking the simple arithmetical mean as the most probable value for any year; and that the evidence, therefore, is not sufficient to establish any periodicity in the rainfall of Madras, or any correspondence between it and sun-spots.

In illustration of his meaning Gen. Strachey applies his criterion of periodicity to the diurnal barometric oscillations at Madras, in which a well-ascertained periodicity exists. The result of the calculation is that the mean difference of all the individual observations from the mean of the whole series is 30, whereas the mean of the differences between the two-hourly individual observations and the averages of the same hours is reduced to 7, thus indicating, it is added, the distinct presence of a periodicity.

In concluding against the presence of any periodicity in the rainfall of Madras, Gen. Strachey makes these two assumptions:—(1) If there be a periodicity in the rainfall of this part of India, it would be made apparent by applying his criterion of periodicity to the observations; and (2) the difference between the two arithmetical means calculated as above must exceed at least 10 per cent.

The diurnal barometric oscillation at Madras is so regular a phenomenon that it was scarcely worth while to make the calculations, since one could have made a close approximation to the averages 30 and 7 by a simple inspection of the figures. Further, this periodicity which has been selected is altogether inappropriate to the subject in hand, as no one could possibly imagine for a moment that any periodicity which might characterise the rainfall of Madras would exhibit an approach to such regularity of occurrence as characterises the curve of the diurnal oscillation of the barometer at that place. The periodicities which suit the subject before us are such as are presented by the curves of the diurnal barometric oscillation in the British Islands.

Let us then apply Gen. Strachey's true criterion of periodicity in the examination of such a curve, taking for our example the barometric observations at Valencia for the month of December, 1876, as published in the *Hourly Readings* at their seven observatories, by the Meteorological Committee for that month. The examination will at the same time test the value of this new criterion as an implement of scientific research.

The mean of the whole 744 observations is 29·256 inches. The differences of the individual 744 observations from 29·256 inches give an arithmetical mean difference of 0·3372 inch; and the differences between each of the individual 744 observations and the means of their respective hours give an arithmetical mean difference of 0·3369 inch. Looking at the individual hours the largest differences are 0·3434 inch and 0·3413 inch at 10 P.M., and 0·3444 inch and 0·3459 inch at 6 A.M. Thus the difference of these two arithmetic means is only 0·0003 inch, and the largest difference for any hour amounts only to 0·0021 inch.

Now Gen. Strachey concluded against the presence of a periodicity in the rainfall of Madras on the ground that the two arithmetical means differed only 10 per cent. from each other. It follows *à fortiori*, if this new criterion is of any value, that the presence of a periodicity in the diurnal barometric oscillation at Valencia during December last be concluded against, inasmuch as the difference between the two arithmetical means is only $\frac{1}{10}$ per cent., and not rising quite to 1 per cent. for any of the twenty-four hours.

The averages for the month, however, show in an unmistakable manner the presence of such a periodicity. The mean pressure there fell to 29·245 inches the morning minimum at 7 A.M., rose to 29·263 inches the morning maximum at 11 A.M., fell to 29·238 inches the afternoon minimum at 3 P.M., and rose to 29·273 inches the afternoon maximum at 10 P.M., the time of occurrence of these phases of the curve differing no more than an hour from the mean periods for Valencia at this season of the year. The periodicity is, as stated, a well-marked one, the sum of the diurnal oscillations amounting to 0·106 inch, being three-fifths of the sum of these oscillations for Madras which average for December 0·174 inch. We do not require to remind our readers that the phenomena of the diurnal oscillations of the barometer take their place among the most universally accepted and best established periodicities of science.

In the concluding paragraph of his paper Gen. Strachey apparently applies another criterion to the Madras rainfall, which consists in the comparison of the successive combination of the observations, beginning with one eleven-year cycle, and then combining two cycles, and so on, till the whole six cycles were united. The result arrived at by this treatment of the figures, is that the successive means of the differences between the mean rainfall for the combined cycles and the mean for the several years of the cycle when combined, show no appreciable periodicity.

With reference to this mode of testing the question, it may be enough to say that even were the result of such an examination as adverse as possible, it could not be used as a conclusive argument against the existence of periodicity, for the very plain reason that we are investigating a periodicity for which only six terms or separate cycles are available, and in these cycles the non-periodical elements bulk largely. But the following table, referring to the portion of the diurnal barometric curve between the morning and evening minima at Valencia for December last will illustrate the point:—

		A.M.						P.M.	
	7	8	9	10	11	noon	1	2	3
Means for month ...	inches. 29·245	in. ·247	in. ·252	in. ·259	in. ·263	in. ·253	in. ·246	in. ·241	in. ·238
Days of month 3 ...	28·511	·533	·550	·551	·562	·555	·546	·535	·516
3–4 ...	28·490	·507	·527	·541	·551	·552	·554	·556	·550
3–5 ...	28·491	·502	·514	·526	·534	·535	·537	·541	·544
3–6 ...	28·589	·600	·613	·627	·637	·640	·644	·647	·652
3–7 ...	28·694	·707	·723	·738	·748	·751	·756	·760	·767
3–8 ...	28·863	·880	·899	·918	·931	·935	·942	·947	·957

The observations of the third will be seen to follow, though somewhat roughly, this portion of the curve for the month. The means of the third and fourth show a greater divergence from the monthly curve, and so on through the combinations, each successive combination showing instead of a continued approximation to, a continually increasing divergence from, the mean hourly variation of the month. It is this consideration, to which Gen. Strachey does not appear to have given due weight in his paper, which has led Meldrum and others in their investigation of periodicities of the rainfall and temperature, to extend their inquiries not only over lengthened intervals of time, but also over as wide areas as possible.

It may be added, that this new criterion of a periodicity enunciated and applied by Gen. Strachey at a meeting of the Royal Society in May last would, were it accepted, equally sweep from our view scores of periodicities now everywhere accepted, and effectually foreclose inquiry in many fields of research in which science is certain to reap brilliant results, namely, in those departments of research in which the non-periodical are very largely in excess of the periodical variations, of which meteorology may be regarded as presenting the most numerous and best illustrations.

GEOLOGICAL NOTES

GERMAN GEOLOGICAL SURVEYS—(1) AUSTRIA.—The programme of the Austrian Geological Survey for this year shows that the work is advancing, as it has been doing for some time past, mainly in two directions, one lying on the extreme east, the other on the far west of the empire. In the Tyrol two sections or parties are in the field; one of these, under Dr. Stache, and Mr. F. Teller, is investigating the crystalline masses of the Central Alps along both sides of the Vintschgau; the other, under Dr. E. v. Mojsisovics and Messrs. M. Vacek and A. Bittner, is engaged among the sedimentary formations between Botsen and the Venetian frontier. On the other side of the empire, in Eastern Galicia, Bergrath C. M. Paul and Messrs. Tietze and Lenz are busy among the Carpathians and their spurs to the south of Stanislawow. The vice-director of the Survey, Bergrath D. Stur, will also this year publish his researches on the flora of the Carboniferous period. The whole of the operations of the Survey are controlled and directed by the able hands of Ritter von Hauer.

(2) PROVINCE OF PRUSSIA.—Besides the national survey organised and paid by the government for the investigation of the geological structure of the kingdom of Prussia, there is in progress under the auspices of the Physical-Economical Society of Königsberg a geological investigation of the Province of Prussia with the publication of a map on the scale of $\frac{1}{100000}$. It might seem at a first glance that this wide alluvial plain could hardly offer much opportunity for geological observations or for much variety of colouring on the map. But by means of careful examination of the surface and well-arranged borings below it, much valuable information is being obtained regarding the structure and history of the alluvial, peaty, and drift deposits of the Baltic plain of Prussia. The lively interest which has been raised on all sides by the undertaking has suggested the idea to publish yearly an account of the progress of the work with notices of the more interesting observations and discoveries, and such additional information from other investigators or from other countries as may throw light upon the geological history of the province. Dr. Alfred Jentsch has prepared the first *Jahresbericht*, which appears in the *Transactions* of the Physical-Economical Society. After a brief account of the preparation of the map, and of the various boring operations he gives an interesting *résumé* of the geology and physical geography of East Prussia, including the variations of water-level in the Vistula and Pregel, the peat-mosses, marls, alluvial clays, drifts, brown-coal, and amber-deposits, with the cretaceous, Jurassic, and palæozoic rocks made known by boring explorations.

GEOLOGICAL SURVEY OF NEWFOUNDLAND.—Mr. Murray has published a second edition of the Index Geological Map of Newfoundland on the scale of twenty-five miles to an inch. It is of course brought up to date, and exhibits with great clearness the distribution of the various rocks of the colony. The remarkable serpentines, slates, and metamorphic rocks overlying the sandstones of the Quebec group on the west side of the island, are so inserted as to show distinctly their unconformable relations to the rocks below them. Four sections are likewise placed upon the map for the explanation of the geological structure of different regions. The map, in regard to execution, is all that could be desired, considering its small scale and provisional character. Mr. Murray's Report for 1876 has just been issued. The ice which hugged the coasts so late last year prevented a start being made until the end of June. During the few months available for exploration, Mr. Murray and Mr. Howley succeeded in mapping some portions of the interior about the Gander and Gambo rivers. As usual the routes lay along the river-courses where almost the only geological observations can be made, the intervening country being covered with swamps or forests. The Report shows that considerable areas of good agricultural land lie in the interior, and that while large masses of valuable timber exist they need to be guarded against the ignorant and wanton operations of lumber-men.

CHEMICAL NOTES

NEW CHROMIUM AND MANGANESE COMPOUNDS.—Some new compounds of chromium and manganese have lately been prepared and examined by Mr. J. B. Hannay, who has communicated a paper on the subject to the Glasgow Philosophical Society. On examining any general list of carbon compounds it is to be remarked that, however complicated their structure, they are not as a rule decomposed by water; on the other hand complex compounds of other elements are as a rule decomposed by this substance into two or more simpler compounds. Mr. Hannay was therefore induced to examine whether carbon is or is not the only element capable of forming series of bodies of complicated structure; and whether the existence of water on this earth is the reason of our not having complex bodies with other elements than carbon for their basis. The plan adopted was to take some complicated substance (containing no carbon) which is decomposed by water, find a solvent for it, and act on it with other reagents out of contact of air and moisture. The substance used was oxychlorid of chromium, (CrO_2Cl_2), and the solvents employed, carbon disulphide and carbon tetrachloride. Mr. Hannay has devised an apparatus which allows of the substance being precipitated, filtered, washed, dried, and weighed off for analysis without coming in contact with air or moisture. The following is a list of the chromium compounds prepared by him :—

$Cr_2Cl_4SO_3$.
$Cr_2Cl_4SO_2$.
$Cr_2Cl_4Br_2$.
$Cr_2Cl_4S_2O_2$.
Cr_2Cl_6S.
$Cr_2Cl_4Br_2S$.
$Cr_2Cl_4S_2$.
$Cr_4Cl_6S_3$.
$Cr_4Cl_6S_3O_4$.
$Cr_4Cl_6S_3O_{12}$.
$CrCl_2S_2.H_2O$.
$CrCl_2S_2$.
$CrCl_3PSO$.
$Cr_3Cl_6P_4O_6$.
$Cr_3Cl_6P_4O_2$.
$Cr_3Cl_6P_2Br_6O_2$.
$Cr_3Cl_6P_46(H_2O)$.
$Cr_3Cl_6P_4$.
Cr_3Cl_6PCl.
Cr_3Cl_6PBr.
$(Cr_3Cl_6P)_2O$.
Cr_3Cl_6P.

Mr. Hannay has prepared some analogous manganese compounds, but the analyses of these have not as yet been finished.

COMPLEX INORGANIC ACIDS.—Dr. W. Gibbs has lately obtained a series of new inorganic acids formed on the type of the silico-tungstic acids obtained by Marignac. The new series of salts contain platinum instead of silicon, and the salt $10WO_3PtO_24Na_2O + 25\ H_2O$ has been obtained by boiling platinic hydrate $Pt(OH)_4$ with acid sodic tungstate. Two metameric sodium salts have been obtained, one of an olive-green colour, the other honey yellow with an adamantine lustre. The corresponding potassium and ammonium salts of this platino-tungstic acid have also been obtained, but they belong to the yellow series. Mr. Gibbs has not as yet obtained salts corresponding to Marignac's twelve atom silico-tungstates. Acid molybdate of sodium also dissolves $Pt(OH)_4$, giving a green solution, which appears red when viewed in thick layers; the only salt of this series studied, crystallises in amber tabular plates having the composition $10MO_3PtO_24Na_2O + 25H_2O$. He is endeavouring to generalise the results by substituting other hydrates, such as $Zn(OH)_4$, $Ti(OH)_4$, $Sn(OH)_4$, but has, as yet, in these cases not obtained very definite results. He is also engaged in examining the phospho-tungstic acids containing $20WO_3$ obtained some time ago by Scheibler.

A SUPPOSED NEW METAL "DAVIUM."—The discovery of this new element is reported from St. Petersburg by Serjius Kern. It was found by him in the residues of platinum ores after treatment to separate out the metals of the platinum group. The specific gravity of the metal is given as 9·385 at 25°. The author supposes this new metal to occupy an intermediate position between molybdenum and ruthenium, but very strong evidence will be necessary to confirm the existence of a new metal belonging to the platinum group.

EFFECT OF PRESSURE ON CHEMICAL ACTION.—M. Berthelot, in a recent number of the *Bull. Soc. Chem.*, calls attention to the fact that some experiments lately made by Quincke have confirmed a statement made by the former chemist some time ago, that the evolution of hydrogen from zinc and sulphuric acid is not arrested by pressure. The experiments of Quincke show that when these bodies are brought in contact, the pressure of the hydrogen evolved rose in a few days from 1·5 to 10 atmospheres, and in a very much longer time from 25 to 126 atmospheres. Berthelot thinks that these experiments, although not performed for this purpose, prove that chemism is not modified, but only the nature and extent of the surfaces attacked. The evolution of gas would thus go on indefinitely, not arrested, but only modified in rapidity.

AMOUNT OF OXYGEN CONTAINED IN SEA-WATER AT DIFFERENT DEPTHS.—At a recent meeting of the Royal Society of Edinburgh Mr. J. Y. Buchanan communicated some results obtained from his experiments on the above subject during the cruise of the *Challenger*. Mr. Buchanan finds that at the surface the amount of oxygen varies between 33 and 35 per cent., the higher numbers having been observed in a water collected almost on the Antarctic circle; the smallest percentages have been observed in the trade-wind districts. In bottom waters the absolute amount is greatest in Antarctic regions, diminishing generally towards the north. The oxygen percentage is greatest over "diatomaceous oozes," and least over red clays containing peroxide of manganese; over "blue muds" it is greater than over "globigerina oozes." In intermediate waters the remarkable fact was observed that the oxygen diminishes down to a depth of 300 fathoms, at which point it attains a minimum, after which the amount increases. The following figures show the nature of this phenomenon:—

Depth (fathoms)	0	25	50	100	200	300	400	800	Between 800 and the bottom.
Oxygen ($O + N = 100$)	33·7	33·4	32·3	30·2	33·4	11·4	15·5	22·6	23·5

It is evident from these figures that between 200 and 400 fathoms there is a great consumption of oxygen going on, and, as it is difficult to conceive its being consumed otherwise than by living creatures, the conclusion may be drawn that animal life must be particularly abundant and active at this depth, or at least more abundant than at greater depths; for, at less depths, there is more opportunity of renewal of the oxygen by reason both of the greater proximity to the surface and of the existence of vegetable life. This conclusion is borne out by the experiments of Mr. Murray with the tow-net at intermediate depths, which go to prove the existence of abundance of animal life down to 400 fathoms, vegetable life never extending much below 100 fathoms. Below 400 fathoms life is sparingly met with.

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF 1605, OCTOBER 12.—It is known that Clavius attributed the ring of light which he observed round the moon during the eclipse of April 9, 1567, about the time of greatest obscuration at Rome, to the circumstance of the sun's disc not being entirely covered by our satellite, a narrow rim of light thereby remaining visible. As Prof. Grant relates, in his "History of Physical Astronomy," Kepler maintained that the luminous ring seen by Clavius could not have been the margin of the solar disc, because he found by calculation that the moon was at her mean distance from the earth, when her apparent diameter exceeds that of the sun, even in perigee; and when a similar ring of light was remarked round the moon during the eclipse of February 25, 1598, and attributed to the same circumstance, Kepler again pointed out that such an explanation was inadmissible, the moon's apparent diameter, on this occasion also, exceeding that of the sun. These opinions were expressed by Kepler in his work "Ad Vitellionem Paralipomena," published in 1604, and Prof. Grant remarks that an eclipse in the following year strikingly confirmed them. This refers to the eclipse of October 12, 1605, observed at Naples, of which Kepler writes thus: (*De Stella Nova in pede Serpentarii*, p. 116) "the whole body of the sun was effectually covered for a short time. The surface of the moon appeared quite black, but around it there shone a brilliant light of a reddish hue, and uniform breadth, which occupied a considerable part of the heavens." We follow Prof. Grant's translation of this passage, which clearly proves that the eclipse was total for a brief interval at Naples.

As the eclipse of 1605 first confirmed the accuracy of Kepler's views, in opposition to those of Tycho Brahé, who disputed the possibility of a total eclipse of the sun, it may not be without interest to examine the circumstances of the phenomenon as it would be observed at Naples. For this purpose the same system of calculation adopted for other eclipses mentioned in this column, is followed. The elements are:—

G.M.T. of Conjunction in R.A. 1605, Oct. 12, at 0h. 31m. 44s.

	°	′	″
R.A.	197	14	51·0
Moon's hourly motion in R.A. ...		35	37·1
Sun's " " " ...		2	19·1
Moon's declination	6	40	27·9 S.
Sun's "	7	31	32·5 S.
Moon's hourly motion in decl. ...		10	50·2 S.
Sun's " " " ...		0	56·4 S.
Moon's horizontal parallax		59	21·2
Sun's " "			8·9
Moon's true semi-diameter		16	10·4
Sun's " "		16	3·9

The eclipse would therefore be central with the sun on the meridian in long. 11° 18′ W. and lat. 52° 26′ N., and the following would also be points upon the central line:—

Long. 19° 9′ E., lat. 39° 32′; and long. 14° 23′ E., lat. 40° 48′.

Calculating directly for Naples we find :—

		h.	m.	s.	
Totality began	October 12 at	2	18	18	Mean time at Naples.
,, ended	,, ,,	2	19	28	

The duration of the total eclipse was 1m. 10s., which is in satisfactory agreement with the words of Kepler. The sun was at an altitude of 31°.

THE BINARY STAR *α* CENTAURI.—As far as can be judged from a projection of the measures published to the present time, it appears probable that the nearest real approach of the components in this binary is already passed, but that they will continue to apparently close-in until the angle is somewhere about 110°, when their distance may have diminished to 1¾″. We can only continue to urge upon southern observers the great importance of frequent measures of this object for some years to come, with all the precision that the case will admit of, that a problem of the highest interest in celestial mechanics may be fully investigated.

MIRA CETI.—This variable star is now close upon the epoch of minimum, as calculated from Argelander's formula of sines, and observations so far are much fewer in number near this part of the light-curve than about the maximum. The gradual ascent to the next maximum may be favourably watched in the present year; the date by the formula is November 10, 1877.

D'ARREST'S COMET.—By M. Coggia's observation at Marseilles on the morning of the 10th inst., it appears that M. Leveau's ephemeris gives the position of the comet within about 3′. Subjoined are the calculated places for Paris noon, during the next period of absence of moonlight :—

	Right Ascension. h. m. s.	North Polar Distance. ° ′	Distance from the Earth.
August 8	3 57 35	83 45·0	1·559
,, 10	4 1 12	83 53·2	1·555
,, 12	4 4 43	84 2·0	1·551
,, 14	4 8 7	84 11·4	1·547
,, 16	4 11 24	84 21·4	1·543
,, 18	4 14 35	84 32·0	1·538
,, 20	4 17 38	84 43·2	1·533
,, 22	4 20 34	84 54·9	1·529

This comet has not yet been observed under its most favourable situation with respect to the earth. When the perihelion passage occurs early in August, it may approach our globe within 0·3 of the earth's mean distance from the sun, but, so far, has not been seen within a distance of about 0·8. At the next return at the beginning of 1884, observations will probably be difficult, but in 1890, when the perihelion passage (as well as can be foreseen without the calculation of planetary perturbations) is likely to fall in the latter part of August or in September, the comet's track in the heavens will be a favourable one.

NOTES

THE annual meeting of the Institution of Mechanical Engineers opened on Tuesday at Bristol. Mr. T. Hawksley, C.E., in his opening address, said it was the duty of the government to adopt such timely measures as would secure to us the paths of the ocean for our food inwards and our manufactures outwards. He deprecated the building of enormous and unwieldy floating castles, and advocated the construction of a fleet of swift, light, well-engined ships, equally capable of sailing or steaming. He thought the extreme action of some of the working classes the cause of England's trades going abroad. There was a conversazione in the evening.

A REMARKABLE case relating to manufacture and transport of explosives has just been the subject of an inquiry before the Wreck Commissioner. The facts are briefly these :—The passenger sailing ship *Great Queensland* left London for Melbourne on the 5th of August last. After the 12th, when she was spoken at sea, she was never seen; but some wreckage from her was washed ashore the same month on the south coast of England. She had taken on board some thirty-four tons of gunpowder, including two tons of the "Patent Safety Blasting Powder" (a compound made in North Wales by treating wood pulp with acid, and stated to have five times the strength of ordinary gunpowder !). There was also a large quantity of detonators and percussion caps. The stowage seems not to have been up to the mark; still the Commissioner regards it as having been fairly safe, but for the danger of spontaneous ignition of the patent powder, to which the facts apparently point as the probable cause of the disaster. The evidence bearing on the manufacture of the compound is not a little surprising. In 1875, the manager in charge of the process was a Mr. Hunt, describing himself as "an engineer, but no chemist." The powder he turned out seems to have been dangerously impure, and some of it having come into the hands of a Government Inspector was found so bad that a regular visit was made to the Company's works. Eight samples were analysed and pronounced impure and dangerous. Mr. Hunt was displaced. His successor, a Mr. Thistleton, made an attempt, at the directors' request, to re-dip the powder left by Mr. Hunt; but the smoke became intolerable, and at 110 deg. the sides smouldered into fire and dirtied everything about, while the heat broke the windows and charred the woodwork. He accordingly suggested that the only way was to dip it in potash solution. The process of remaking was going on in the early months of last year, and it was a portion of this remade impure powder of Mr. Hunt which was shipped on the *Great Queensland*. A few days after she sailed news came of an explosion at the Patent Gunpowder Works, and Major Majendie, having examined a cartridge found on the works after this, wrote that "accident is hardly the term to apply" to what happened. The conclusion of the Wreck Commissioner, then, is that the same event happened at sea and caused the disappearance of the ship. The facts speak for themselves. The case is evidently one of gross mismanagement based on an ignorance which might be laughable, though not excusable, in people employed in mixing tea and coffee, but shameful in the direction of a company for making an explosive. Considering the scientific knowledge imperative in making and handling our modern explosives, the appointment of the one manager who was "no chemist," and of the other who was so good a chemist (from the Royal Polytechnic) as to proceed to *re-dip* Hunt's material *in order to make it stronger*, at the request of his directors, and was only warned off when this compound nearly blew him into the air, calls loudly for explanation. It is important that the whole responsibility involved in this disgraceful case be fully elucidated by further inquiry.

WE regret to announce the death of Prof. Adolph Erman, the well-known physicist, which occurred in Berlin, July 13th. He was born in Berlin, 1806, and after completing a broad range of scientific study, devoted himself to physics, following in the path of his father, who was then professor of that branch in the Berlin University. In 1828 he joined the Norwegian expedition sent out to Siberia to investigate the phenomena of terrestrial magnetism. His own researches were carried out far beyond the confined limits of the expedition, and after thoroughly examining the hitherto almost unknown volcanoes of Kamtschatka, he terminated his journey by completing the circuit of the world in a Russian frigate. The rich store of magnetic observations made during the entire tour were gathered together into a work of two volumes. In 1834 Erman was appointed Professor of Physics at Berlin, a post which he continued to occupy up to the time of his death. From 1841–1866, he edited the *Archiv für wissen-*

schaftliche Kunde von Russland, a periodical issued at the expense of the Russian Government and designed to keep the world at large informed of the progress of scientific research in Russia. His investigations, extending into nearly every branch of the natural sciences, appeared chiefly in *Poggendorff's Annalen* and the *Astronomische Nachrichten*. The most valuable are his researches on terrestrial magnetism. In connection with H. Petersen he calculated the constants for Gauss's theory of terrestrial magnetism, based on his own multitudinous observations. A most valuable contribution to Gauss's theory is also to be found in his work on the magnetic phenomena of the year 1829, which includes a complete study of secular changes based on all then made observations. An equally exhaustive work on the magnetic phenomena of 1860, was left uncompleted at his death. In 1874 Prof. Erman was elected a fellow of the Royal Society.

WITH the view of extending the rudimentary teaching of physiology and the laws of health in elementary schools, the National Health Society has placed at the disposal of the School Board for London, in addition to a sum of 100*l*. offered previously, a further amount of 25*l*. annually for four years, to be given in premiums to those teachers and children who pass the best examinations in these subjects.

SINCE the institution of the Morgue in Paris, unidentified bodies have, it is known, been exposed, unclad, on stone slabs. It has now been decided (we learn from the *Revue Scientifique*) that the dead shall be placed before the eyes of the public just as they have been found, with the proper exception of those who bear on any part of their body a mark which may facilitate recognition. It is anticipated that this measure will increase by a third the number of identifications at the Morgue.—A course of lectures on legal medicine at the Morgue will be commenced in November. This practice has been discontinued for the last fifteen years.

THE Helvetic Society of Natural Sciences is to hold its annual congress at Bex, canton de Vaud, on August 20, 21, and 22.

THE intention is known to have been long cherished to erect a monument in Stockholm to Linnæus, and a sum of 45,000 crowns has been collected for the purpose. There have been two proposals, and to carry out the smaller of these the sum just named would be sufficient. But since the conviction has of late gradually gained strength that the statue should be raised on the so-called "Flora's Bakke" (Flora's Hill), in the Hop Garden, a desire has also grown to realise the larger proposal, according to which Linnæus would appear surrounded by four allegorical figures representing the four sciences to which he devoted himself, viz., Botany, Zoology, Mineralogy, and Medicine. On the understanding that the commune will supply the necessary means for the pedestal and for erection of the monument, a sum of 30,000 crowns was still required to give effect to the larger scheme, and a subscription list has lately been started by thirty influential citizens of the Swedish capital with this object. These thirty have together subscribed 15,000 crowns, and it may be anticipated that the remaining 15,000 will ere long be forthcoming.

WE have recently received a large number of reports of local societies, several of them containing papers of more than local interest, but to which we can refer only in the briefest possible way. The Norfolk and Norwich Naturalists' Society is numerically and financially stronger than at any previous period. Among the twelve papers published in its *Transactions* is one by Prof. Newton, giving an interesting account of the naturalisation of the Edible Frog (*Rana esculenta*) in Norfolk. Mr. Randall Johnson contributes an approximate list of the extinct mammalia of Norfolk.—The *Annual Report* of the Manchester Scientific Students' Association speaks favourably of its position and prospects, as does also the *Report and Proceedings* of the Manchester Field Naturalists and Archæologists' Society in the case of that Society.—The Cardiff Naturalists' Society is a large one and its thick *Report and Transactions* for 1876 contains several good papers.—Other Reports or Proceedings received are from the Bath Natural History Society, the Miners' Association of Cornwall and Devon, the South London Microscopical and Natural History Club, the Croydon Microscopical Club, the East Kent Natural History Society, the Geological and Polytechnic Society of the West Riding of Yorkshire, the Torquay Natural History Society, the Brighton and Sussex Natural History Society, Quekett Microscopical Club, and the York School Natural History, Literary, and Polytechnic Society. Of one or two of the papers in the Natural History Transactions of Northumberland and Durham we hope soon to give a detailed notice.

THE heat conductivity of hardened caoutchouc has been recently determined by Prof. Stefan, of Vienna. With six plates of equal thickness a parallelepipedal vessel was formed, and arranged as an air thermometer. The apparatus having acquired the temperature of a regularly-tempered room, it was inserted quickly in a vessel of broken ice; the time of insertion, and the position of the mercury in the manometer immediately observed, and then the times noted at which the mercury reached particular heights. The thermometric conductivity was found about $0{\cdot}000928 \frac{\text{cm}^2}{\text{sec.}}$. Taking the specific heat of unit mass of vulcanised caoutchouc $= 0{\cdot}23$, and the specific gravity $= 1{\cdot}22$, it follows that the thermal conductivity $= 0{\cdot}00026$.

THE American expedition round the world, recently organised by Mr. Woodruff, of Indianapolis, is to start in October, and continue two years. Among the naturalists that have been engaged are Prof. Burt G. Wilder, of Cornell University, Dr. W. G. Farlow, Prof. Jenney, of Michigan University, Prof. Sidney J. Smith, of Yale College, Prof. S. C. Russell, of the School of Mines, Columbia College, C. Hart Merriam, and Dr. J. H. Kidder, U.S.N. The number of students is limited to eighty. The whole expense to each student is $5,000.

AT the last session of the Berlin Photographic Society a report was presented on the extent of photography in Germany from which we glean some interesting statistics. In the German Empire there are 3,000 photographers, who require each on an average 3 lbs. of nitrate of silver annually. Austria possesses the same number of photographers, but the average annual consumption of nitrate of silver is but 1 lb. Germany manufactures 20,000 reams of albuminised paper annually, of which but 1,000 are for home use. 40,000,000 cartes-de-visite were prepared in Germany during 1876.

THE Italian Committee for the exploration of Africa held its first session in June at Turin, under the presidency of the Crown Prince. It was decided to co-operate vigorously with the International Committee. The Italian Committee will devote its energies at first to the establishment and maintenance of a station at Shoa, where the Marchese Antinori is at present, regarding this as one of the most desirable positions from which to send out expeditions into the interior of Africa.

THE Scottish Meteorological Society holds its half-yearly general meeting in Edinburgh to-day. The business includes reports by Mr. Buchan on the temperature of the sea at Peterhead, and on the relations of the herring fishery to meteorology, for the four years 1873-76.

THE first number of Prof. Hoppe-Seyler's new quarterly journal, the *Zeitschrift für physiologische Chemie* has appeared. It contains valuable papers on the process of urea-formation in animal bodies, and the influence of ammonia salts on this

(Salkouski).—On aromatic substances in the animal body, and determination of sulphuric acid in urine (Baumann).—On animal and plant albuminous bodies (Weyl).—On lactosuria (Hofmeister); and on the physiology of lactic acid (Spiro). The journal is a decided acquisition to scientific literature.

We have received the programme of excursions of the Manchester Field Naturalists' and Archæologists' Society for July to October. The seven excursions arranged (for Saturdays) appear to be of a varied and interesting character.

The additions to the Zoological Society's Gardens during the past week include an Egyptian Gazelle (*Gazella dorcas*) from North Africa, presented by Mr. H. B. Benson; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mrs. Escott; a Burmeister's Cariama (*Chunga burmeisteri*), a Brazilian Stilt Plover (*Himantopus brasiliensis*) from Buenos Ayres, two Black Swans (*Cygnus atratus*) from Australia, two Piping Guans (*Pipile cumanensis*) from Bahia, an Urumutum Curassow (*Nothocrax urumutum*) from Brazil, deposited; a Wapiti Deer (*Cervus canadensis*), an Indian Muntjac (*Cervulus muntjac*) born in the Gardens.

ASTRONOMICAL SYMBOLISM OF THE EAST[1]

THE two stars which in the book of Job are connected with Orion, and to which the Indian Orion-legend referred, are connected with the two red stars or Rohini of Indian traditions. It is shown that these two fixed stars, observed as contemporaneously rising and setting on the horizon, formed the unchangeable starting points for regulating the lunations, and that they thus brought about in course of time an absolute correct chronology. The early discovery of equinoctial precession led to the substitution of these fixed stars by the changeable equinoctial points, till Copernicus, by separating the latter from the solar path, re-established the correct measurement of time by referring the solar motion to fixed stars. The determining single stars, later, constellations nearest to the equinoctical points, to which former, both Chinese and Persian, traditions refer, became the symbol of the order manifested by the heavenly bodies, which cosmical order was attributed to the Deity. The symbol of the two cherubs or kirubs, that is "bulls," in the language of cuneiform inscriptions, are shown to have referred to the rising and setting of the constellation of Taurus, which being called Kirub at its rising, was called Seraph, or Ser-Apis, literally, "the grave of the bull," at its setting. The Pleiades in the neck of Taurus stand in the same relation to this constellation as the god Sebaot, the god of the Sheba-ut, or seven stars, the Sibut of the Babylonians, to the Cherub. The symbol of the chariot of the Cherubim, and of Jehovah riding on the Cherub, as the Pleiades may be said to be riding on Taurus, are thus astronomically explained, and connected with the representations of Ormuzd riding on the winged bull, as also with similar Mithraic representations. The fortnightly period of the Hebrews, from the new moon to the full moon, in connection with the precessional cycle of seventy-two years, probably known to the Hebrews, is shown to have formed the basis of the Osiris-Typhon legend, which was fully developed before the commencement of Egyptian history. The fourteen divisions of the litanies of Thot, the god riding on the moon, and whose secret number was seventy-two, are explained by reference to fourteen moon-stations of the lunar zodiac, the hidden Mazzaroth or mansions of the moon in the book of Job, and to the precessional cycle of seventy-two years, with which can be connected the solar year and the Phœnix period of the Egyptians, the Saros of the Babylonians, and the Mosaic period of one day like a thousand years, as well as several other Babylonian periods; also the number of the sons of Japhet, and the genealogies in Luke from Seth to Joseph, the husband of Mary. When the solar zodiac had taken the place of the lunar zodiac, when the two determining fixed stars had been replaced by the changeable equinoctical points, and the commencement of spring and of autumn became the fundamental symbol of all religions, the ideal heroes of light were connected with the spring-equinox, at first in Taurus, and the ideal heroes of darkness with the autumn-equinox, at first in Scorpio, close to which is the constellation of the Serpent. Thus to Ormuzd, Indra, Osiris, Dionysos and Apollos, were respectively opposed the serpent deities Ahriman, Ahi, Typhon, the Titans and Python. So also the Messiah, "The Sun of Righteousness," and "the day-spring from on high," was opposed by Satan, literally the antagonist, "the old serpent" the devil. The transition from the sacrifice of bulls to the sacrifice of lambs, and the vicarious and sin-removing character of these sacrifices in pre-Abrahamic times, is shown to have been connected with, and probably to have been caused by equinoctial precession, by Aries having taken the place of Taurus. Some of the mysteries of the Great Pyramid are explained by Eastern astronomical symbolism and the two passages pointing north and south are shown probably to have referred to the approximatively contemporaneous midnightly culminations of Aldebaran and Antares at the autumn equinox, as observable at places in the latitude of the Great Pyramid, and likewise in the latitudes of Bactria and Northern India, during a period of about 150 years, within which the year 3300 B.C. falls. The fact that Chinese, Indians, and Arabians, at a remote period, counted twenty-eight moon stations, but that there is nowhere a trace of twenty-nine mansions of the moon, is submitted as possibly implying a date for the earliest astronomical observations of the East transmitted to us, at a time when the lunar month, now having a duration of about twenty-nine and a half days have only twenty-eight days, or rather, not yet twenty-nine. As the mean motion of the moon is the same in long periods, this period of twenty-nine days would have probably commenced about 600,000 years ago, if the retardation of the earth's rotatory motion, by which alone the prolongation of the lunar month seems to be explainable, really does amount to twenty-two seconds in a century, as now asserted by high authorities.

THE NORWEGIAN EXPEDITION TO THE NORTH SEA

FROM a letter by Prof. G. O. Sars, in the *Christiania Dagbladet*, dated Bodö, June 24, we make the following quotations with reference to the progress of the Norwegian expedition which left Bergen on June 11:—

"On the 16th of June we had arrived sufficiently far northward to commence our labours, and sounding-lines, thermometers, dredges, and trawling-nets were at once called into use. Since then the work has been pursued unremittingly, despite stormy weather, and we have every reason to be satisfied with the results so far. The hitherto-unknown contour of the sea's bottom between Folden fiord and the Lofoten Islands is now so clearly ascertained by means of our transverse section, that we can map out to the north with a certain degree of precision the curve of the extended barrier, which keeps back the cold water coming from the depths of the Polar Sea. We have found the curve somewhat different from our expectations, especially in the neighbourhood of the Lofoten. The soundings appear to indicate the presence of a remarkable indentation, similar to the one on the southern part of the coast, and we have found a precipitous slope of the sandbank resembling that of the well-known "Storeg" near Aalesund. It is evident that we have encountered here a most important submarine conformation. The consideration of its effect on our sea-fisheries will be delayed until more detailed surveys have been carried out. In the course of our soundings on the way to Bodö, we were able by means of the improved Negretti-Zambra thermometer to establish beyond the range of doubt the presence of a layer of warm water below a layer of cold water of considerable depth. . .

"Our zoological acquisitions have been highly satisfactory, especially those in the cold zone. We have added several species to the list of the previous expedition; amongst them some hitherto detected in the Polar Sea only and others entirely new.

"The voyage will be pursued to Röst, where several days will be spent in magnetic observations, and in gathering zoological specimens. The latter promise to be of value on account of the zoographical interest of the locality, which has as yet been left unvisited. The section from Röst will be followed carefully in order to determine with certainty the expected bend in the sandbank. The progress of our expedition shows us more and more the fundamental importance of an accurate knowledge of the physical nature of the North Sea, not only for Norway, but also for the solution of the general questions with regard to the physical and biological conditions of the ocean in general."

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[1] Abstract of paper read at the Society of Biblical Archæology, "On Astronomical Symbolism of the East, as transmitted by Hebrews and Christians," by M. Ernest de Bunsen.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Berlin.—No small degree of excitement has been caused in the university circles of Germany by the expulsion of a Privat-docent, Dr. Dühring, from the University of Berlin, not only on account of the great rarity of the occurrence, but on account of the circumstances leading to the act. Dr. Dühring, who has suffered from blindness since the commencement of his pedagogic duties in 1865, has developed, notwithstanding, a remarkable degree of mental activity in a variety of directions, issuing works on political economy, metaphysics, mathematical physics, &c., and attracting a numerous auditory by the radical and unfettered character of his utterances on these subjects. His unsparing denunciation of the more shadowy side of German universities, the system practically in vogue with regard to the appointment of professors, &c., has induced a somewhat wide-spread feeling of bitterness towards him, which has led to an accusation before the university based on the unmeasured personal criticism of his colleagues, especially Prof. Helmholtz, in lately issued works. The expulsion has led to the holding of meetings and issuing of addresses by the more radical portion of the students, who regard the section of the Prussian constitution, "die Wissenschaft und ihre Lehre ist frei" as endangered; and the whole affair has led to a thorough ventilation of the condition of the universities in the press of the empire.

Russia.—The Russian Government expends 2,140,000 roubles (over 300,000*l.*) on its seven universities, of which sum the Moscow University receives the most—425,000 roubles.

Salaries of German Professors.—From a recent statement with regard to the salaries in the Berlin University, we notice that the two well-known professors of chemistry and physics receive each 1,500*l.* independent of the lecture receipts, while 900*l.* is the salary of a number of other leading professors. These figures are, however, much above the corresponding ones of other German universities.

Strassburg.—According to an imperial order the University receives in the future the title "Kaiser Wilhelms Universität." The present attendance—658—shows for the first time since the re-establishment of the University a decrease in the number of students. The faculty embraces 106 professors and privat-docenten. The large anatomical laboratory is approaching completion.

Tübingen.—The present number of students in attendance on the University amounts to 1,103, the largest number attained since its foundation. Extensive preparations are being made for the celebration in August of the 400th year of its existence.

Zurich.—The University has at present an attendance of 324, of which the majority are in the medical faculty. Of the seventeen female students fourteen study medicine and three philosophy.

SCIENTIFIC SERIALS

The *Quarterly Journal of Microscopic Science* for July contains several articles of special interest. First is the fourth part of Mr. Archer's *résumé* of recent contributions to our knowledge of "Fresh-water Rhizopoda," in which *Lecythium hyalinum*, *Chlamidophrys stercorea*, *Platoum parvum*, *Gromia paludosa*, and *Cyphoderia truncata* are described and figured.—Following is an abstract of Mr. E. C. Baber's researches on the lymphatics and parenchyma of the thyroid gland of the dog, with illustrations, published at length in the *Philosophical Transactious*.—Dr. Watney also, from the same *Transactions*, gives, with a plate, an account of his study of the minute anatomy of the alimentary canal.—Dr. Angelo Andres describes and figures a new genus and species of *Zoanthina Malacodermata* (*Panceria spongiosa*).—Prof. Franz Boll follows with his "Contributions to the Physiology of Vision, and of the Sensation of Colour."—Mr. A. Sangster has observations on the muscular coat of sweat glands, in which it is shown that the epithelium of the gland rests directly upon the muscle surrounding it.—Dr. Klein contributes a paper on the minute anatomy of the omentum, describing certain bud-like structures occurring on the fenestrated portion, the method of production of the fenestræ, and some points in the formation of the blood-vessels.—Mr. F. Darwin writes on the protrusion of protoplasmic filaments from the glandular hairs on the leaves of the Common Teasel, in which he helps to develop the important principles of the incipient steps in the formation of special organs.—Mr. H. N. Moseley gives notes on the structure of several forms of Land Planarians, with a description of two new genera and several new species, and a list of all species at present known.—Notes and memoranda conclude the number.

Bulletin of the Buffalo (N.Y.) *Society of Natural Sciences*, 1877. This society has published a valuable list of the freshwater fishes of North America by Mr. D. S. Jordan. Mr. M. C. Cooke is contributing a similarly exhaustive list, with references, of the hyphomycetous fungi of the United States.—An interesting paper by Mr. F. S. Dellenbaugh gives some account of the so-called Puebla Indians of the Rocky Mountain region, also known as Shinumos. They were not really of Indian race; they seemed to have died out in consequence of the incursions of the true Indians. The name Moquis, by which seven of their villages are designated, signifies "the dying race." In their retreat before the enemy they occupied the most inaccessible retreats in the cañons, built cliff houses, cultivated minute patches on the cliffs, lived in caves, &c. They appear to have had considerable artistic skill, from the designs, and even paintings, left on sandstone in the walls of houses, &c. The writer urges a careful exploration of all the extant remains of these people, for they are rapidly being destroyed by careless and ignorant settlers.

SOCIETIES AND ACADEMIES

London

Meteorological Society, June 20.—Mr. H. S. Eaton, M.A., president, in the chair.—Henry Hearder and Henry Law, M.Inst.C.E., were elected fellows of the Society.—The following papers were read:—On an improvement in the mechanism of self-recording meteorological instruments, by the Rev. C. J. Taylor. In order to obviate sluggishness in self-recording aneroids and ordinary small-bore mercurial barometers, the author places a small electric bell apparatus, from which the bell has been removed, so that the clapper when in action shall strike on the top of the vertical brass bar on which the recording pencil slides; by this means a very rapid succession of light taps can be administered at a point which affects all the moveable parts of the mechanism.—Results derived from the sunshine records obtained at the Royal Observatory, Greenwich, by means of Campbell's self-registering sun-dial, during the year ending April, 1877, by W. Ellis, F.R.A.S. The instrument consists of a very accurately-formed sphere of glass four inches in diameter, supported concentrically within a well-turned hemispherical metal bowl in such a manner that the image of the sun formed when the sun shines falls always on the concave surface of the bowl. A strip of some material being fixed in the bowl, the sun, when shining, burns away the material at the points at which the image successively falls, by which means a record of periods of sunshine is obtained. The duration of sunshine in hours for each month of the year ending April 30 (excepting from May 1 to 6, July 31, and October 27 to 31), was as follows:—

	hrs.		hrs.		hrs.		hrs.
May	152·3	Aug.	216·9	Nov.	35·9	Feb.	36·4
June	184·5	Sept.	106·1	Dec.	6·5	Mar.	99·3
July	214·3	Oct.	47·3	Jan.	18·7	Apr.	71·8

The greatest daily duration was 13·9 hrs. on June 11, daily durations exceeding 10 hrs. occurred six times in May, eight in June, ten in July, and eleven in August. One of the most remarkable periods was August 7 to 14, the duration having exceeded ten hours on every day during this time. It appears that in the months of August, September, and October, the mean maximum and minimum temperatures were both, on the average, higher on days of greater sunshine than on days of lesser sunshine, whilst in the months of January and February, an exactly opposite condition existed. In the remaining months the mean maximum temperature of the greater sunshine group was higher, and its mean minimum lower, than the corresponding mean maximum and minimum temperatures of the lesser sunshine group. It also appears that there was more sunshine after noon than before noon in every month, except August, March, and April.—On the diurnal variation of the barometer at the Royal Observatory, Greenwich, by W. Ellis, F.R.A.S. The Astronomer-Royal having communicated the numerical values of the variations of the barometer as deduced from the photographic records at the Royal Observatory, during the twenty years ending

1873, the author adds a few general remarks thereon. Comparing together the different months of the year, it is observed that the morning minimum and the forenoon maximum both occur earlier as the year advances; they are earliest in summer, and become later again on the approach of winter. The afternoon minimum and evening maximum, on the contrary, occur later as the year advances; are latest in summer, and become earlier again towards the end of the year. They all change in a certain degree with the changes in the times of sunrise and sunset. As a consequence of this the intervals between the morning minimum and the forenoon maximum, and between the afternoon minimum and evening maximum do not change very much through the year, whilst that between the forenoon maximum and afternoon minimum is much shorter in winter than in summer, and that between the evening maximum and morning minimum is much longer in winter than in summer.—On the rainfall of Jamaica during the seven years 1870-76, by Griffith N. Cox.—Contributions to the meteorology of Cannes, by William Marcet, F.R.S.—Mr. Marriott exhibited and described Bogen's hygrometer and new standard siphon barometer. The barometer possesses the following special features:—It is so constructed that it can easily be put together and taken to pieces again. The long leg consists of a tube of the same diameter throughout, and is supplied with a glass stopper which has a very fine bore passing through its centre, enabling one to fill the tube with mercury in one or two minutes and to completely exhaust it of air in one or two minutes more. The open end of the long leg is ground air-tight to the short (curved) leg and can be instantaneously put together or separated. The long tube alone is graduated so as to have only one scale, but by a peculiarly-constructed screw of given length placed on the short tube a precise and accurate reading can be easily obtained. The barometer is mounted on a peculiarly-shaped stand which has three adjusting screws by means of which the perpendicularity of the instrument can be ensured.

PARIS

Academy of Sciences, July 16.—M. Peligot in the chair.—The following papers were read:—Anthrax and septicæmia, by MM. Pasteur and Joubert. Anthrax may be called the disease of (what the authors call) *bacteridie*, as trichinosis is the disease of the trichina. The blood of a healthy animal contains neither microscopic organisms nor their germs. That of an animal having anthrax contains no organisms but the *bacteridie*, which is aerobic, so that the anthracic blood is imputrescible of itself. In the carcase the blood putrefies through vibrios lodging in it. The *bacteridie* disappears in liquid, in presence of CO_2. It is a mistake that putrefaction, as such, destroys the anthracic virulence. The development of the *bacteridie* takes place difficultly when in the presence of other organisms. These observations the authors apply to certain facts of practical experience. They state that septicæmia is produced by a *vibrionien* as anthrax is by a *bacteridie*.—Experiments, according to which the fragmentary form of meteoric irons may be attributed to a rupture under the action of strongly compressed gases, such as arise from explosion of dynamite, by M. Daubrée. To steel prisms were attached charges of dynamite, which were exploded in clay pits, so that the parts could be brought together after rupture. The alveoli produced (often grouped) with projecting rim, were of more pronounced character than those from powder. The surface was sometimes raised in long bulging ridges. The surfaces of fracture were some of them polished, others striated.—Researches on the tertiary strata of Southern Europe, by M. Hebert. This first paper relates to the eocene and lower miocene of Hungary.—M. Dumas, referring to a recent paper by M. Sée, affirmed that the real inventor of salicylic acid was the eminent Italian chemist M. Piria.—On a disease of the grape in the vineyards of Narbonne (June and July, 1877), by M. Garcin.—Observations of D'Arrest's periodic comet at the Marseilles Observatory, by M. Stephan.—Note on the theory of quadratic forms with any number of variables, by M. Frobenius.—Demonstration of two geometric laws enunciated by M. Chasles, by M. Fouret.—On the division of the circumference into equal parts, by M. Lucas.—Researches on the compressibility of liquids (continued), by M. Amagat. *Inter alia*, the coefficient of compressibility increases when the pressure increases for all liquids in which it increases with the temperature. The compressibility of successive terms of the family of formenic carburets decreases regularly as we descend in the series, both at 100° and at ordinary temperature. The presence of sulphur, chlorine, or bromine, in liquid bodies, tends to render them less compressible.—On the electric and capillary properties of mercury in contact with different aqueous solutions, by M. Lippmann. When mercury is in contact with pure or acidulated water, the addition of a small quantity of certain substances to the water changes notably the capillary constant and the electromotive force. M. Lippmann proves that for each value of the latter the capillary constant has one, and only one, determinate value independent of the chemical composition of the liquid. That is, if for two different combinations the electromotive force is the same, the capillary constant is also the same.—On the vapours of alcoholate of chloral, by M. Troost.—Action of light on hydriodic acid, by M. Lemoine. Hydrogen and iodine do not sensibly combine in the cold state under the influence of light. It may be inferred that the decomposition of hydriodic acid by the sun is unlimited. The slow decomposition in light might be used to measure the degree of illumination of the sky in large meteorological observatories. In a month of insolation in the cold state 0·80 of gaseous hydriodic acid is decomposed, whereas heating one month at 265° decomposes only 0·02, and even at 440° during a few hours, only 0·20.—Note on a new derivative of indigotine, by M. Schutzenberger.—On the properties of resorcine; thermochemical studies, by M. Calderon.—On the reform of some processes of analysis used in the laboratories of agricultural stations and observatories of chemical meteorology; second memoir, acidimetry, by M. Houzeau.—On the nature of acids contained in the gastric juice (continued), by M. Richet. There is in gastric juice an organic acid soluble in ether, and it is probably sarcolactic acid.—Note on numeration of blood corpuscles in diphtheritis, by MM. Bouchut and Dubrisay. In this disease there is a considerable increase in the number of white corpuscles, and a diminution of that of the red.—On the influence of excitations of organs of sense on the heart and the vessels, by MM. Couty and Charpentier. Such phenomena seem to be produced, not by the sensorial perception itself but by an ulterior cerebral work which might be called *emotional*.—Experiments proving that neither pure air nor oxygen destroy the septicity of putrefied blood, by M. Feltz.—Researches on a case of congenital ectopia of the heart, by M. François-Franck.

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THURSDAY, AUGUST 2, 1877

THE PHYSICAL BASIS OF MIND

The Physical Basis of Mind, with Illustrations, being the Second Series of Problems of Life and Mind. By George Henry Lewes. (Trübner and Co., 1877.)

WHEN the first volume of "Problems of Life and Mind" appeared, I ventured to say that perhaps Mr. Lewes promised too much in undertaking to exhibit "how the sentient phenomena may be explained by neural phenomena." I also directed a criticism, as pointed as I could make it, against a proposition placed by Mr. Lewes at the foundation of his psychology; namely, that "actions are prompted and really guided by feeling." The present volume is, in addition to much else, Mr. Lewes fulfilment of his promise and his reply to the criticism.

Considering the limited amount of space at my disposal, I shall, I believe, put it to most advantage by confining myself to these two points. As regards the first—the possibility of finding a physical basis of mind—a sentence in the preface rouses misgivings: "Materialism, in attempting to deduce the mental from the physical, puts into the conclusion what the very terms have excluded from the premisses;" "the attempt to interpret the one by the other" is a legitimate undertaking only "on the hypothesis of a physical process being only the objective aspect of a mental process." This is of ill omen; that which can be done as science does not seek its justification in metaphysics. But let the interpretation be taken on its merits. What is it? Though prepared for disappointment, readers will doubtless be surprised to hear that when looked for, it is nowhere to be found. "The sensation, or state of consciousness," says Mr. Lewes, "is the ultimate fact; we can only *explain* it by describing its objective conditions." In place of the second proposition, "we can only *explain*," &c., most thinkers prefer to say we *cannot* explain it, we can only describe its objective conditions. The difference, then, between Mr. Lewes and others, is not that he has any new light to offer, but that he insists on calling that an explanation which others cannot see to have that character. The sense in which Mr. Lewes thinks he can correctly call a description of neural processes an interpretation of mental facts rests on his statement of the metaphysical hypothesis that these are but "different aspects," "the two faces of one and the same reality." "It is thus indifferent," he continues, "whether we say a sensation is a neural process or a mental process; a molecular change in the nervous system, or a change in feeling." Suppose all this to be understood and granted, where is the explanation or interpretation of the one by the other? Is a description of one aspect of a thing an explanation of a very different aspect of the same reality? Not even metaphysical legerdemain can give the illusion of a physical basis of mind. Mr. Lewes sees that it is impossible to conceive a neural process as *causing* the mental process. He does not say that molecular movement becomes, or is transformed—in any sense, conceivable or inconceivable—into sensation. Mind is not the outcome of physical conditions or combinations; it is an *aspect*, "the spiritual aspect of the material organism." Readers may now judge whether Mr. Lewes can claim to have explained sentient phenomena by neural phenomena, to have shown the manner in which the Self and Not-self "are combined in feeling and thought."

Against Mr. Lewes' proposition that the movements of living beings are prompted and guided by feeling, I urged that science has carried us to a point at which we have but to pause and reflect to see that all movements must be the consequents of purely physical antecedents; that the amount and direction of every nervous discharge must depend solely on physical conditions. And I contended that to see this clearly is to see that when we speak of movement being guided by feeling, we use the language of a less advanced stage of enlightenment. This view has since occupied a good deal of public attention. Under the name of Automatism it has been advocated by Prof. Huxley, and with a firmer logic by Prof. Clifford. It has been argued about in the *Spectator*, zealously combated by Dr. Carpenter, and now Mr. Lewes makes it the subject of one of his Problems, devoting seven chapters to its discussion.

Mr. Lewes cannot think that Prof. Huxley really holds the repulsive doctrine in question, though "*supposed* to hold (it) by those whom his expressions mislead." Yet, curiously enough, it is against Prof. Huxley's statement that Mr. Lewes' polemic is specially addressed. It is not my affair to reply for Prof. Huxley. Mr. Lewes has, however, mentioned me as having insisted "with iterated emphasis" on the view he now "most earnestly desires to refute." I must give my own statement. Here it is as given in my review of "Problems of Life and Mind" (*The Examiner*, March 14, 1874):—"Using the word feeling in its ordinary acceptation, as a name for subjective phenomena alone, we assert not only that no evidence can be given that *feeling* ever does prompt or guide action, but that the process of its doing so is inconceivable. How can we picture to ourselves a state of consciousness putting in motion any particle of matter, large or small? for this is really what it comes to. . . . Puss, while dozing before the fire, hears a slight rustle in the corner, and darts towards the spot. What has happened? Certain sound-waves have reached the ear, a series of physical changes have taken place within the organism, special groups of muscles have been brought into play, and the body of the cat has changed its position on the floor. Is it asserted that this chain of physical changes is not, at all points, complete and sufficient within itself? Mr. Lewes, we believe, will not assert this; he will admit that the material succession is unbroken. Once more, then, in what sense can we take the proposition that actions are prompted and really guided by *feeling?*" Putting in the place of my cat hunting for a mouse, the analogous case of a wolf springing on a sheep, Mr. Lewes replies: "Unless the term *physical* is here used to designate the *objective sequence*, as contemplated by an onlooker, who likens the process to the sequence observable in a machine, I should say that from first to last the process has been *not* physical, but *vital*." The word "unless," with which the reply opens, might be objected to, as implying that the term "physical" might be here employed to designate something else than the objective sequence—that succession of external events which can be seen or imagined in terms of vision. Quite irrelevantly, as it seems to me, Mr. Lewes specifies a particular kind of on-looker—one who likens the process to the sequence observable in a machine. I will only say that for myself I decline the honour of a place among those physiologists and philosophers who, according to Mr. Lewes, have failed to perceive the "radical difference between organic and inorganic mechanisms." However, Mr. Lewes has put it on record that *if* when I spoke of a series of physical changes taking place within the organism I meant series of *inorganic* changes—that the movements of the cat resulted from something of the nature of a combination of levers, springs, and pulleys, then, he "should say that from first to last the process has been *not* physical but *vital*." And who will question that Mr. Lewes would be quite right in so saying? But why suppose anything so unlikely? Yet this is the meaning Mr. Lewes gives to the word "physical" when it occurs in the mouths of those against whom he directs his arguments. For instance: physiologists are in the habit of describing unconscious reflex movements as physical processes. Of this description Mr. Lewes says: "Restate the conclusion in different terms and its fallacy emerges; 'organic processes suddenly cease to be organic, and become purely physical by a slight change in their *relative* position in the consensus.'" But to proceed. Not having used the word "physical" in any peculiar sense, but in accordance with ordinary usage, my question remains—"Is it asserted that the chain of physical changes is not, at all points, complete and sufficient within itself?" So far is Mr. Lewes from denying the physical succession to be unbroken, that he states this, or something very like it, over and over again, as a truth almost too self-evident to require expression. Thus we read: "So long as we are dealing with the objective aspect we have nothing but material processes in a material mechanism before us. A change within the organism is caused by a neural stimulation, and the resulting action is a reflex on the muscles. Here there is simply a transference of motion by a material mechanism. There is in this no evidence of a subjective agency; there could be none." But we also find statements that seem to have a contrary implication. Here is one: "The physiologist is compelled to complete his objective observations by subjective suggestions; compelled to add feeling to the terms of matter and motion, in spite of the radical diversity of their aspects." How is he *compelled* to infer that of which Mr. Lewes has just told us there could be "no evidence"? Again, while the volume abounds with detailed descriptions of the behaviour of dogs, frogs, and men, given as instances in which it is "evident enough," to Mr. Lewes, that their actions were "determined by sensations, emotions, and ideas," yet Mr. Lewes is equally positive that we are "passing out of the region of physiology when we speak of feeling determining action. Motion may determine Motion, but Feeling can only determine Feeling." Where, then, are we, when we talk of feeling determining action? In, I maintain, the gray morning of that intellectual light which is still far from having reached its noon-day splendour.

In the minds of our savage ancestors *feeling* was the source of all movement. Every one of them had what Mr. Lewes, after all he has written about scientific method, can call "the irresistible evidence each man carries in his own consciousness, that his actions are frequently—even if not always—determined by feelings;" and they spoke according to their light. But while we shall continue to speak of feeling determining action, it will only be as we speak of the rising and the setting of the sun. Mr. Lewes is of a different opinion. He says: "We do so speak and are justified. For thereby we implicitly declare, what psychology explicitly teaches, namely, that these two widely different aspects, objective and subjective, are but the two faces of one and the same reality." If Mr. Lewes did not go farther than this I should not care to quarrel with his endeavour to put a new metaphysical meaning into the language of old error. But he thinks that on the strength of this hypothesis the material succession may be regarded as unbroken, and yet a rational interpretation found for the proposition—actions are prompted and really guided by feeling. Because the molecular changes in the brain which form part of the series of material changes involved in the production of motion may be held to be, in a metaphysical sense, the other side of what we know as feeling, Mr. Lewes somehow concludes that "we must declare consciousness to be an agent (in the production of motion), *in the same sense that we declare one change in the organism to be an agent in some other change*" (the italics are by the author). Let us see. The word "consciousness" here denotes two things assumed by Mr. Lewes to be two faces of one thing. If we substitute for this word one of these denotations and say "we must declare the molecular changes involved in the production of motion to be an agent, &c.," the statement becomes the most empty tautology. If we give to the word "consciousness" its other meaning—*feeling*—the proposition becomes what Prof. Clifford calls "nonsense;" and is, as Mr. Lewes says, placing feeling where "there is obviously no place for it—among material agencies." If by "consciousness" Mr. Lewes means neither the molecular changes nor the feeling, but the something of which both are but aspects, then he is altogether beyond science, and for the moment it is enough to say that this metaphysical entity is *not* an agent "*in the same sense*," &c.

Corresponding to those feelings, which Mr. Lewes will have it inspire and guide movement, there are conditions of the organism which can be conceived as the causal antecedents of the movements—the feelings, as admitted, cannot. Our instinctive faith in the unity and constancy of things leaves us no room to doubt that identical organic conditions will ever be accompanied by identical feelings and followed by identical movements; but this does not bring into view any scientific sense in which the feelings can be said to inspire and guide the movements. These for ever remain parts of an infinite series of physical consequents following on physical antecedents. This is the thesis at present so repulsive to many minds. Against this Mr. Lewes has nothing to advance. If any look to him for comfort they will find that, promising them bread, he gives them a stone—the same stone that has already set their teeth on edge.

One word to correct a false impression that the foregoing critical remarks would leave on minds unacquainted with Mr. Lewes' writings. Let no one suppose that I have not read the book with admiration. Like all Mr. Lewes' works, it is a repertory of suggestive fact and of equally valuable and suggestive thought; and if any reader derive from its perusal a tithe of the intellectual stimulation it has afforded me, he may regard his time as well spent. Reflective minds are diligently working towards clearer conceptions in a region that has hitherto been all obscurity. There is reason to believe that ere long philosophic thinkers of the highest rank will for the first time agree as to one or two fundamental conceptions. Few living men have done as much as Mr. Lewes to usher in this new era. Knowing my criticisms to be inspired solely by the same impersonal motives by which he has himself been sustained throughout his extensive labours, I am sure Mr. Lewes would be the last person to suggest that I could have made better use of the space at my disposal. Others, better qualified than myself, will draw attention to the importance of those parts of the work that I have not mentioned, as, for instance, the splendid essay on the Nervous Mechanism.

DOUGLAS A. SPALDING

GORE'S "ELECTRO-METALLURGY"

The Art of Electro-Metallurgy; including all known Processes of Electro-deposition. By G. Gore, LL.D., F.R.S. Text-books of Science Series. (London: Longmans, Green, and Co., 1877.)

DR. GORE has evidently spared no pains to make this text-book a complete manual of the art of electro-metallurgy. Beginning with the history of the subject, he gives an interesting account of the rise and development of the art, full of names and dates and references, and makes the early inventors tell, as far as may be, their own story by quoting freely from their published papers. Then comes a "theoretical division," about which we have something to say presently, and this is followed by what forms the greater part of the work—a detailed account of practical methods of depositing the various metals. This portion of the book, at once thoroughly circumstantial and comprehensive, cannot fail to prove most useful to the practical electroplater as well as to the scientific student. The metals most commonly employed in the arts receive, of course, most attention; but almost none, even of the rarest metals, pass without notice, and the experiments are described with the precision that comes only of experience. An admirable feature of Dr. Gore's book is the habit he has of giving specific references to the authorities he makes use of, so that any one with a library at his command may, if he choose, turn up the passages cited. The remainder of the book is filled by a "special technical section" containing various practical directions and details, and, in conclusion, we have a list of the books previously published on the subject and of the English patents referring to electro-metallurgy. The author is to be congratulated on the accumulation and systematic arrangement of an immense mass of information of a kind that will be welcomed alike in the workshop and in the laboratory.

If Dr. Gore had given us only the practical parts of his book we should have had little to say beyond praise and thanks. Unluckily, however, for himself as well as for his readers, he has introduced a chapter on the theoretical principles which underlie the art of electro-deposition. Such theoretical *réchauffés* are often to be found in practical text-books, but their existence is surely a thing to be protested against even when they are tolerably well written. No one can hope to give a satisfactory account of chemical and electrical theory in fifty pages, and when his work is to form one of a series in which chemistry and electricity have already been treated of in separate books, the attempt is not only useless but unnecessary. These short abstracts are certainly not to be recommended to the novice; and to the student who has already studied the subjects at greater length they are little short of an impertinence. In a book which stands by itself they might be tolerated if they were at once concise and accurate, giving what is needed and no more. In the case before us these extenuating circumstances are all absent. That Dr. Gore's "theoretical division" is not concise the following quotation will suffice to show:—

> "The strength of the current is equal to the electromotive force divided by the resistance; this is known as Ohm's law; it is directly proportional to the electromotive force, and inversely proportional to the resistance; if the resistance remains the same, and the electromotive force varies, the strength is directly proportional to the electromotive force; and if the electromotive force remains the same, and the resistance varies, it is inversely proportional to the whole of the resistance in the circuit" (p. 71).

As an instance of matter which might very well have been left out, take the following. After giving a table of conductivities, Dr. Gore proceeds:—

> "If the conduction-resistance of distilled water is so great in relation to that of copper, we can easily ununderstand, by referring to the previous table, that the resistance of gases must be enormous. The electric conduction-resistance of air heated to redness (*sic*) is 30,000 greater than that of water, containing a 20,000th part of its weight of sulphate of copper in solution" (p. 31).

Why this long-buried result of E. Becquerel's (here, by the way, the authority is not cited) should be unearthed for the benefit of students of electro-metallurgy is almost as puzzling as is the strange piece of *à priori* reasoning in the first sentence, which, it is distressing to find, we are expected to understand easily.

The vagueness and inaccuracy of some parts beggar criticism, and leave the reviewer but one weapon—a severe one indeed, but he has no other—he can only quote. Here are a few specimens chosen almost at random.

> "The fundamental act or principle of magneto-electric action is, *wherever there is varying magnetism, there is an electric current induced in an adjacent closed circuit at right angles to it*" (p. 57); the italics are the author's.
>
> "The electromotive force, or strength of the current to overcome resistance, depends upon the degree of difference of strength of chemical affinity of the two metals for the electro-negative constituents of the liquid" (p. 70).
>
> "The electromotive force (commonly called 'the intensity') of the current . . ." (p. 337).
>
> "*As the electromotive force is diminished by resistance, a diminution of resistance in any part of the circuit will increase it*" (p. 337); this extract we have ventured to italicise.
>
> "Motion of the articles is very advantageous . . . it

greatly diminishes the electric conduction-resistance which would be produced by polarisation, due to layers of liquid of opposite electrical nature, collecting in contact with the electrodes" (p. 344).

"*Potential and tension.*—Previous to the completion of the circuit and formation of an unimpeded current, the free ends of the polar wires attached to the two metals are charged with the two kinds of electricity in an accumulated or free static condition, and are in a state of *electric potential*, *i.e.*, possessing a capability of doing electric work. These accumulated electricities in the wires may be detected by means of a very delicate electroscope. The free electricities are also in a state of *tension*, constantly tending to escape and unite; and their degrees of tension may be measured by means of an electrometer" (p. 71).

From which it would appear that the difference between potential and tension lies in the fact that the one is to be detected by an electroscope, and the other measured by an electrometer. It would be just as satisfactory a distinction, and would besides have the merit of being true, to say that "potential" is the shibboleth of the electrically unlearned, while "tension" is their refuge at all times.

Over and over again we find such phrases as these:—"If the current to be measured is one of low electromotive force" (p. 73); "a current of less quantity and greater electromotive force" (p. 338); and after we have been expressly told on p. 72 that there is no difference between currents except as regards their quantity per minute, it is surprising to learn that "as a general rule, the greater the electromotive force, and the smaller the quantity of the current, the harder and brighter is the deposited metal" (p. 344).

But it is needless to multiply examples. We have given enough to show how much Dr. Gore has done to mar a really good book by adopting a precedent which, however well followed, is of very doubtful utility. In the present instance it may, perhaps, serve the good purpose of acting as a warning to future practical writers.

We have noticed comparatively few typographical errors. Is it the author, or the printer, or the author's evil genius, or the printer's devil that we have to thank for this bewildering statement on p. 182?—

"Silver may be cleaned in water in which potatoes have been boiled, and a superior polish is thus imparted to them."

OUR BOOK SHELF

Enumeracion de las Plantas Européas que se hallan como silvestres en la Provincia de Buenos Aires y en Patagonia. Por Cárlos Berg. (Buenos Aires, 1877.)

THIS is a very interesting list of European plants introduced by various means into the two above-mentioned countries. It gives the relative abundance of each species and the conditions under which it is found. Altogether 116 Dicotyledons, 30 Monocotyledons, and 8 Cryptogams are mentioned. Of these no less than 108 are common to Britain. As might be expected, the natural orders Compositæ and Gramineæ, each with 20 species, and Caryophylleæ with 12, are the strongest in point of number of species. Many notes are scattered through the twenty-four pages, from which we learn that under such extremely different conditions some of our British plants attain extraordinary dimensions.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Optical Spectroscopy of the Red End of the Solar Spectrum

BEING now, with my wife, on the return voyage from a private spectroscopic experiment on solar light in Lisbon, there appear to be two or three reasons why I should request your leave to send this preliminary note to NATURE before attempting to publish anywhere a full account of what was seen.

As the *first* of these reasons, I may mention that the continual assistance kindly afforded us by M. Oom, the Astronomer-Royal of Portugal, and the several facilities obligingly granted to us, through his intervention, by the Portuguese Government, render an early and hearty acknowledgment imperative. All the more so, too, as the last and most successful series of observations, through four successive days of blazing sunshine, without the smallest speck or suspicion of a cloud anywhere from morning to evening each day, was made in a new suite of rooms recently prepared for the local astronomer's residence in the Royal Park of the Ajuda.

The *second* reason is the pleasing one to confess, that out of four prismatic arrangements tried in the same spectroscope—the one which had the highest dispersion (viz., 32° from A to H) gave also the best and most satisfactory definition, showing thereby such wondrously fine and minute detail amongst close lines as to cause it to be almost invariably employed, and *that* prismatic arrangement, I am happy to say, was lately made for me by Mr. Adam Hilger, of 192, Tottenham Court Road, London, on his own long-approved plan of three powerful and symmetrical compound prisms, while the eye-piece of the telescope, also by him, was of rock-crystal, and fitted with his peculiar reference line for micrometer mensuration.

The *third* reason is the total contradiction given by the best of these observations to some conspicuous features of the Royal Society's last publication on the red end of the solar spectrum, when seen at a high altitude with their second and most improved "Indian Spectroscope."

Our late Lisbon measures, though made at a station close to the sea-level, were yet, near the noon of each day there, and on a midsummer sun in that latitude, taken through almost the very same *thickness* of atmosphere, as the Royal Society's, and Mr. Hennessey's high-sun series on the Himalaya Mountains. But those Indian observations having been printed in the *Philosophical Transactions* so long ago as 1874, I should be glad to know whether either the Royal Society or anyone else has published further particulars of the extreme red end of the solar *optical* spectrum since then. PIAZZI SMYTH,

Astronomer-Royal for Scotland

The Cretaceous Flora of America

NEAR the close of his very interesting lecture "On the Tropical Forests of Hampshire," published in NATURE (vol. xv. pp. 229, 258, 279) Mr. J. S. Gardner says:—"I have great doubts, however, as to the correct position of many of the foreign so-called cretaceous beds. Those of America, from which most of the list of dicotyledons of this period is derived, appear to me, from the character of their fauna, to be rather lower eocene, or at most, filling in the gap between our chalk and London clay. Most of the shells have a marvellously eocene-like aspect, and I take it that the presence of an ammonite, and some few other forms of shells which in England do not reach above the chalk, should not be taken as conclusive evidence of the antiquity of the bed, as, although migrated from our seas, they may very well have lived on in other regions. It is inconsistent to assume that no ammonite lived on in any part of the world to a more recent period than that of our chalk."

From these remarks it is evident that Mr. Gardner is not fully informed in regard to the evidence which exists on the question he has raised; and as the subject is one of great interest, and one which it is necessary should be carefully understood by those who write upon the progress of plant life on the globe, I take the liberty of reporting briefly what we really know in regard to the cretaceous flora of the North American continent.

Some twenty years ago numerous impressions of angiospermous leaves were brought by Dr. Hayden and myself from the group of sandstones which lie at the base of our cretaceous system. Outline sketches of a part of these were sent by Mr. Meek to Prof. Heer, of Zurich. He pronounced them miocene tertiary. To this conclusion he was led by their high botanical rank, their generic affinities with miocene plants, and the supposed identity of some of them with miocene species.

The announcement of Prof. Heer's decision led to a somewhat earnest discussion, in which Prof. Heer, M. J. Marcou, and Mr. Leo Lesquereux supported the view that the plants in question were tertiary, while Messrs. Meek and myself asserted that they were cretaceous, because the strata which contain them are overlain by more than 2,000 feet of limestones filled with characteristic cretaceous fossils, a number of which are identical with those found in the gault and chalk of Europe. An end was finally put to this debate by M. Marcou and Prof. Capellini, of Bologna, going to Kansas and collecting a large number of these leaves from beds overlain by unmistakable cretaceous strata. The true position of this flora was then not only acknowledged but proclaimed by these gentlemen, and since that time every geologist in America has accepted the statement which I made in my letter to Messrs. Meek and Hayden in 1858, and in my article on the Ancient Vegetation of North America (*American Journal of Science*, vol. xxix., 1860, p. 208), that "the American flora assumed nearly the botanical character it now has in the cretaceous age, and that our lower cretaceous rocks contain the remains of sixty or seventy species of angiospermous trees, many of which belong to our most common living genera, such as *Quercus*, *Salix*, *Magnolia*, *Platanus*, *Liriodendron*, *Fagus*, *Alnus*, *Liquidambar*, &c."

Since the settlement of this question a large number of additions have been made to the then known species of this flora, and it is probably not too much to say that we have obtained leaves of nearly one hundred species of angiospermous trees from the base of our cretaceous system, the equivalent of the upper greensand of England.

All the leaves figured in Lesquereux's "Fossil Flora of the Western Territories," part I., were obtained from this horizon, and a large number of additional species have been described by Prof. Heer in his "Phyllites Cretacea," or by myself in "Our Later Extinct Floras," while many others yet wait publication.

The plants of our upper cretaceous and tertiary rocks have not yet been fully described, and there is some difference of opinion as to where the line should be drawn between these two systems, but it is quite certain that a large part of the species described by Mr. Lesquereux from the "lignite beds," and referred by him to the tertiary, are really cretaceous; not only because they are associated with *Ammonites*, *Inoceramus*, and other cretaceous fossils, but because the strata which contain them underlie unconformably the *Coryphodon* beds, the base of our eocene. Whatever shall be ultimately decided in regard to the line of separation between our later cretaceous and earlier tertiary strata, this will in no wise affect our conclusions in regard to the general facies of the American cretaceous flora. The statements made many years since are confirmed by all fresh evidence, and now stand unquestionable, that between the trias and the chalk—we know nothing of our Jurassic flora—the vegetation of North America was revolutionised, and that at the beginning of our cretaceous age it had assumed essentially the character and consisted chiefly of the same generic elements that it exhibits now.

I may also add that up to the present time no species of *Ammonites*, *Baculites*, or *Inoceramus* have yet been found in America above the cretaceous system; and that so far as we now know, these genera are as decisive of the age of the strata which contain them here as in the Old World. J. S. NEWBERRY

Columbia College, New York, June 19

Meteorological Notes from Lisbon

THE following meteorological notes, compiled in great part from the daily bulletins of the Observatorio Real of Lisbon, supplemented by observations made by myself, by means of a Casella's self-registering thermometer and a good aneroid barometer, during a seven months' residence in that city, may not be without some value to weather observers. I arrived on October 15, consequently the observations for this month refer only to the latter half. The records were made at 9 A.M. and at 5 P.M. To save space the readings will be given throughout (except for October) in the following order:—I. Barometer (reduced to sea-level), (*a*) the average of observations taken at 9 A.M, (*b*) the highest, and (*c*) the lowest reading of the month. II. Thermometer (Fahrenheit), (*a*) average of daily observations made at 9 A.M., (*b*) average of the highest, and (*c*) of the lowest readings in the twenty-four hours; (*d*) the highest, and (*e*) the lowest reading of the month. III. Direction of Wind: N. S. E. W. represent the directions indicated, or any point thereof, after which the number of days is given on which it blew from that quarter. IV. The rain of the month is stated in inches.

OCTOBER, 1876.—The morning temperatures ranged from 54° F. to 70°; midday, from 62° to 80°; and evening, 53° to 72°; the average of the night temperature for the half-month, 52·2; and the average rainfall for the same period was 3·8 inches. No wind record was kept.

NOVEMBER, 1876.—I. (*a*) 29·95, (*b*) 30·44, (*c*) 29·44. This last reading is the record of the 12th, and was accompanied by a terrific gale from the south-west, which wrought much damage both on land, on the river, and at sea. Several residents, who were not unfamiliar with earthquake shocks, averred that they felt a distinct tremor of the earth about 4 A.M., at which time the barometer registered 29 inches. In the Bay of Biscay on the same morning the lowest point reached by the mercury was 28·25, as I was, I believe accurately, informed by the captain of a Glasgow steamer which arrived in the Tagus some days later. II. (*a*) 57°·59, (*b*) 63°·9 (*c*) 54°·09 (*d*) 70°·98 (*e*) 46°. III. N. 7 days, S. 10, E. 7, W. 2; of 3 days no record. IV. Rain, 10 inches, which fell on 17 days. This was one of the most rainy Novembers for many years. The rainfall of the year 1874 was 17·2, and that of 1875, 18·3 inches. The total amounts for the months of November from 1873-1875 was 5·5 inches. The mean of this month for the last twenty years is 4·3 inches. Most destructive floods occurred during the month.

DECEMBER, 1876.—I. (*a*) 29·96, (*b*) 30·3, (*c*) 29·4 inches. II. (*a*) 54°·8, (*b*) 59°·5, (*c*) 51°·6, (*d*) 65°·5, (*e*) 44°·2. III. N. 5, S. 18, E. 0, W. 5, calm 3 days. IV. Rain 19·19 inches on 28 days, greatest fall on 1 day (6th) 3·2 inches, and least ·003.

JANUARY, 1877.—I. (*a*) 30·18, (*b*) 30·58, (*c*) 29·54 inches. II. (*a*) 52°·93, (*b*) 58°·96, (*c*) 50°·8, (*d*) 65°·66, (*e*) 44°. III. N. 17, S. 9, E. 0, W. 3, calm 2 days. IV. Rain which fell on 14 days, 7·007 inches; from 1st to 10th, 6·669 inches.

FEBRUARY, 1877.—I. (*a*) 30·35, (*b*) 30·54, (*c*) 29·92 inches. II. (*a*) 52°, (*b*) 56°·64, (*c*) 48°·29, (*d*) 67°·38, (*e*) 42°·9 (the lowest temperature of the seven months). III. N. 25, S. 1, E. 1, W. 0, calm 1 day. IV. Rain, which fell on 2 days, 1·28 inches.

MARCH, 1877.—I. (*a*) 29·94, (*b*) 30·39, (*c*) 29·36 inches. II. (*a*) 47°·63, (*b*) 59°·34, (*c*) 48°·5, (*d*) 71°·6, (*e*) 43°·3. III. N. 10, S. 9, E. 1, W. 6, calm 1, of 4 days no record. IV. Rain, which fell on 13 days, 2·5 inches.

APRIL, 1877.—I. (*a*) 29·92 (*b*) 30·13, (*c*) 29·60 inches. II. (*a*) 65°·5 (*b*) 62°·9, (*c*) 51°·9, (*d*) 71°·7, (*e*) 48°·2. III. N. 8, S. 8, E. 1, W. 11, 2 days unrecorded. IV. Rain in 17 days, 6·5.

I would draw the attention of those threatened with bronchial or pulmonary complaints to this locality as a winter and spring refuge. The site of the city of Lisbon is finely chosen, facing almost due south, and the position of the principal part of the town in which the chief hotels are, is nearly sheltered from the northerly and easterly winds by surrounding heights. It is of easy access from England—3½ days, and sometimes fewer, from Southampton by a royal mail steamer. Fires are rarely to be seen in a Portuguese sitting-room, and during the seven months of my sojourn there it was necessary only once or twice to have one in our room for an invalid's sake. I had an opportunity of seeing many sufferers both *en route* for, and again returning to England from, Madeira. Some of them complained much of the weather experienced there, and said how they wished they had remained in Lisbon, where the climate seemed equally to suit them, and where they should have had at least more comforts, more cheerful society, and more varied means for killing the Enemy—time. HENRY O. FORBES

Fertilisation of Flowers by Insects

IN my last article on Alpine Gentiana species, I supposed that the chief, if not the only fertiliser of *G. bavarica* and *verna* might be *Macroglossa stellatarum* with its proboscis of 25-28 mm.

length. Yesterday, near the Albula pass, I was happy enough to confirm this supposition by direct observation. Altogether I saw five specimens of *Macroglossa stellatarum* at work, one on *Gentiana bavarica* and *verna*, three on *Primula integrifolia*, and one on *Viola calcarata*, each of them in a few minutes fertilising some hundreds of flowers. For instance, the last of my five Macroglossa specimens, which I observed with the watch in my hand, in less than four minutes visited 108, and in other 6¾ minutes 194 flowers of *V. calcarata*.

As an illustration to what I have said in a former article on alpine orchids generally being adapted to cross-fertilisation by Lepidoptera, I may mention that near my present domicile there grow nine species of orchids, eight of which (*Nigritella Angustifolia*, *Platanthera bifolia*, *Grymnadenia conopsea*, *odoratissima*, *albida*, *Habenaria viridis*, *Orchis globosa*, and *ustulata*) are adapted to cross-fertilisation by Lepidoptera, whilst only a single one (*Orchis latifolia*) is adapted to cross-fertilisation by other insects. Hermann Müller

Wissenstein, Albula Valley, Switzerland, July 23

Local Museums

I have read with very great interest both the letters and articles which have lately appeared in Nature on the subject of local museums. The suggestions offered by your various correspondents are in every way admirable, and my only excuse for adding my own name to the number is because I think that although a great deal has been said on the matter next to nothing has actually been done. If local museums are to be established amongst us as a means of promoting advancement in education the sooner the matter is taken in hand by those most competent to deal with it the better.

What I would strongly advocate is that a society be formed in London for the promotion of local museums. If Prof. Boyd Dawkins, and any others possessing the requisite attainments for taking the matter in hand, would form an association of this kind, I, for one, and doubtless many others of your readers, would gladly subscribe and co-operate for the realisation of the scheme. J. Romilly Allen

34, James Street, S.W.

Proposed New Museum

Now that the new Natural History Museum is approaching completion, will you allow me to call attention to a need which has probably been felt by others beside myself, and which we may hope will be met in the new institution? This is a museum or collection of varieties of plants and animals produced by domestication. I need not enlarge upon the value of such a collection to the student of biology. The revolution in the philosophy of biology created by Mr. Darwin was founded upon an examination of such varieties, and I have small doubt that my plea will be seconded by botanists and zoologists who will speak with much greater authority than I can.

I base my own request upon another ground, and one which touches very closely the science I am chiefly conversant with, namely, ethnology. Rutimeyer in Switzerland, Busk, Dawkins, and others in England, Brandt in Russia, and others elsewhere, have shown how invaluable the evidence furnished by varieties of domestic animals is for elucidating the earlier history of our race. Yet there is no collection known to me anywhere except the one made by Mr. Darwin himself, illustrating the subject, and if one wishes to examine the various breed of cattle, sheep, dogs, or pigs, of vegetables and fruits, &c., which have become localised in various parts of the world, as the companions of man, one is entirely at a loss for materials in an accessible form.

May we hope that the very efficient staff of the National Museum will see their way to setting apart one room at least in which the variation of animals and plants under domestication may be shown, and the glorious discoveries of the greatest biologist of modern times may be fitly illustrated in the National Museum of the country whose science he has so adorned.

Henry H. Howorth

Adaptation of Plant Structure

I have lately observed a curious adaptation of plant-structure which has not, to my knowledge, been recorded in books, and which may be interesting to your botanical readers.

There is in the Himalayas an Arum bearing a remarkable resemblance to a cobra with its hood raised, which is well known to natives and many Europeans by the name of the "cobra plant." Standing immediately behind and above the spathe is a large ternate leaf, the two lower leaflets of which, at an early stage of growth, enfold the spathe and spadix, and subsequently stand in front of and partially conceal them from view. When, however, the anthers or the stigmas, as the case may be (for the plant is diœcious), are mature, the lower halves of these lateral leaflets fold close up over their upper halves, thus leaving the whole of the spathe conspicuously exposed to the notice of passing insects. I inclose a rough sketch made from a living plant. It will be observed that if the lateral leaflets were extended they would conceal the flower from insects flying at a higher level than the mouth of the spathe. It is therefore an advantage to the plant that they should assume this abnormal position.

I may add that the resemblance of this arum to the cobra snake is very close, and cannot easily be accounted for. The diamond-shaped markings of the cobra's head are counterfeited on the spathe, as also are the lines on the neck; while the tongue-like prolongation of the spadix and of the mid-rib of the spathe serve to complete the resemblance of the plant to a living animal. As the cobra is almost unknown in the localities where this arum grows, it seems that the strange mimicry can be nothing more than accidental coincidence, even if any theory of advantage to the plant therefrom could be devised. But the "counterfeit presentment" is so striking that I am convinced any person who unexpectedly saw this plant "rearing its horrid head" above the rank herbage of an Indian jungle would start back with horror. Henry Collett

Nagkunda, near Simla, June 15

Rattle-snakes in Wet Weather

I have had much pleasure in reading Mr. Frank Buckland's edition of "White's Selborne." Among the notes on page 448, Mr. Buckland says:—"I know that rattle-snakes cannot play up their rattles in wet weather. The horn of the rattle becomes more or less saturated with water, and no sound can then be produced from it. By placing a rattle in a glass of water, and letting it soak a while, I find this is the case."

Mr. Buckland's dried rattle has led him into an error. The live rattle-snake can "play up" his rattle in the very wettest of wet weather. I have taken them alive on two occasions in the midst of a heavy rain, and I could discover no difference in their rattling powers. It is true, however, that rattle-snakes are seldom found in low moist places; they frequent, by preference, high and dry ground.

During the year 1873 I kept in my room a rattle-snake for eight months. In this time I came to know that "Rattler," so I called him, could "play up" several different notes indicative of anger, of pleasure, and of loneliness.

I think that it will be found, upon proper examination, that the fangs of the rattle-snake are shed just as the teeth of other animals. Hunter Nicholson

East Tenn. University, Knoxville, Tenn., U.S.A.

Meteors

At 9.48 last evening I saw a bright meteor pass downwards towards α Aquarius, where it disappeared. It emitted a bluish light, and although the moon was up, it shone for a few seconds with the brilliancy of Venus. A second smaller meteor passed upwards towards the zenith about 10.5. In both cases the vanishing point was near Delphinus. W. Ainslie Hollis

Brighton, July 30

OUR ASTRONOMICAL COLUMN

The Herschelian Companion of Aldebaran.—In a communication lately received from M. Camille Flammarion, it is endeavoured to show that the change of relative situation of the small star with respect to Aldebaran, is not accounted for by the proper motion of the latter, as was stated by Struve ("Positiones Mediæ," p. ccxxvi.), but that it is necessary to admit the existence of a very appreciable proper motion of the companion, which would be the first instance of the kind in so small a star. M. Flammarion collects the various published measures and adds to them measures made by Mr. Gledhill in 1876, and by himself in 1877. Sir W. Herschel's measures may be set aside at once as not sufficiently exact for a discussion of a moderate proper motion; his angle in 1781 is the result of a single measure, and his distances, as is well known, are in defect, when they exceed a minute. Taking then Struve's epoch, 1836·06, as the earliest reliable datum we possess, and bringing it up to Dembowski's epoch, 1863·37, with Leverrier's proper motion for Aldebaran, we find no greater difference than may be accounted for by unavoidable error of observation, so that Struve's inference on comparing the Pulkowa measures, in 1851 with the Dorpat measures of 1836, "itaque comes motus non est particeps, sed in cœlo quiesca videtur," and Dembowski's conclusion when speaking of this object and of λ Aurigæ," "Les différences s'accordent assez bien avec les mouvements propres des deux principales," are thus supported. The angle, however, should now be less than is assigned by Mr. Gledhill and M. Flammarion from their own measures (35°·5), and further careful measures will be desirable to clear up a possible question of personal equation. If we reduce Struve's angle for 1836 with Leverrier's proper motion of Aldebaran to the present year, we find an angle nearer 32°·5 than 35°·5.

THE THIRD COMET OF 1759.—This comet was not observed until January, 1760, but appears in our catalogues as comet 1759 III., from the circumstance of the perihelion passage having occurred in December of that year. It approached very near the earth, but was not a conspicuous object more than a few days. There are several references to the comet in the *Annual Register* for 1760, where we learn that it was "discovered and astronomically observed by Mr. Dunn at his Academy at Chelsea," who had determined the positions of Halley's comet on every evening during the first week of May previous. Pingré states that the sky, having been constantly overcast at Paris for several days, all the astronomers of that capital, including Cassini de Thury, Maraldi, Lacaille, and Messier, detected the comet on the evening of January 8; Dunn is credited with having found it on January 1. It was seen at Lisbon on January 7. For the purpose of these remarks we shall adopt the elements of Lacaille, in deducing the apparent places of the comet.

There is no reason why the comet should not have been found on January 1, if atmospherical conditions had been favourable, but it must have been on the morning, not on the evening of that day. In fact, the comet would rise soon after 1 o'clock A.M., in London, and would be upon the meridian a few minutes before six at an altitude of more than 23°. It is, however, distinctly stated in the *Annual Register* that Dunn discovered the comet in the evening, that "it appeared to the naked eye like Jupiter or Venus through a thick fog, and made a near appulse to the star in Orion's right knee, and moved more than four degrees of the heavens in four hours of time." This can only refer to the evening of January 8. The elements give the following positions:—

	d. h.	R.A. ° ′	N.P.D. ° ′	Distance from earth.
January	8 8	... 88 59	... 99 55	... 0·0734
,,	8 12	... 84 17	... 98 34	... 0·0760

There was, therefore a motion of upwards of four degrees in as many hours, and soon after midnight the comet would not be more than $1\frac{1}{4}°$ from Orion's right knee, or κ Orionis. It is therefore pretty certain that Dunn did not precede other observers by a week, as might at first sight appear from the statement in the *Register*. Clouded skies had evidently prevailed in Western Europe for some days, and the comet was detected on the same evening, January 8, on the heavens clearing, in England, France, and Holland.

The rapid course of this retrograde comet will be apparent from the following positions, calculated for Greenwich midnight.

	R.A. ° ′	N.P.D. ° ′	Distance.
Jan. 5	... 165 36	... 109 22	... 0·112
,, 6	... 147 11	... 110 3	... 0·083
,, 7	... 116 27	... 106 58	... 0·068
,, 8	... 84 17	... 98 34	... 0·076
,, 9	... 64 9	... 91 30	... 0·101
,, 13	... 39 22	... 82 33	... 0·248

Between January 7 and 8 the comet passed over $32\frac{1}{2}°$ in arc of great circle, and was nearest to the earth soon after midnight on the former date. On January 9 in the evening Dunn says the comet passed near μ and ν in Eridanus, and we find from Lacaille's elements that at 5h. 39m. P.M. it would have the same right ascension as μ, with only 40′ greater declination. So far as regards position the comet might have been observed as early as the day of perihelion passage, December 16, when it was in R.A. 199° and N.P.D. 103°, rising in London at 2h. 45m. A.M.; but the intensity of light was only $\frac{1}{120}$th of that on the evening of January 8, when it was generally discovered. It is rather unfortunate that it was not observed over a longer period, since it appears certain that in its approach to perihelion it must have passed very near to the planet Jupiter, and we might expect a sensible deviation from the parabola. On November 7, 1758, Lacaille's orbit would give the comet's distance from Jupiter less than 0·05.

METEOROLOGICAL NOTES

SUN-SPOTS AND RAINFALL OF CALCUTTA.—Mr. E. Douglas Archibald, of Naini Tal, has written an interesting letter to *The Englishman*, the leading Calcutta newspaper, in which he shows from the observations made from 1837 to 1876, that the winter rainfall (Nov. to April inclusive) of Calcutta is marked by a distinct periodicity, the maximum rainfall occurring during the years of minimum sunspots, and the minimum rainfall during the years of maximum sun-spots. The following are the figures for the years of the sun-spot cycle beginning with the year of minimum sun-spots:—

		Average Rainfall.
1st and 2nd year of cycle		... 6·44 inches
3rd and 4th	,,	... 5·93 ,,
5th and 6th	,,	... 4·44 ,,
7th and 8th	,,	... 5·03 ,,
9th and 10th	,,	... 6·15 ,,
11th	,,	... 8·49 ,,

the average rainfall for the forty years being 5·41 inches. Mr. Archibald is of opinion that this peculiarity, which is the reverse of what obtains as regards the rainfall of the whole year, in its relation to sunspots, will be found not to occur much farther south than Calcutta, and that it will be more decidedly marked over the region farther to the north lying more immediately under the great range of the Himalayas. The point is one of very considerable interest and deserves the fullest investigation, since, if the supposition proves to be correct, it will doubtless lead to a more exact method of examining the rainfall in its relation to sun-spots. It may be remarked that the winter rainfall at Sydney (in the southern hemisphere) from 1840 to 1876, which is situated within the latitudes indicated by Mr. Archibald, exhibits the same peculiarity as that of Calcutta in its relation to the sun-spot period.

WINDS OF THE SOUTH ATLANTIC.—M. Brault announces the publication by the French Marine of a series of new meteorological charts giving the direction and force of the winds of the South Atlantic for each of the four seasons, the charts being similar to those published by M. Brault about two years ago on the winds of the North Atlantic. The new charts contain the results of 189,573 observations of the wind. The general movement of the winds in summer over this portion of the globe resembles an immense whirl whose centre is about 30°–35° lat. S., and 10°–20° long. W. The whirling movement is in a direction contrary to that of the hands of a watch, being thus opposite to the general circulation of the atmosphere over the North Atlantic in summer. Out from this centre winds blow in all directions, the more important being the south-east trades, which are deflected to south and south-south-west off the coast of Africa, and to east-south-east and east on approaching America; then in succession north-east, north, and north-west winds on advancing southward along the coast of America, merging finally in the westerly winds which blow across the Atlantic from Cape Horn to the Cape of Good Hope. Looking both at the direction and force of the winds, M. Brault concludes that the results establish beyond a doubt the fact that, contrary to views entertained up to a comparatively recent date, there does not exist any tropical zone stretching across the South Atlantic, characterised by the prevalence of calms and light variable breezes. These results are in entire accordance with recent researches into the atmospheric movements over this region, and are of peculiar interest when viewed in connection with the distribution of atmospheric pressure and its variation with season over South America, the South Atlantic, and South Africa.

CLIMATE OF KOSSEIR, ON THE RED SEA.—In the last number of the *Journal* of the Austrian Meteorological Society, p. 225, there is an interesting article on the climate of Kosseir, on the Red Sea, based on a year's observations by Dr. Klunzinger during 1872-73. The interest of the climate of this region lies in its extreme character in certain directions, and the regularity of occurrence of its changes from season to season. The mean atmospheric pressure is 30·020 inches, rising to the maximum 30·213 inches in January, and falling to the minimum 29·863 inches in July, showing thus a variation of 0·350 inches in the monthly means. The mean temperature is 76°·3, the warmest month, August, being 84°·9, and the coldest, January, 64°·9. There is little cloud in any season, and in summer the skies are constantly all but cloudless. A prominent feature of the climate is its dryness, the mean relative humidity for the year being only 56, falling in June to 51, and rising to 62 in November. This great dryness is due to the winds, which are northerly and north-westerly during the whole year, the only change being from about north-north-west in summer to north-west in winter. Occasionally, however, when easterly winds set in, the air becomes so saturated that everything is wetted with the vapour with which it is heavily charged. On June 4, 1873, a "Samum" commenced (north-north-west, force 7), the horizon having a grey troubled appearance, the sky cloudless, and the air hot and dry; it continued till the 6th, and it was during this strong dry wind that the highest temperature, 93°·9, was observed.

DROUGHT IN CANADA.—An unusual drought has prevailed in Canada during the past spring. Little rain having fallen for ten weeks, the waters of the Ottawa and St. Maurice, two of the principal lumbering rivers, have been reduced to their summer level, having never before been so low at this season. A serious consequence of this state of matters is, that very large quantities of the finest timber of the dominion must remain in the woods till next year.

EARLY ALLUSIONS TO THE MAGNETIC NEEDLE

AT recent meetings of the Literary and Philosophical Society of Manchester interesting contributions to the subject were made. In a paper by Mr. H. Grimshaw he refers to such an allusion in a work entitled, "An Apologie of the Power and Providence of God in the Government of the World; or, An Examination and Censure of the Common Error touching Nature's Perpetual and Universal Decay: Divided into Four Books." The author is one "G. H.," D.D. (Doctor of Divinity), and the work is printed at Oxford by John Litchfield and William Turner, "Printers to the famous University," Anno Domini 1627, being therefore exactly 250 years old.

The third book of the four into which the work is divided treats of "The pretended decay of mankind in regard and duration, of strength and stature, of arts and wits." The tenth chapter of this third book is said to be "Touching diverse artificiall workes and usefull inventions, at leastwise matchable with those of the ancients, namely and chiefly the invention of Printing, Gunnes, and the Sea-Card or Mariners Compasse." This tenth chapter again, for such is the orderly division of the subjects, is subdivided into four sections, and the fourth of these is headed "Of the use and invention of the Mariners Compasse or sea-card, as also of another excellent invention sayd to be lately found out upon the Load-stone, together with the conclusion of this comparison touching Arts and Wits, with a saying of Bodius, and another very notable one of Lactantius."

It is in the account of this "excellent invention sayd to be lately found out upon the loadstone" that a curious prevision or dream, so to speak, of the application of electricity as a means of communication occurs, and there is small wonder that the old philosopher called it as he does further on, "an excellent and secret conclusion upon the stone," for, whilst perusing his description, one can hardly imagine that the writer has not in his mind's eye one of our most modern telegraphic instruments. The paragraph is as follows:—

"Another excellent and secret conclusion upon this stone, pretended to be found out in these latter times, is, that by touching two needles with the same stone, they being severally set so as they may turne upon two round tables, having on their borders, the *Alphabet* within circlewise, if two friends agreeing upon the time, the one in Paris, the other in London (having each of them their table thus equally fitted) be disposed upon certayne dayes and at certaine houres to conferre, it is to bee done by turning the needle in one of the tables to the *Alphabet*, and the other, by *Sympathie* will turn itself in the same manner in the other table though never so farre distant: which conclusion if infallibly true, may likewise proove of good and great consequence; howsoever, I will set it down as I find it described by *Famianus Strada* in imitation of the stile and vaine of *Lucretius*."

Magnesi genus est lapidis mirabile, &c., &c.

Then follows the extract in Latin, with the English translation in verse attached.

It will be acknowledged by any one familiar with the instrument, that the dial telegraph of Cooke and Wheatstone, invented subsequently to their first upright needle form, most curiously carries out the ideal description of this old author, and it will be seen that the date at which his work is written was nearly 200 years prior to the first attempt made to communicate at a distance by means of magnetic needles.

Prof. Stanley Jevons, in a subsequent paper, stated that ten years ago he spent some trouble in investigating this curious anticipation of the telegraph, but only published the results in the form of a brief anonymous article in a weekly newspaper. This curious subject, Mr. Jevons thinks, has not received the attention which it seems to deserve, but it was not wholly unknown. The Abbé Moigno, in his "Traité de Télégraphie Electrique" (Paris, 1852), alludes to what he calls this "Charmant rêve, ou operation necromancienne," and he points out that Addison had quoted the remarkable verses of Famianus Strada in the *Spectator*, No. 241. Addison speaks of "a chimerical correspondence between two friends by the help of a loadstone." Strada's remarkable lines are also quoted and translated in Mr. George Dodd's account of "Railways, Steamers, and Telegraphs: a Glance at their Recent Progress and Present State" (Chambers, 1867). Mr. Jevons found allusions to a magnetic telegraph running through many scientific, or quasi-scientific, works of the sixteenth and seventeenth centuries. Sir Thomas Browne, in his "Pseudodoxica Epidemica," says:—"The conceit is excellent, and, if the effect would follow, somewhat divine;" and he speaks of it as a conceit "whispered thorow the world with some attention, credulous and vulgar auditors readily believing it, and more judicious and distinctive heads not altogether rejecting it." Sir Thomas, it would seem, submitted the matter to experiment, but found that though the needles were separated but half a span, when one was moved "the other would stand like Hercules' pillars"

Joseph Glanvill, in his *Scepsis Scientifica* (1665), discusses the objections of Sir Thomas Browne, and concludes that "there are some hints in natural operation that give us probability that it is feasible." How can we read without wonder these words written by Glanvill more than 200 years ago? "Though this pretty contrivance possibly may not yet answer the expectation of inquisitive experiment, yet 'tis no despicable item that by some other such way of magnetick efficiency it may hereafter with success be attempted, when magical history shall be enlarged by riper inspections; and 'tis not unlikely but that present discoveries might be improved to the performance." It is evident that Glanvill treats the matter quite seriously as a scientific possibility. The Marquis of Worcester probably refers to the magnetic telegraph when he speaks of "intelligence at a distance communicative, and not limited to distance, nor by it the time prolonged." (Dirck's "Life of Worcester," p. 357.)

Mr. Jevons tried to trace these notions to the first inventor, but, as might be expected, without much success. Strada attributed the invention to the celebrated Cardinal Bembo, the secretary of Leo X., but as Bembo (who died in 1547) was a historian and literary character, it is hardly likely that he would originate a scientific conception of the sort. The earliest books in which Mr. Jevons found allusions to a magnetic telegraph is the *Natural Magic* of Baptista Porta, published in 1589. In the seventh book he describes the "wonders of the magnet," saying in the preface, "I do not fear that with a long absent friend, even though he be confined by prison walls, we can communicate what we wish by means of two compass needles circumscribed with an alphabet." In the eighteenth chapter of the same book, he describes the experiment of putting a magnet under a table and moving thereby a needle above the table. This experiment, as Porta remarks, was known to St. Augustine, and an exact description will be found in his "De Civitate Dei," a work believed to have been begun A D. 413. It seems probable that this passage in St. Augustine suggested the notion either to Porta, Bembo, or some early Italian writer, and that thus it came to be, as Sir Thomas Browne says, "whispered thorow the world."

Mr. William E. A. Axon refers to the passage in Strada in which he supposes the loadstone to have such virtue that "if two needles be touched with it, and then balanced on separate pivots, and the one be turned in a particular direction, the other will sympathetically move parallel to it. He then directs each of these needles to be poised and mounted on a dial having the letters of the alphabet arranged round it. Accordingly, if one person has one of the dials and another the other, by a little pre-arrangement as to details, a correspondence can be maintained between them at any distance by simply pointing the needles to the letters of the required words." The date of the first edition of Hakewill's "Apologie or Declaration of the Power and Providence of God in the Government of the World" is 1627; but the work of Strada's from which he quotes was published ten years earlier. Famianus Strada was born at Rome in 1572, and his "Prolusiones Academicæ et Paradigmata Eloquentiæ" appeared at Rome in 1617. Several editions of his "Prolusiones" have been printed in this country. The particular poem referring to the loadstone has been translated into English, and is printed in "The Student; or, Oxford and Cambridge Miscellany," 1750. The passage is referred to by Addison in a paper in the *Spectator*, No. 241, and in the *Guardian*, No. 119. In the former of these he adds: "In the meanwhile, if ever this invention should be revived or put in practice, I would propose that upon the lover's dial-plate there should be written not only the four-and-twenty letters, but several entire words which have always a place in passionate epistles: as flames; darts; die; language; absence; Cupid; heart; eyes; hang; drown; and the like. This would very much abridge the lover's pains in this way of writing a letter, as it would enable him to express the most useful and significant words with a single touch of the needle."

The subject is an interesting one, and seems to us well worth being followed out.

EVOLUTION OF NERVES AND NERVO-SYSTEMS[1]

II.

As the Medusæ are thus the lowest animals in which a nervous system has yet been discovered, we have in them the animals upon which we may experiment with the best hope of being able to elucidate all questions concerning the origin and endowments of primitive nervous tissues. I have therefore spent much time and labour, both last year and this year, in cultivating this field of inquiry; and as it is a field whose ground had never before been broken, and whose fertility has proved itself prodigious, it is not surprising that I should have reaped a rich harvest of results. So far as these results have any bearing on the general theory of evolution, their character is uniformly such as that theory would lead us to expect. For if I had two hours at my disposal instead of one, I might mention a number of facts which tend to show, in a very striking manner, that the primitive nervo-muscular tissues of the Medusæ, in respect of their physiological properties, present unmistakable affinities, on the one hand with the excitable tissues of certain plants, and on the other hand with the nervo-muscular tissues of higher animals. But not having time to go into this matter, I shall on the present occasion restrict myself to describing such of my results as tend to substantiate Mr. Herbert Spencer's theory concerning the mode in which nerves and nervo-systems have been evolved. And I adopt this course, not only because I feel that any facts bearing on so important a subject cannot fail to be of interest to all intelligent persons, but also because I think that this is a place best suited for publishing the somewhat speculative inferences which I have drawn from my facts. If these inferences are correct, their philosophical as well as their scientific influence will be great and far-reaching; but until they shall have been more completely verified I have not thought it desirable to adduce them in my communications to the Royal Society. Referring, therefore, those among you who may be interested in the research as a whole to the *Philosophical Transactions*, I will now invite your attention to a connected interpretation of some of the facts that it has yielded—an interpretation which I here publish for the first time.

To begin, then, with this diagram, Fig. 2. It represents *Aurelia aurita*, with its polypite cut off at the base, and the under, or concave, surface of the bell exposed to view. The bell, when fully expanded as here represented, is about the size of a soup-plate, and in it all the ganglia of the margin are collected into the eight marginal bodies; so that on cutting out these eight marginal bodies total paralysis of the bell ensues. But although the bell is thus paralysed as to its *spontaneous* movements, it continues responsive to stimulation; for every time you prick or electrify any part of the contractile sheet, a wave of contraction starts from the point which you stimulate, and spreads from that point in all directions as from a centre. Such contractile waves, at ordinary temperatures, travel at about the rate of a foot and a half per second; and the important question with regard to them which we shall have to consider to-night is this—Are they merely of the nature of muscle-waves, such as we see in undifferentiated protoplasm, or do they require the presence of rudimentary nerve-fibres to convey them—the *stimulus* wave in the rudimentary *nerve*-fibres progressively causing, as it advances, the *contractile* wave in the rudimentary *muscle*-fibres?

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[1] Abstract of a Lecture delivered at the Royal Institution on Friday evening, May 25, 1877. By George J. Romanes, M.A., F.L.S., &c. Continued from p. 233.

Now the great argument in favour of these contractile waves being muscle-waves, and nothing more, is simply this—that the contractile tissue is able to endure immensely severe forms of section without the contractile waves in it becoming blocked. For instance, when the bell of *Aurelia* is cut as here represented, Fig. 3, and any part of the circle is stimulated, a contractile wave radiates from the point of stimulation just as it did before the cuts were introduced, notwithstanding the wave has now to zig-zag round and round the ends of the overlapping cuts. Similarly, if instead of employing artificial stimulation, a single ganglion (*g*) be left *in situ* while all the other seven are removed, contractile waves will radiate in rhythmical succession from the single remaining ganglion, and course all the way round the disc. Now this experiment seems to prove that the contractile waves depend for their passage, not on the conductile function of any primitive nervous net-work, but on the protoplasmic qualities of the primitive muscular tissue. The experiment seems to prove this, because so severe a form of section would seem of necessity to destroy the functional continuity of anything resembling such a nervous net-work as we observe in higher animals.

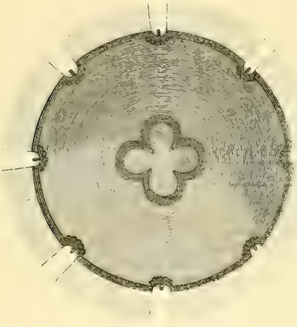
Fig. 2.

Here, again, Fig. 4, is another form of section. Seven marginal bodies having been removed as before, the eighth one was made the point of origin of a circumferential section, which was then carried round and round the disc in the form of a continuous spiral—the result, of course, being this long riband-shaped strip of tissue with the ganglion (*g*) at one end, and the remainder of the swimming-bell at the other. Well, as before, the contractile waves always originated at the ganglion; but now they had to course all the way along the strip until they arrived at its other extremity, and as each wave arrived at that extremity it delivered its influence into the remainder of the swimming-bell, which thereupon contracted. Hence, from this mode of section as from the last one, the deduction certainly appears to be that the passage of the contractile waves cannot be dependent on the presence of a nervous plexus; for nothing could well be imagined as more destructive of the continuity of such a plexus than this spiral mode of section must be.

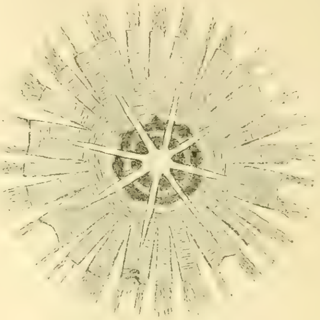
Fig. 3.

Nevertheless there is an important body of evidence to be adduced on the other side; but as I can only wait to state a few of the chief points, I shall confine my observations to the spiral mode of section. First of all, then, I have invariably found it to be the case that if this mode of section be carried on sufficiently far, a point is sooner or later sure to come at which the contractile waves cease to pass forward: they become blocked at that point. Moreover, the point at which such blocking of the waves takes place is extremely variable in different individuals of the same species. Sometimes the waves will become blocked when the strip is only an inch or less in length, while at other times they continue to pass freely from end to end of a strip that is only an inch broad and nearly a yard long; and between these two extremes there are all degrees of variation. Now if we suppose that the influence of the ganglion at the end of the strip is propagated as a mere muscle-wave along the strip, I cannot see

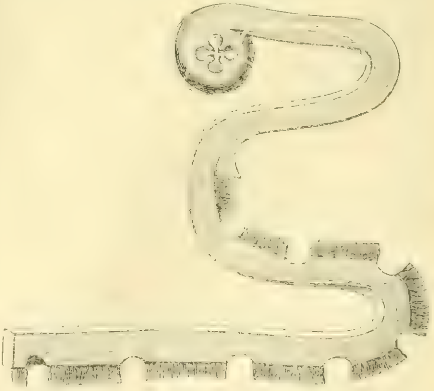
Fig. 4

why such a wave should ever become blocked at all, still less that the point at which it does become blocked should be so variable in different individuals of the same species. On the other hand, if we suppose the propagation of the ganglionic influence to be more or less dependent on the presence of a more or less integrated nerve-plexus, we encounter no difficulty; for on the general theory of evolution it is to be expected that if such fibres are present in such lowly animals they should not be constant as to position.

But there is a still stronger argument in favour of nerve-fibres, and it is this. At whatever point in a spiral strip which is being progressively elongated by section the blocking of the contractile wave takes place, such blocking is sure to take place completely and exclusively at that point. Now I cannot explain this invariable fact in any other way than by supposing that at that point the section has encountered a line of functionally differentiated tissue—has severed an incipient nerve.

Such, some of you may remember, was the state of the evidence when I last addressed you upon this subject. On the whole I provisionally adopted the view that all parts of the rudimentary muscular sheet of the Medusæ are pervaded by a plexus of rudimentary nerves, or "lines of discharge;" and I explained the fact of the tissues in some cases enduring such severe forms of section without suffering loss of their physiological continuity, by supposing that all the rudimentary nerve-fibres composing the plexus to be capable, in an extraordinarily high degree, of vicarious action. If you were to represent the hypothetical nervous plexus by a sheet of muslin, it is clear that however much you were to cut the disc of muslin with such radial or spiral sections as are represented in the diagrams, you could always trace the threads of the muslin with a needle round and round the disc without once interrupting the continuity of your tracing; for on coming to the end of a divided thread you could always double back on it and choose another thread which might be running in the required direction. And this is what I last year stated to be my opinion as to what took place in the fibres of the hypothetical nervous plexus;—whenever a stimulus wave arrives at a cut, I imagined it to double back and pass into the neighbouring lines of discharge, which I thus supposed to act vicariously for the divided line.

Such, then, when last I addressed you, was the standing of this question as to the character of these highly remarkable contractile waves. On the whole I decided in favour of a rudimentatary nervous plexus, notwithstanding the improbability that such a plexus should be capable of vicarious action in all its parts to so almost unlimited a degree.[1] I am glad to say that this decision has now been further justified by some additional observations which are of the first importance. For since my last lecture I have noticed the fact that reflex action takes place between the marginal ganglia of the Medusæ and all the contractile tissues of the animal. Thus, for instance, if you seize the polypite with a pair of forceps, the marginal ganglia almost immediately set the swimming-bell in violent motion—thereby showing that the stimulus must have coursed up the polypite to its point of insertion in the bell, and then down the sides of the bell to the ganglia, so causing them to discharge by reflex action. Again, suppose that seven of the eight ganglia have been removed from the margin of *Aurelia*, and that any part of the contractile disc is stimulated too gently to start a contractile wave from the point immediately stimulated, a contractile wave will nevertheless shortly afterwards start from the ganglion—thus showing that a *stimulus* wave must have passed through the contractile sheet to the ganglion, and so caused the ganglion to discharge. Indeed in many cases the passage of this stimulus wave admits of being actually *seen*. For it is a peculiarity of the numberless tentacles which fringe the margin of this Medusa, that they are more excitable than is the contractile tissue of the bell. Consequently a stimulus may be applied to the contractile tissue of the bell which is not strong enough to start a contractile wave in the bell-tissue itself, and is yet strong enough to start a contractile wave in the tentacles—one tentacle after another contracting in rapid succession until the wave of stimulation has passed all the way round the disc. The latter, of course, remains quite passive until the tentacular wave, or wave of stimulation, reaches one of the ganglia (or the single remaining ganglion, if the disc has been prepared by removing seven of the ganglia), when, after an interval of half a second for the period of latency, the ganglion is sure to discharge, and so to cause a general wave of contraction.

Now these facts prove, in a singularly beautiful manner—for this optical expression of the passage of a wave of stimulation is a sight as beautiful as it is unique—these facts, I say, conclusively prove that the whole contractile sheet of the bell presents not merely the protoplasmic qualities of excitability and contractility, but also the essentially nervous quality of conducting stimuli to a distance irrespective of the passage of a contractile wave. So I conclude there can be no longer any question that we have here to deal with a tissue already so far differentiated from primitive protoplasm, that the distinguishing function of nerve has become fully established.

THE NORWEGIAN ATLANTIC EXPLORING EXPEDITION

Tromsö, July 13, 1877

THE expedition met at the beginning with several unfavourable circumstances. In the last week of May Capt. Wille went out to Husö with the *Vöringen*, in order to determine the magnetical constants of the ship. After his arrival a flaw was discovered in the shaft, so that he went back to Bergen, where there was fortunately a new shaft lying ready. A few days later the ship was again at Husö, and was swung, not without some difficulty owing to rough weather. The *Absolute* magnetical observations on shore were secured by Capt. Wille. The necessary observations for compass error having been obtained, the *Vöringen* returned to Bergen, where the scientific staff was assembled. There was, however, something still wanting before we could put to sea. The accumulators used last year had got brittle, and new ones had been ordered from London in March, but they had not arrived in May, and in answer to a telegram Capt. Wille learnt that the order had been forgotten. The new accumulators kept us waiting in Bergen till June 11, when we sailed for Stavanger, and received them on the 13th, and we put to sea at once.

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[1] This antecedent improbability is not so overwhelming as it is at first sight apt to appear; for we must remember that in a peripheral nervous plexus as we meet with it in the higher animals—*i.e.*, in the fully evolved form of such a structure—each of the constituent nerve-fibres is provided with an insulating coat for the very purpose of preventing vicarious action among these fibres and the consequent confusion among the reflex mechanisms which such vicarious action would manifestly occasion. But because insulation of peripheral nerve-fibres is thus an obvious necessity in the case of a fully evolved nervous plexus, it by no means follows that any high degree of insulation should be required in the case of an incipient nervous plexus. On the contrary, any hypothesis as to the manner in which nerve-fibres first begin to be differentiated from protoplasm must suppose that the conductile function of the incipient nervous tracts precedes any structure, such as that of nerve-coats, whereby this function is strictly confined to particular tracts. The antecedent probability being thus in favour of the view that insulating structures are a product of later evolution than are the essential nervous structures which they insulate, it would clearly be very hazardous to draw any analogy between an incipient nervous plexus such as I suppose to be present in the Medusæ, and a fully-evolved peripheral plexus of any of the higher animals. A less hazardous analogy would be furnished by the fibres which occur in the *central* nervous system of the higher animals; for here it may be said, both *à priori* from Mr. Spencer's theory and *à posteriori* from histological indications, that the nerve-fibres occur in various degrees of differentiation. And that vicarious action is possible to some considerable extent through a bridge of the grey matter of the cord, has been shown by the double hemi-section experiments of Brown-Séquard. Moreover, the admirable experiments of Goltz would seem to indicate that vicarious action is also possible to a large extent among the ultimate elements of the brain. I may add that recent research has tended to suggest a novel interpretation of the way in which certain poisons, such as strychnia, act upon the cord; for whereas it has hitherto been supposed that the abnormal reflex excitability which these poisons engender is due to their exerting a stimulating influence on the cord, the researches in question have fairly well proved that the very reverse is true, viz., that the action of these poisons is to *depress* the vitality of the cord. For a number of facts go to prove that the abnormal reflex excitability is due to the impairment of some function which has been provisionally termed "resistance of the cord," a function which in health prevents the undue spread of a stimulus through the substance of the cord, and the impairment of which by the poison consequently admits of a stimulus spreading to an undue extent, so giving rise to the abnormal reflex excitability in question. As bearing on this subject, I may observe that while the action of strychnia on the Medusæ is the same as it is on the higher animals, viz., that of causing paroxysmal convulsions, it certainly seems to exercise a *depressing* influence on the tissues; for an extremely weak sea-water solution has the effect of blocking contractile waves in any part of a spiral strip that is submitted to its influence.—G. J. R.

Outside the coast we took a series of temperatures, which showed the minimum, not at bottom, but at a certain depth below the surface. The same phenomenon has lately been observed in all latitudes near the coast. I attribute it to the action of the winter cold on the sea.

Our first working station was in lat. 66° 8′·5, long. 3° 0′ E., which was reached on the morning of June 16. The depth here was 805 fathoms, the temperature at bottom, 29°·7. We now worked in even sections, running west-north-west and east-south-east perpendicular to the coast. The third section from lat. 67° 53′, long. 5° 12′ E. to the island of Trocuew having been completed, we went northwards into the West Fiord, where a series of temperatures was taken with Negretti and Zambra's deep-sea thermometer. Last year we could not use this instrument at sea because the slightest upward movement of the ship caused the thermometer to turn over before it had had sufficient time to accommodate itself to the temperature of the sea. This year it was fitted with a new turning apparatus devised by Capt. Wille, which proved satisfactory. In the outer part of the West Fiord the temperature on the surface was 45°·7; it decreased to 38°·8 in sixty fathoms; and from that point it rose to 41°·0 in 140 fathoms, ten fathoms above the bottom. The Casella-Miller thermometer of course registered from this depth the minimum 38°·8. The phenomenon here noticed is universal all along our coasts in the summer months; I discovered it for the first time in the West Fiord two years ago. The explanation seems to be this: In winter the air is generally cooler than the sea-surface, especially at the coast; the water is chilled from above, and the cooled layers being denser, sink down, and so the winter cold descends in the water; the temperature down to a certain depth increases with the depth. When spring and summer come, the air warms up the sea surface, and the surface layers getting warmer get lighter also, and have no tendency to sink. The temperature becomes highest at the surface, and decreases to a certain depth, below which the action of the winter cold still shows itself in a temperature increasing with the depth.

After dredging and trawling in the inner part of the West Fjord, we went to Boelö, where the expedition stayed a couple of days. On the 26th we arrived at Rösh, the outermost of the Loffoden Islands; there we stayed some days, strengthening the accumulators, cleaning the ship, taking magnetical and astronomical observations, and making excursions.

We left Rösh on the 28th at noon, and commenced our work on the sections further north, sounded, dredged, and trawled outside the Loffoden Islands till the 30th, when we went into the Hadsel Fjord, and anchored at Sortland in Westeraalen. The next week was spent in working outside Westeraalen. There the greatest depth for this year was reached, 1,710 fathoms in lat. 70°, long. 6° 15′ E. The Casella-Miller thermometer registered at the bottom a temperature of 28°·4 when corrected for instrumental error and for pressure, the lowest temperature hitherto found by our expedition. A series of temperature observations showed that the temperature at all depths decreased with the depth, and that 32° lay in about 580 fathoms. The next Sunday, July 8, found us in Tromsö.

The expedition has this summer been favoured with remarkably fine and quiet weather, which has allowed us to carry out all our operations according to our proposed plan. The number of sounding stations is already 101; last year's total was only 93. Seventeen serial temperatures have been obtained, and the dredge and trawl have been out on the bank in the *Umbellularia* region (one specimen has been caught), and in the deep *biloculina* clay, at the depth of 1,700 fathoms, animal life was rather scarce. The boundary line between the water above and below 32° at the bottom, lies between lat. 65° and the Arctic circle as far west as 5° 30′ E. A little north of the Arctic circle it has a curvature towards the coast, and farther north it lies only from five to ten geographical miles off the outer side of the islands of Loffoden and Westeraalen. On this northern part the edge of the bank is very steep, and the bottom falls very rapidly towards the deep part of the Arctic Ocean. Out at sea the isothermal surface of 32° lies at very different depths in different latitudes. In the channel between Faroe and Shetland, it lies in 300 fathoms, between Iceland and Norway it sinks to 400 fathoms, and between Jem Mengen and Norway we have found it in 580 fathoms. To the westward it rises, as we found last year, east of Iceland. How it behaves further north, off Spitzbergen, we expect to find next year. Near the coast, 32° always lies at a much higher level.

The *Umbellularia* region has been found extending as far down as 880 fathoms off Westeraalen, where the specimen found came up with the weights on the dredge rope. In several places off the coast we have found, besides Norwegian rocks, specimens of chalk and flint. Of deep-sea animals, some new species have been found. On the bank off Langenes (lat. 69°) we caught plenty of the same kind of fish as are caught at the bank fisheries on the "Storeggen," off the coast of Romsdal.

The expedition is now lying at Tromsö, refitting and taking in stores for further work. We intend first to work only two more sections north of Tromsö, and then call here to make all ready for the voyage to Jem Mengen. From that island the course will be westward till we reach ice-cold water, then southwards to a point midway between Jem Mengen and Iceland, and thence to Bodö, whence the expedition will return to Bergen.

Among the novelties used in our work this year I must mention a piezometer, kindly sent me by Mr. Buchanan, chemist of the *Challenger*. This instrument has registered the depth very well. A new atmometer of my own construction has been constantly in use, giving good results. Two such instruments have, under favourable circumstances (they cannot be used in rough weather), given almost identical results; the depth of sea-water evaporated in twenty-four hours is sometimes more than four millimetres. Meteorological observations have been made every hour when at sea. The chemist has got many samples of air from the sea-water, both at the surface and at the bottom. He has taken the specific gravity of the water and determined its amount of chlorine. He has also made several determinations of its amount of carbonic acid.

MR. FROUDE'S NEW DYNAMOMETER

MR. FROUDE, in solving the problem assigned to him by the Admiralty—of producing a dynamometer calculated to test the power delivered at the end of the screw-shaft by large-sized marine engines—has enabled us to utilise a new principle of great value among the "applications of science."

In the friction-brake dynamometer, as is well known, the power delivered to a revolving shaft is measured by the rate at which a definite weight is being virtually lifted, and the number of foot-pounds of work done per minute is the circumference of the drum at the effective radius at which the weight is lifted, multiplied by the weight and by the number of revolutions per minute. Simple as the arrangement is when employed on a small scale, it involves serious difficulties when greatly magnified, owing to the great amount of heat; and it was chiefly in order to escape this difficulty that Mr. Froude sought some fresh *modus operandi*, and ultimately felt his way to the arrangement to which we desire to draw attention.

Under this arrangement, the engine, in utilising its power, will still be virtually winding up a weight; but the weight, instead of being constant, will vary with the speed of rotation, much in the same way as the resistance of the propeller itself does; and thus the work performed by the engine under trial will more closely resemble its natural work.

The reaction, instead of arising from the continuous friction of two solid surfaces, will consist of a multitude of reactions supplied by the impact of a series of fluid jets or streams, which are maintained in a condition of intensified speed by a sort of turbine revolving within a casing filled with water, both being mounted on the end of the screw-shaft in place of the screw, the turbine revolving while the casing is held stationary; the jets being alternately dashed forward from projections in the turbine against counter-projections in the interior of the casing, tending to impress forward rotation on it, and in turn dashed back from the projections in the casing against those in the turbine, tending to resist its rotation. The important point is, that the speed of the jets is intensified by the reactions to which they are thus alternately subjected; and thus in virtue of this circumstance a total resistance of very great magnitude is maintained within a casing of comparatively very limited dimensions.

The nature of this arrangement will be gathered from the accompanying figures.

In Fig. 1, A A represents the screw-end of the screw shaft; B B shows in section what has been termed "the turbine;" it is a disc or circular plate, with a central boss, keyed to the screw-shaft in place of the screw, and revolving with the shaft. The disc is not flat throughout, its entire zone being shaped into a channel of semi-oval section, which sweeps round the whole circumference concentrically to the axis. To give definiteness to the conception, imagine that, to deal with an engine of 2,000 Ind. horse-power, making 90 revolutions per minute, the diameter of the turbine-disc to the outer border of the channel is five feet.

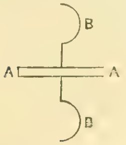


FIG. 1.

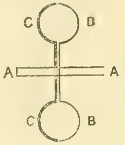


FIG. 2.

In Fig. 2, Fig. 1 is repeated, and what has been called "the casing" is added, being indicated by the letters C C, D D, the former representing the front and the latter the back. The face carries a channel, the counterpart of that carried by the disc, which it also fronts precisely, so that the two semi-oval channels in effect form one complete oval channel, though the two halves are in substance separated by an imaginary plane of division. The back of the casing embraces or includes the disc entirely, but without touching it; the casing is also provided with a boss, which is an easy fit over that of the disc or turbine, and thus the disc carried by the shaft can revolve within the casing without touching it, while the casing itself is stationary, and one half of the oval channel is running round while the other half is at rest.

Thus far the two half channels have been regarded as open and unobstructed; they are, however, in fact each closed or cut across by a series of fixed diaphragms, a single one of which is shown in Fig. 3, as in its place in the disc-channel. The diaphragms cut the channel, not perpendicularly, but obliquely, being semicircular in outline, so that when placed obliquely their circular edges fit the oval bottom of the channel, while their diameters span the major axis of the oval Fig. 4 shows one of the diaphragms seen end on or edgeways, as it would appear in an edgeways view of the turbine if this were transparent.

Each half channel has twelve of these diaphragms, and is thus divided into a series of cells, each of which, if viewed at right angles to one of the diaphragms, or what is the same thing, if shown in a section taken parallel to one of them, is semicircular in outline; and if thus viewed in connection with the cell which is for the moment opposite to it in the counterpart half channel, the two together make one complete cell with circular outline. Thus the whole oval channel may be regarded as a series of obliquely placed circular cells, and as the function of the turbine is to rotate while the casing remains at rest, one half of each cell is moving past the other half in such a manner that the moving half, if viewed from its stationary counterpart, would by reason of the oblique direction of the diaphragms which form the cell sides, appear to be advancing antagonistically towards it; indeed the motion virtually constitutes such an advance, because the bottom of each moving half cell is continually growing nearer to the bottom of the stationary half cell which it faces. The effectiveness of the combination to resist rotation will be seen to depend essentially on this quasi-antagonistic virtual approach of the moving to the stationary half cell.

The channel and the whole casing is filled with water, and the turbine is made to rotate as described. When the turbine is thus put in motion, the water contained in each of its half cells is urged outwards by centrifugal force; and in obeying this impulse it forces inwards the water contained in the stationary casing half cells, and thus a continuous current is established, outward in the turbine's half cells, inward in those of the casing.

Now the action of these cells on the water contained in them may be rendered more clear by the following illustration:—Suppose a person in a railway train moving at a certain velocity to hold a racket fixedly in his hand and a ball thrown to him strikes the racket; also that there is provided a series of walls beside the railway inclined at such an angle that the ball leaving the racket and striking one of the walls will rebound to the racket. Suppose, also, for the sake of simplicity, that the ball is perfectly rigid while the walls and racket are perfectly elastic. On striking the racket for the first time the ball will rebound with a velocity equal to double that of the train added to the original velocity of projection. In order to see this clearly we must look at what takes place in the racket. The ball meets the racket with a velocity equal to that of projection added to that of the train, the strings of the racket stretch to such an extent that their recoil would cause the ball to rebound with the sum of these velocities. At this instant suppose the train stopped, and we should then see the ball projected through the air with the sum of these velocities in consequence solely of the elastic reaction of the racket. But in the experiment we are supposing the train is not stopped but is at this instant capable of impressing on a ball at rest the full velocity of the train; this it does equally well on the ball, which we imagine, for the instant, resting against the strained racket. Thus we see that the ball will be projected through the air with a velocity equal to that of the train in addition to that with which it would have been impressed on it by the racket had the train been stopped at the moment indicated, or, in other words, its velocity of projection will, after one contact with the racket, have had added to it twice the velocity of the train. Each time the ball, rebounding from the walls in succession, meets the racket, an additional double train-velocity will be impressed on it. Another important point in this illustration must not be passed over. The action of the ball on the racket tends to retard the train, and that on the walls tends to push them forward in the direction of train as well as away from the line of railway.

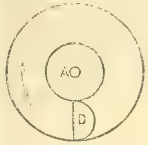


FIG. 3.

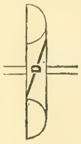
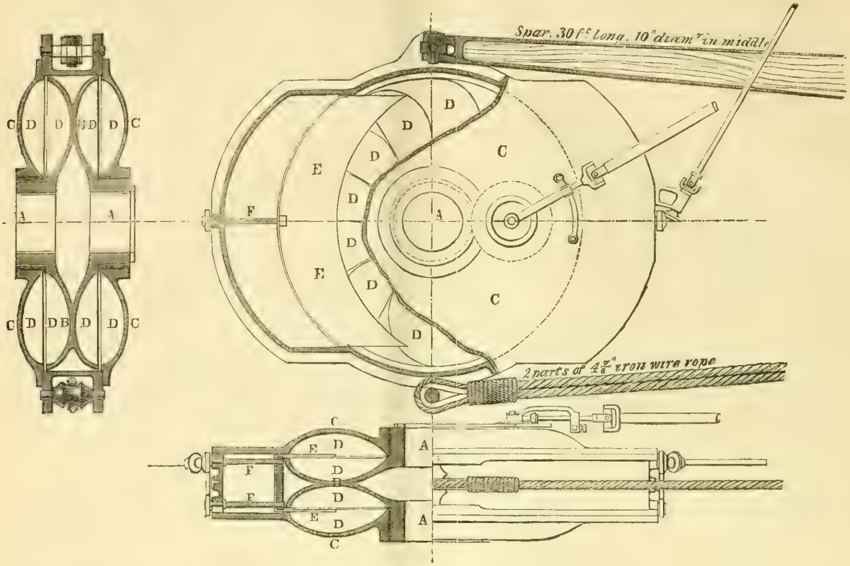


FIG. 4.

In an analogous manner the currents originated solely by centrifugal tendency, in being unceasingly reversed in direction and increased in velocity by the opposing cells, produce a resistance to the rotation of the turbine which is measured by the torsion it produces on the casing. This torsion is most conveniently measured by a spring balance attached to the end of a lever made fast to the casing.

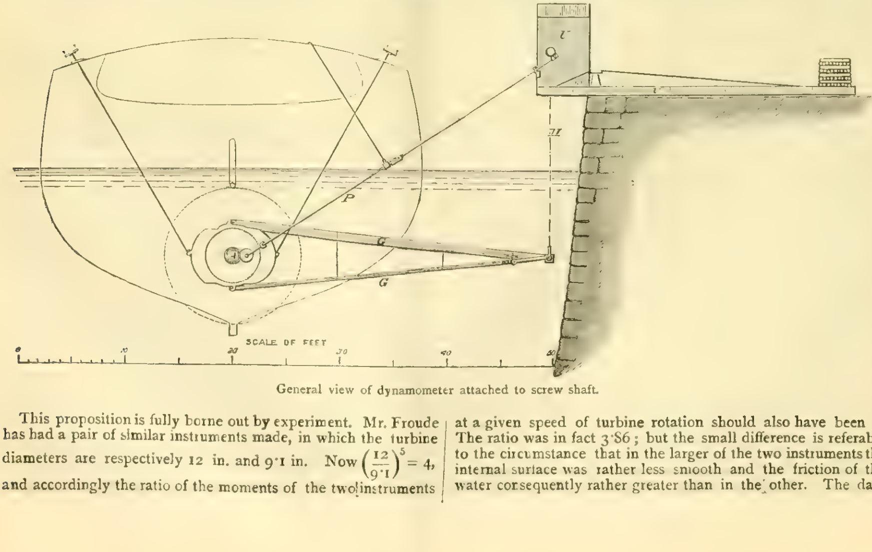
The manner in which the currents, when established, produce the dynamometric reaction, can be traced very easily. The explanation already given of the internal form of the cells which the current traverses, shows that the volume of water which constitutes it in each complete cell may be regarded as a circular plane or disc of water, rotating in its own plane between the diaphragms, which define the direction of the water disc and which are the boundaries of its thickness.

Having now traced the *modus operandi* by which the reaction is produced, it is necessary to show that (1) an adequate amount of total reaction can be produced by an instrument of conveniently limited dimensions; and that (2) an instrument of given dimensions is governable as regards its reactions, that is to say, is capable of being made to produce at pleasure a greater or less reaction with a given number of revolutions, so that within reasonable limits the same instrument shall be capable of dealing with engines of great or small power, allowing each to make its proper number of revolutions.


Spar. 30 ft. Long. 10" diamr in middle
2 parts of 4⅝" iron wire rope
A
B
C
D
E
F

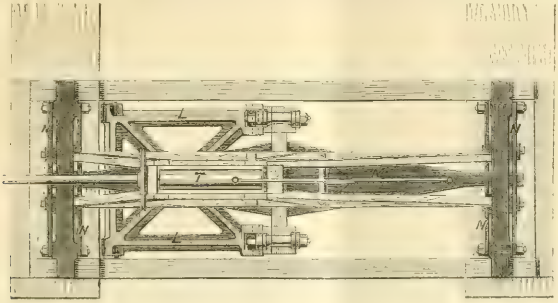
Enlarged view of dynamometer attached to screw shaft.

As regards condition No. 1, the theory shows, as will appear in the appendix, that comparing two strictly similar but differently dimensioned instruments, their respective "moments of reaction" with the same speed of rotation in each, should be as the fifth powers of their respective dimensions.



General view of dynamometer attached to screw shaft.

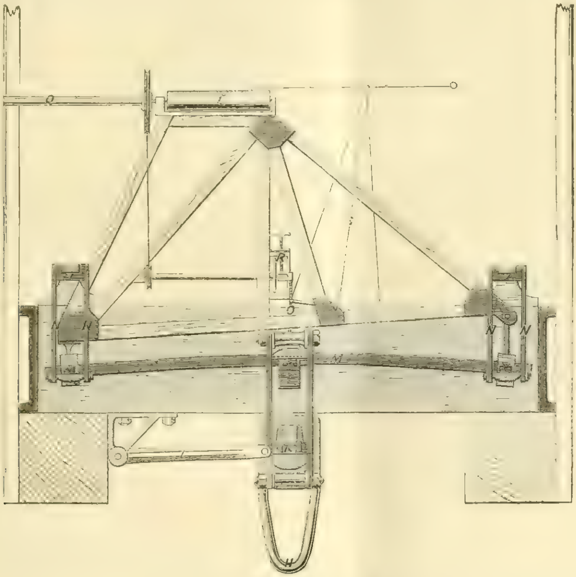
This proposition is fully borne out by experiment. Mr. Froude has had a pair of similar instruments made, in which the turbine diameters are respectively 12 in. and 9·1 in. Now $\left(\frac{12}{9\cdot1}\right)^5 = 4$, and accordingly the ratio of the moments of the two instruments at a given speed of turbine rotation should also have been 4. The ratio was in fact 3·86; but the small difference is referable to the circumstance that in the larger of the two instruments the internal surface was rather less smooth and the friction of the water consequently rather greater than in the other. The data thus obtained not only verify the scale of comparison based on the 5th power of the dimension, but they also furnish a starting-point by which to quantify the dimensions of the instrument which will be required to deal with any given horse-power delivered with a certain number of revolutions per minute; and it thus appears that to command the measurement of 2,000 horse-power delivered with 90 revolutions per minute (a fairly typical speed for the power), an instrument of the dimensions shown in the accompanying drawings will suffice, the turbine being 5 ft. in diameter, and being in fact a duplicate turbine, or formed with two faces, with a double-sided casing to match. This two-faced arrangement, it may be added, while it supplies a doubled circumferential reaction with a given diameter, has the advantage of obliterating all mutual thrust on the working parts; the centrifugal forces of the double set of vortices pressing with equal intensity on the two internal opposite faces of the rigid casing.



Integrating apparatus. Plans.

As regards condition No. 2, the theory suggests that, by contracting the internal waterways, that is to say the passages through the cells in the turbine and the casing, and thus intercepting the free vortical rotation, all other things remaining the same, the moment of reaction due to a given speed of rotation could be greatly reduced.

The experiments with the models fully bore out this anticipation also, and proved that, by the very simple arrangement shown in the drawings, the reaction with any given speed of turbine rotation can be reduced with a perfectly graduated progression in any required ratio down to 1-14th, the object being effected by advancing, from recesses in the casing abreast of the two opposite quadrants in each turbine, a lunette-shaped sliding shutter of thin metal, so fitted as to be carried forward (by a screw motion governed from the outside) along the divisional plane between the turbine cells and the casing cells. The intensity of the reaction is thus brought completely and easily under command; and in virtue of it, it follows that the instrument represented in the drawing, which, as already stated, is capable of dealing with an engine of 2,000 horse-power, making ninety revolutions per minute, is also capable of dealing with one of only 340 horse-power, making 120 revolutions per minute. And as the reaction of the instrument varies as the square of its speed of rotation, and the horse-power delivered through it consequently varies as the cube of the speed of rotation (that is to say, with a given setting of the shutters), and as, moreover, this law of variation is somewhat the same as that which the engine itself



Integrating apparatus. Section.

experiences when propelling the ship under natural conditions, it follows that the same setting of the shutters which suits a given engine when working with its highest speed and power will also approximately suit it when eased down to its lowest.

It remains to be explained in detail how it is proposed to carry out the operation in dealing with any given ship.

The ship, before she leaves the dock for the trial of her machinery, will have the instrument mounted as described, in place of her screw. The casing will be provided with proper apertures, capable of being closed at will, to permit the egress of air and the ingress of water as the dock fills. The casing will thus be in a condition to receive the moment of rotation delivered by the screw and communicate it to the recording apparatus.

The arrangement of the dynamometric apparatus presents no difficulty. In this, the downward pull delivered by the lever operates vertically on the middle of a flat horizontal steel spring, which is supported at both ends; and it is proposed so to proportion the spring that its maximum deflection shall be about 1½ inches. Different springs, however, would be required for engines if of widely different power.

Reference has previously been made to the amount of heat developed by friction in the friction brake, as probably the most formidable of the objections to its employment when the horse-power to be dealt with is as large as that now contemplated. But it must not be supposed that the absorption of the same amount of work in the instrument that has been described will fail to be converted into the same amount of heat here also. The dynamic theory of heat is unquestionable as a theory, and the quantitative relation of work and heat is known with certainty within far narrower limits than deserve even to be mentioned in reference to the present subject. Although, however, the extinction of say 2,000 horse-power will in fact here, as well as in the friction brake, consist in its conversion into so many units of heat, the circumstances of the conversion are entirely different in the two cases, and the difference is such as to obliterate here the inconvenience which was fatally great there. There, the heat was to be dealt with as being constantly developed between surfaces in close contact and inaccessible to water. Here, it will be making its appearance in the body of a mass of water; and though the rapidity of the development will be so great that the whole contents of the casing would be quickly raised to boiling heat if the heat had no escape, yet, in the first place, there is a considerable refrigerating power always at work, since the whole casing is enveloped in cold water, and, moreover, there is no difficulty in creating a constant change of water within the casing sufficient to keep down the mean internal temperature to any limit which may be thought proper. For instance, when the instrument is dealing with 2,000 horse-power, the temperature would be kept well below the boiling point if in each minute eight cubic feet of cold water be substituted for the same quantity of the hot contents of the casing, nor would the exactness of the dynamometric action be in the smallest degree impaired by the substitution.

Mr. Froude in his valuable paper, to which we are glad to call attention, thus summarises briefly the advantages which would be derived from the system of submitting marine engines to dynamometric trial. It is certain that a very large but unmeasured amount of power is wasted, in friction and otherwise, between the cylinders and the propeller; and that the amount probably differs, both in respect of difference in type of engine and in respect of goodness of construction and workmanship. The chief difficulties which thus arise are as follows:—

(1) The speed attained by a given ship, driven by a given indicated horse-power, fails to measure discriminatively the merits of the ship.

(2) No means exist of testing which type of engine delivers the largest proportion of the power which it indicates.

(3) No test exists by which to measure concisely the specific constructional merit of this or that engine, or to determine the relative constructional merit of the engines supplied by different firms.

The dynamometric test would remove at once each of these difficulties, by substituting a final and real test for a collateral and to a large extent a delusive one. For to rely exclusively on the test furnished by the indicator is almost equivalent to testing the power of a horse solely by the quantity of food he consumes and digests, or the efficiency of a boiler solely by the quantity of coal per hour it will legitimately consume on its firebars.

Table of Reference Letters used in Diagrams and Drawings.

A.	Screw shaft.
B.	Turbine.
C.	Casing.
D.	Diaphragms.
E.	Sliding regulating shutters.
F.	Screws for moving E, governed by telescopic rods actuating bevel gear controlled from ship's deck.
G.	Lever for holding casing
H.	Links connecting G with dynamometric apparatus.
K.	Knife-edged gimbal for carrying strain of H to spring.
L.	Framed radius for guiding K and eliminating oblique strains.
M.	Dynamometer spring.
N.	Suspension links carrying the ends of M.
O.	Feeler conveying elastic motion of M.
P.	Telescopic rod taking rotation of screw shaft by bevel gear and communicating it to integrating apparatus.
Q.	Motion axis of integrating apparatus governed by O.
R.	Automatic integrator.
S.	Bell crank for magnifying motion of O and conveying it to paper cylinder.
T.	Paper cylinder recording magnified motion of O. The graphic integration of the record given by T is comparable with the automatic integration given by R.
U.	Shed covering integrating apparatus.
V.	Strong balk brackets upholding U.

THE COMMISSION OF THE FRENCH ACADEMY AND THE PASTEUR-BASTIAN EXPERIMENTS

IN further reply to a communication of mine to the Academy of Sciences of Paris on July 10, 1876, and as his latest contribution to a controversy which grew out of it, M. Pasteur, at the *séance* of January 29, 1877, threw down a very definite challenge.

The discussion was raised according to M. Pasteur by my statement, "that a solution of boiled potash caused bacteria to appear in sterile urine at 50° C., after it had been added to the latter in quantity sufficient for exact neutralisation," and he then said:—"I defy Dr. Bastian to obtain, in the presence of competent judges, the result to which I have referred with sterile urine, on the sole condition that the solution of potash which he employs be pure, *i.e.*, made with pure water and pure potash, both free from organic matter. If Dr. Bastian wishes to use a solution of impure potash I freely authorise him to take any in the English or any other Pharmacopœia, being diluted or concentrated, *on the sole condition that that solution shall be raised beforehand to* 110° *for twenty minutes, or to* 130° *for five minutes.* . . . This is clear enough, it seems to me, and Dr. Bastian will understand me this time."

At the *séance* of February 12 my reply was read. The essential part of it was as follows:—"During the last week I have repeated my experiments several times, and with a degree of precaution going much beyond the severity of the conditions prescribed by M. Pasteur. . . . I repeated them at first with liquor potassæ which had been previously raised to 110° *C. for sixty minutes*, and afterwards with liquor potassæ which had been raised, in the same manner, to 110° *C. for twenty hours*. The results have been altogether similar to those produced upon sterile urine by liquor potassæ which has been raised only to 100°, when added in suitable quantity; that is to say, *in twenty-four to forty-eight hours the urine was in full fermentation and swarmed with bacteria*."

After the reading of this reply, M. Pasteur asked the Academy to appoint a Commission to report upon the subject in dispute, and at the next meeting of the Academy (February 19) it was announced that "MM. Dumas, Milne Edwards, Boussingault sont designés pour constituer la Commission qui sera appelée à exprimer une opinion sur le fait qui est en discussion entre M. le Dr. Bastian et M. Pasteur."

The following correspondence then ensued:—

20, *Queen Anne Street, W., February* 27, 1877

DEAR SIR,—I was pleased to learn, from the *Comptes Rendus*, yesterday, that the Academy had appointed you together with MM. Milne Edwards and Boussingault to act as a Commission to "express an opinion on the fact" now under discussion between M. Pasteur and myself.

I can scarcely suppose that the Commission would deem it expedient to express an opinion on this subject without having an opportunity of seeing both M. Pasteur and myself perform our respective experiments.

I write, therefore, to inform you that if a convenient time can be arranged, I shall be very happy to come to Paris for three days in order to perform my experiments before the Commission which has been nominated by the Academy.

I should, moreover, feel much obliged if you will have the goodness to inform me exactly what steps the Commission proposes to take, and how the precise terms for formulating the question of fact which is to be submitted to their consideration are to be settled. It appears to me that these terms ought, in the first place, to be agreed upon between M. Pasteur and myself. Faithfully yours,

H. CHARLTON BASTIAN

Monsieur Dumas, le Secrétaire perpétuel,
Académie des Sciences

No reply to this letter was received, though a translation of it was published shortly afterwards in the *Comptes Rendus*. The first letter which subsequently came to hand on this subject was the following:—

Académie des Sciences, Paris, le 5 *mai*, 1877

MONSIEUR,—Je crains que la lettre que j'ai eu l'honneur de vous adresser il y a trois semaines ne vous soit pas parvenue, et je prends la liberté de vous faire savoir de nouveau que la Commission chargée par l'Académie des Sciences de prendre connaissance de vos expériences est prête à vous recevoir. Elle a déjà demandé à M. Pasteur d'opérer sous ses yeux.

Puisque vous avez accepté de venir à Paris, tout est preparé pour vous recevoir et dès vôtre arrivée, si vous voulez bien m'en informer, le laboratoire de l'École Normale, ou tout autre, seront mis à votre disposition.

Agréez, Monsieur, l'assurance des mes sentiments les plus distingués, J. B. DUMAS

Paris, rue St. Dominique, 69

20, *Queen Anne Street, W., London, May* 8, 1877

DEAR SIR,—On February 27 I had the honour of informing you that I was willing to come to Paris to perform some experiments before the Commission appointed by the Academy, if a convenient time could be arranged. I asked also to be informed as to the steps the Commission proposed to take, and how the precise question submitted to them for report was to be agreed upon.

I anxiously awaited a reply to this letter for some time, but none came.

This morning I had the honour of receiving a letter from you, bearing the date of May 5, which was reposted to me from a wrong address, viz., 81, Avenue Road, Regent's Park. Therein you state that you had written to me three weeks previously. I shall be glad if you will be good enough to inform me to what address this first letter was sent, as it has not yet come to hand; and I find, on inquiry, that it has not been delivered at 81, Avenue Road, where I resided two years ago. On receipt of this information I will make further inquiries at the General Post Office.

The letters which I have had the honour of addressing to you concerning my communications to the Academy have always borne the address which stands at the head of this sheet.

Three weeks ago, if the arrangements made by the Commission had been satisfactory, I could have gone to Paris without much inconvenience; now, however, my engagements, both public and private, will not permit me to leave London, and I fear it may be impossible for me to go to Paris till about the third week in July, when our medical session will terminate.

Meanwhile I trust to be able to recover your first letter, and I hope to be fully informed, not only as to the precise question on which the Commission is to report, but as to the mode in which the Commission will conduct the inquiry. I am still anxious, in fact, to receive that information for which I asked in my letter of February 27.

Believe me, dear Sir, faithfully yours,

Monsieur Dumas H. CHARLTON BASTIAN

18 *mai*, 1877, *Paris*

MONSIEUR,—Je me suis empressé de vous faire un duplicata de la lettre que j'avais eu l'honneur de vous adresser au nom de la Commission de l'Académie des Sciences, dès qu'elle avait été delivrée des soins de la séance publique tenue le 23 avril, et qui ne vous était pas parvenue.

J'ai vu M. Pasteur. Il se tient à vôtre disposition pour le 15 juillet, époque à laquelle vous seriez libre de venir à Paris.

M. Lockyer, qui a passé quelque jours ici, s'est chargé de vous dire combien nous desirons voir avec vous vos expériences et avec quelle entière liberté d'esprit elles seront appreciées.

Agréez, Monsieur, l'assurance de ma considération la plus distinguée, J. B. DUMAS

rue St. Dominique, 69

Académie des Sciences, Paris, le 25 *avril*, 1877

Duplicata.—Le Secrétaire perpétuel de l'Académie à Monsieur le Docteur Charlton Bastian, 20, Queen Anne Street, à Londres

MONSIEUR,—La Commission nommée par l'Académie des Sciences pour examiner le dissentiment qui s'est élevé entre M. Pasteur et vous a consacré plusieurs séances à suivre les expériences de M. Pasteur. Elle est donc en mesure de s'occuper des vôtres.

Puisque vous avez offert de venir les répéter devant elle à Paris, elle se met à vôtre disposition, et elle vous offre le laboratoire qu'il vous plaira de désigner pour les accomplir. Vous choisirez vous-même, après les avoir visités, celui qui vous conviendra le mieux. M. Pasteur vous prie de considérer le sien comme tout à vos ordres.

La Commission, avant d'engager tout examen de la question, a pensé qu'il convenait d'abord de voir les expériences mêmes, réalisées en liberté par leurs auteurs. S'il y a lieu d'ouvrir plus tard entre elles une comparaison contradictoire, elle en déterminera les conditions, en vue de donner, à son opinion, une base certaine.

Le premier élément de l'enquête à laquelle vous avez souscrit, M. Pasteur et vous, devait consister, en effet, à donner à chacun de vous l'occasion de produire les faits sur lesquels vos opinions respectives se fondent.

Agréez, Monsieur, l'assurance de mes sentiments les plus distingués. J. B. DUMAS

20, *Queen Anne Street, W., May* 24, 1877

DEAR SIR,—I have the honour to acknowledge the receipt of the duplicate of the missing letter, bearing date April 25, and also your note of May 18, the assurances in which were very gratifying to me.

Your official letter of April 25 contains some information in regard to the conduct of the inquiry by the Commission, which I have been for some time desirous of obtaining. In respect to these proposed proceedings I may perhaps now be permitted to make some observations, in order, as far as possible, to avoid the chance of any misunderstanding between M. Pasteur and myself and the Commission, during the progress of the inquiry.

I am anxious, in fact, to define (1) what I understand to be the object of the Commission, and (2) to explain to what extent I am prepared to submit to its judgment. I desire to do this in order that I may have the honour of learning from you whether I am correct in this understanding, and whether my submission to the extent to be specified is all that the Commission will expect from me.

1. I gather from the *Comptes Rendus* of February 19, that the Commission has been appointed that it may "express an opinion upon the fact" under discussion between M. Pasteur and myself; and the fact in question seems to me to be this:—*Whether previously boiled urine, protected from contamination, can or cannot be made to ferment and swarm with certain organisms by the addition of some quantity of liquor potassæ which has been heated to* 110° *C., for twenty minutes at least*. M. Pasteur asserts that he has not seen fermentation occur under these conditions, whilst I assert that I have; so that the point of principal importance would seem to be to ascertain whether such positive results can be reproduced before the Commission. I learn, therefore, with much satisfaction, that the Commission will allow to each of us the opportunity of reproducing before it the facts upon which we found our respective opinions. This, indeed, I regard as an essential condition of the inquiry.

2. If the Commission proposes to limit itself to reporting upon this mere question of fact I will freely submit to its decision. If, however, it does not propose thus to restrict itself, and is empowered to express an opinion upon the interpretation of the fact attested, and on its bearings upon the "Germ Theory of Fermentation," or "Spontaneous Generation," then I must respectfully decline to take part in this wider inquiry.

I feel compelled to adopt this decided position because my stay in Paris must be limited to three or four days; and if any other questions beyond that above specified were subsequently raised by the Commission demanding the performance of some new experiments, either by M. Pasteur or myself or by both of us, the inquiry, instead of being limited to a few days, might be prolonged indefinitely.

I desire, therefore, to obtain the assurance of the Commission that no new experiments shall be demanded from either of us, except with the full concurrence of both M. Pasteur and myself. Under these circumstances I will undertake, so far as it lies in my power, to be in Paris by the 14th of next July, in order to place myself at the disposal of the Commission.

Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

20, *Queen Anne Street, W., June* 21, 1877.

DEAR SIR,—One month ago (May 24) I had the honour of writing to you to ask for some official information as to the precise scope of the inquiry to be made by the Commission appointed by the Academy of Sciences, before whom I have been invited to appear. To this letter I have as yet received no reply, so that I do not even know whether it has been received.

I have made arrangements which will enable me to go to Paris and perform my experiments before the Commission at the time named in your letter of May 18, namely, about July 15, but naturally before taking part in any arbitration I desire to receive some official intimation as to the exact terms and scope of the question which has been submitted to the arbitrators. I know not whether the few lines which I saw in the *Comptes Rendus* of February 19 announcing the nomination of the Commission, contain also the only instructions which have been given to it, or whether any other and fuller instructions exist. No information has been communicated to me and I am, unfortunately, not acquainted with the custom of the Academy in regard to commissions of this kind.

Craving the favour of an early reply,

Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

MONSIEUR,—Il est parfaitement entendu que la Commission de l'Académie des Sciences sera le 15 juillet à vôtre disposition.

Il est également qu'elle desire, si c'est possible, n'avoir à s'occuper que de l'expérience de M. Pasteur et de la vôtre, au sujet de l'urine traitée par la potasse.

Vous n'avez donc aucun motif de craindre qu'elle ait besoin de vous demander un séjour prolongé.

Veuillez agréez, Monsieur, l'assurance de ma considération la plus distinguée. J. B. DUMAS

r. St. Dominique, 69

20, *Queen Anne Street, W., July* 6, 1877

DEAR SIR,—I beg to acknowledge the receipt of a letter from you which came to hand on June 29.

I do not find in it any distinct acceptance of the conditions mentioned in my letter of May 24, as those upon which alone I should be prepared to repeat my experiments before the Commission, viz., (1) the limitation of the report to the question of fact mentioned, (2) the assurance that no new experiments shall be demanded from either of us except with the full concurrence of both M. Pasteur and myself.

I might infer from your silence that no objection is raised to these restrictions, but before leaving for Paris I must receive your definite assurance that this is so.

Not being thoroughly proficient in the French language I presume the Commission will permit me to avail myself of the services of some French friend as an interpreter. I also trust that the Commission will provide for the taking of shorthand notes of any discussion during the progress of the investigation of which the Commission, M. Pasteur, or myself may desire to have a record.

On the receipt of a favourable reply you may expect me to be in Paris on Saturday morning, the 14th inst., otherwise I shall be most reluctantly compelled to decline to participate in the inquiry. Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

Paris, 12 *juillet*, 1877

MONSIEUR,—La Commission de l'Académie des Sciences sera dès le 15 à vôtre disposition.

Elle est prête à vous entendre ; mais elle desire, comme vous, *que son examen soit borné au point en discussion entre vous et M. Pasteur.* Ce serait seulement au cas où vous desireriez aller plus loin qu'elle aurait à examiner si le temps lui permet d'entreprendre davantage, vôtre séjour étant très court.

M. Edwards, membre de la Commission, parle très bien l'anglais.

Dès vôtre arrivée vous auriez la bonté de m'en informer, rue St. Dominique, 69.

Agréez, Monsieur, l'assurance de ma considération la plus distinguée. J. B. DUMAS

Having received this acceptance of the limitations which I had specified, I left London for Paris on July 13.

On the afternoon of July 15, I met the Commission by arrangement at the laboratory of M. Pasteur, at the Ecole Normale Supérieure. The Commission was represented by MM. Dumas and Milne Edwards, M. Boussingault having been compelled to withdraw on account of a recent domestic affliction.

The first stage of our discussion was the announcement to me by M. Milne Edwards of his objection to the second condition mentioned in my letter of July 6, and of his determination to take no part in the inquiry if I still adhered to this condition. M. Dumas' letter of July 12, in the name of the Commission, and on the faith of which I had come to Paris, was thus at once set aside.

M. Milne Edwards contended that he could not take part in any Academy Commission which had not full power to vary the experiments at discretion ; whilst I, on the other hand, contended that my stay in Paris must, as I had said from the first, be limited to a few days, and that I could not see my way, therefore, to consent to the initiation of new experimental conditions. I further urged that the Commission had been appointed to report upon a simple question of fact, that M. Pasteur had challenged me to obtain certain results, before "competent judges," that I had come to Paris to repeat certain well-defined experiments before them, and that they were commissioned to express an opinion thereon and on the experiments of M. Pasteur to the Academy of Sciences.

A very long discussion ensued, but no satisfactory conclusion was arrived at. In the evening I wrote the following note to M. Dumas :—

Grand Hôtel St. James, Paris, July 15, 1877

DEAR MONSIEUR DUMAS,—After our conference this afternoon I had a long conversation with M. Pasteur, and am going to his laboratory early to-morrow morning, to show him the mode in which I make my experiments. I shall thus be enabled to learn what precise alterations he would desire in order that the experiments may be conducted in a manner satisfactory to himself.

Afterwards I trust it may be more possible for me to meet the wishes of the Commission in regard to the inquiry, and I hope you will therefore be able to make it convenient to see me for a few minutes at your own house to-morrow at 1.30 P.M.

If you are able to do this, pray do not take the trouble to answer this note. Should it not be convenient to you, perhaps you will kindly send a few words to me to the care of Professor Würtz, upon whom I am to call about noon.

Believe me, dear Sir, faithfully yours,
À Monsieur Dumas H. CHARLTON BASTIAN

At my interview with M. Dumas on Monday, July 16, I proposed a kind of compromise. The proposition was that on the present occasion we should have "the first element" of the inquiry as defined by M. Dumas in his letter of April 25 ; viz., that the opportunity should be given to M. Pasteur and myself of repeating (without variation) the actual experiments upon which we based our respective opinions ; that I should then return to London, and after the Commission had expressed its opinion to M. Pasteur and to myself as to any variations in the experimental conditions which they might desire to institute, that I should return to Paris to witness and to perform such modified experiments.

The names of MM. Fremy, Trécul, Robin, and Würtz had been mentioned as persons one or other of whom I should like to see placed on the Commission in succession to M. Boussingault. But at the meeting of the Academy that afternoon it was announced that M. van Tieghem had been nominated to succeed M. Boussingault. This gentleman being a former pupil and present colleague of M. Pasteur, the Commission was left without a single member who could be considered as representing my views, or even as holding a neutral position between me and my scientific opponent.

The next day I received the following note from M. van Tieghem :—

Paris, 17 *juillet*, 1877

MONSIEUR LE DOCTEUR,—La Commission de l'Académie se réunira *demain, mercredi, à huit heures du matin* au laboratoire de M. Pasteur à l'Ecole Normale. Je viens, en son nom, vous prier de vouloir bien vous y trouver pour procéder à la mise en train des expériences en litige.

Veuillez agréez, Monsieur, l'expression de mes sentiments les plus distingués.

PH. VAN TIEGHEM,
Membre de la Commission

I made all the necessary arrangements that afternoon in M. Pasteur's laboratory for the performance of my experiments, and the next morning at eight o'clock M. Pasteur and I were at the appointed place. M. van Tieghem was also there, and shortly afterwards M. Milne Edwards arrived. He apparently had had no communication with M. Dumas since the time of my interview, and when told, in reply to a question of his, of the proposition which I had made to M. Dumas, M. Milne Edwards very hastily expressed his disapproval of it, and at once, without listening further, left the laboratory. He was followed by M. van Tieghem. I remained, and after one hour M. van Tieghem returned. He informed me that, having waited in vain for the arrival of M. Dumas, M. Milne Edwards had at length gone away.

I remained in conversation with M. van Tieghem for nearly an hour in an upper room of M. Pasteur's laboratory. When we came down, much to my surprise, we learned from M. Pasteur that M. Dumas had arrived, that he had been told of the departure of M. Milne Edwards, and that he also had then left, saying that the Commission was at an end—but without in any way communicating either with his colleague, M. van Tieghem, or with myself.

Thus began and ended the proceedings of this remarkable Commission of the French Academy.

July 30 H. CHARLTON BASTIAN

NOTES

FROM correspondence which we have received, we gather, that because we omitted to state in our leading article of last week the fact that London is the only University which treats science as a necessary branch of education, that article has been thought hostile to the University of London. The fact in question is of course well known and appreciated, but it did not seem to us to be relevant. Our article had reference to the question of Universities as against Examining Boards rather than to the quality of the examinations. We heartily acknowledge the good the London Examining Board has done, and the obligations under which it has placed science and scientific men.

THE Annual Conference of the Royal Archæological Institute of Great Britain and Ireland commences, on the 7th proximo, at Hereford, for a week. The Bishop of Hereford is president.

AN important resolution of the International Geodetic Congress is now being carried out. The Montsouris observatory is being connected by telegraphic observations with Bonn and Berlin in Germany, and with Geneva and Neufchatel in Switzerland. Two astronomers from Berlin having arrived in Paris, and M. Lœwy, member of the French Academy of Sciences, with two assistants, having arrived in Berlin from Paris, the work has been at once proceeded with. The wires are freed a few hours every night for obtaining comparisons. The connection with Geneva and Neufchatel is executed, *viâ* Lyons, by Commander Perrier, of the staff, and the operations have been continued to Marseilles and Algiers. The comparison between the Montsouris and Paris observatories will be a work of triangulation, the two establishments being about a gun-shot from each other.

A NUMBER of Abyssinians have arrived in Paris on their way to London. They are encamped in the Acclimatisation Gardens (Bois de Boulogne), with camels, elephants, ostriches, &c., and other animals destined to the London Zoological Gardens. The heads and manners of the blacks have been scientifically examined by Dr. Broca, and a report on them will be read at the French Society for the Advancement of Science at Havre.

THE Bureau of the French Association to meet in Havre on the 23rd instant, consists of Prof. Broca, president; M. Kuhlmann, vice-president; M. Deherain, general secretary; M. Perrier, vice-secretary; M. Masson, treasurer. Most of the French railway companies give half-price tickets to persons going to the Association. The hotel proprietors in Havre guarantee a certain number of beds; furnished apartments have also been largely promised, and the berths in one of the Transatlantic Company's steamers have been placed gratuitously at the disposal of members.

AN interesting account of the recent falling of a mountain in Tarentaise, Savoy, causing disaster to two flourishing villages, has been communicated to the *Courrier des Alpes*, by M. Bérard. The phenomenon has been incorrectly reported as instantaneous, and the destructive effect complete, whereas the case is that of a mountain which for twenty days, without cessation, has been dismembering itself and literally falling night and day, into the valley below, filling it with piled-up blocks of stone, extinguishing all sounds by its incessant thunder, and covering the distant horison with a thick cloud of yellowish dust. The entire mass comprised in the slope forms a mutilated cone 200 metres broad at the top and 600 at the base (the slope being about 50°); this is composed of blocks of hard schist lying close together, but no longer united; and it is united to the body of the mountain only by a vertical mass 40 to 50 m. thick, which already is fissured and shaken. Periods of repose occur lasting only a few seconds or a minute at most; then the movement recommences, and continues about 500 hours. Blocks of 40 cubic metres become displaced with no apparent cause, traverse the 1800 m. of descent in thirty seconds, leaping 400 or 500 m. at a time, and finally get dashed to pieces in the bed of the torrent, or launch their shattered fragments into the opposite forest, mowing down gigantic pines as if they were so many thistles. One such block was seen to strike a fine fir-tree before reaching the bridge between the villages; the tree was not simply broken or overthrown, but was crushed to dust (*volatilisé*); trunk and branches disappeared in the air like a burning match. Rocks are hurled together and broken into fragments that are thrown across the valley like swallows in a whirlwind; then follow showers of smaller fragments, and one hears the whistling sound of thousands of pebbles as they pass. M. Bérard reached the edge of the rock (2,460 m. high), on one of the sides of the falling cone, and ventured along it, obtaining a good view of the "terrifying" spectacle. He reaffirms his conviction that the phenomenon is inexplicable by any of the usual reasons that account for Alpine disturbances, such as penetration of water, or melting of snows, or inferior strata in motion; nor does the declivity of the slope explain it. His hypothesis is that some geological force is at work, of which the complex resultant acts obliquely to the axis of the mountain and almost parallel to its sides.

ACCORDING to M. Perrin, an eighth or a tenth portion of the French army is incapable of doing good service, in consequence of indistinct vision. M. Perrin formally proposes to remedy this by the adoption of spectacles. It is affirmed that spectacles are useful, if not indispensable, to 47 per cent. of the officers coming from the École Polytechnique.

FROM the Annual Report of the Council of the Royal Society of New South Wales, we gather that the membership at the beginning of the session of 1877 was 298, and that the Society is in a generally flourishing state. A considerable access of activity has occurred since the establishment of sections (nine) last year. The Council are hopeful of obtaining an annual endowment from the Government.

FOR want of space the gigantic Giffard captive balloon will not be constructed, as was anticipated, in the Paris Exhibition, but special ground will be granted as we announced a few months ago. The spot selected is the Carrousel interior yard. The large space within the railings has been found sufficient, after special inspection by MM. Lefeul and Tetreau. The ministerial sanction is expected daily. M. Giffard is continuing his experiments on the production of hydrogen gas with continuous apparatus.

A BALLOON was sent up on Wednesday carrying an aëronaut, and elicited an interesting fact of aërial physics. The ground current was blowing gently from north-west, but higher up a south-west current was met by the aëronaut. The balloon was carried at a rate of 500 metres per minute to the north-east of Paris. In the night 8 millimetres of rain fell, the upper current having descended into contact with the ground.

A GERMAN Society for the Exploration of Palestine has recently been started by Dr. Zimmermann, Gymnasial Rector in Basle, along with Professors Kautzsch and Socin, of Tübingen. Several other *savants* have joined it. The first quarterly number of the society's projected journal will appear shortly. The annual contribution to the society (10 marks) entitles one to receive the journal.

IT is proposed in Stuttgart to erect a simple monument over the grave of Th. v. Heuglin, the well-known African traveller, recently deceased. The committee, at whose head is Prince Hermann of Saxe-Weimar, invite subscriptions.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. C. L. Norman; three Chaplin Crows (*Corvus capellanus*) from Persia, presented by Dr. J. Huntley; a West African Python (*Python sebæ*) from West Africa, presented by Mr. Lionel Hart; a Red River Hog (*Potamochœrus penicillatus*) from West Africa, received in exchange; a Barbary Ape (*Macacus inuus*) from North Africa, a Squirrel Monkey (*Saimaris sciurea*) from Guiana, deposited; a Military Maccaw (*Ara militaris*) from South America, purchased; ten Amherst pheasants (*Thaumalea amherstiæ*), three Temminck's Tragopans (*Ceriornis temminckii*) bred in the Gardens.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, July 23.—M. Peligot in the chair.—The following papers were read:—New researches on electro-capillary phenomena, by M. Becquerel. One experiment is this: into a cracked tube containing nitrate of silver solution are introduced some very small fragments of carbon, and the tube is put in a vessel holding monosulphuret of sodium. Here the wall of the crack in contact with the inner solution is the negative pole of the electro-capillary couple, and that in contact with the outer solution the positive. Not only does the negative wall get covered with metallic silver, but the carbon fragments are also coated in proportion to their nearness to the crack. Each fragment acts like the crack. The action is like what occurs in a metallic circuit composed of several conductors.—Fixation of nitrogen on organic matter and formation of ozone under the influence of weak electric tensions, by M. Berthelot. He has given up metallic armatures, introducing the gas into an annular space between two vessels holding dilute sulphuric acid solution, which were connected with the battery poles. He mentions four reactions in which formation of ozone has thus been obtained. Again, to estimate fixation of nitrogen, a glass cylinder (with spherical calotte), internally covered with tin, externally half with water-moistened Berzelius paper, half with syrupy solution of dextrine, was placed on a lac-covered glass-plate and enclosed in a concentric glass cylinder with outer coating of tin; the tin armatures were connected with five Leclanché elements during several months, and fixation of nitrogen in paper and dextrine was demonstrated. He shows the application of such facts.—On an experiment by Dr. Bastian relating to urine neutralised by potash, by M. Pasteur. He describes a form of Dr. Bastian's experiment he has performed several times in presence of Academy members, and never got bacteria; the nature and treatment of the vessel is a salient point.—Tertiary strata of Hungary (continued), by MM. Hebert and Munier-Chalmas.—On the electric conductivity of trees, by M. Du Moncel. After referring to the local currents and currents of polarisation got on applying to each tree two platinum electrodes 9 ctm. square, with an interval of 6·44 m., he gives a table of resistances for various species. The soft woods with spongy tissue and vigorous vegetation, such as elm (resistance 1,431 km.), chestnut (1,694), lime (1,988), poplar (2,090), are the best conductors. Among hard woods with slow vegetation, box had a resistance of 12,511 km. Birch (4,777) formed an exception.—Reply to M. Cosson's observations on the Saharan Sea, by M. D'Abbadie. M. de Lesseps corroborates M. D'Abbadie's arguments.—On the ophitic phenomenon in the Pyrenees and the Haute-Garonne, by M. Leymerie. Ophite proper and lherzolite are two different but concomitant facies of an eruptive phenomenon characteristic of the Pyrenees, which may, as a whole, be termed *ophitic*. It is only met with in the lower part of slopes.—Reply to M. Naudin's observations on the interior sea of Sahara, by M. Roudaire.—On the degree of efficacy of sulphide of carbon as a means of destruction of phylloxera, by M. Boiteau.—On the grape-disease of the Narbonnese vineyards, by M. Cornu.—On the Doryphora of potatoes, by M. Girard. He thinks sulphocarbonate of potash would be useful against it; also that the fear of the beetle is exaggerated. Another chrysomelian (*Colaspidema atrum*), which attacks lucern in France, is very like the Colorado beetle in its ways, and it is successfully resisted.—On curves having the same principal normals, and on the surface formed by these normals, by M. Mannheim.—On the extension to space of two laws relative to plane curves, given by M. Chasles, by M. Fouret.—Influence of heat on magnetisation, by M. Gaugain. Certain magnetic bars of Sheffield steel heated and let cool are found at last to have changed in the sign of their magnetism.—On the magnetisation of circular plates where the isodynamic lines are concentric circumferences, by M. Duter.—On the electrolysis of sulphurous acid, by M. Gueront. This substance is decomposed like a salt.—Note on the determination of manganese, nickel, zinc, and lead, by M. Riche.—On the density of vapour of sulphhydrates of ammonia, by M. Horstmann.—On the nature of gases contained in the tissues of fruits, by M. Livache. He applied M. Schlœsing's analytic method of immersion in ether (without lesion of tissue). In the tissues of healthy fruit the gases are a mixture of nitrogen and oxygen in the proportions found in air.—On the products of fermentation of the mud of Paris, by M. Maumené.—On the fecundation of the star-fish and sea-urchin, by M. Fol.—On the anatomy and the migrations of oxyurides, parasites of insects of the genus Blatta, by M. Ghaleb.—Influence of the sun and moon on magnetic and barometric variations, by M. Broun.—Some observations on the trajectory of hail during thunderstorms, by M. Ziegler. A hailstone cannot (he considers) attain a great weight except through a long course in dense air in the lower regions of the atmosphere, and he cites cases to prove that the trajectory of large hailstones forms a very acute angle with the ground.

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ERRATUM.—P. 238, col. 1, line 9 from top, for *Ekdennite* read *Ekdemite*.

THURSDAY, AUGUST 9, 1877

ELECTRICITY IN WAR

THE important *rôle* played by electricity in modern warfare affords an excellent example of the influence which science has of late exerted in naval and military affairs. It is no isolated example of scientific warfare that we have here to deal with, for the electric fluid has in a great measure changed our whole practice of war, and bids fair to revolutionise it still more in the future. Every soldier or sailor, if he desires to make his mark, must be something of an electrician, for there seems to be no limit to the useful applications of the galvanic spark in battle. Broadly, we may divide these applications under three heads; namely, the employment of electricity for signalling, for the explosion of charges, and lastly, for illumination, both for the purposes of attack or defence, it being a difficult matter to decide in which connection the electric spark fulfils the most important duty.

To begin with the telegraph. All will agree that it is well-nigh impossible to overrate the advantages which this rapid means of communication gives to the general, in these days, when the line of battle sometimes extends for a dozen miles. Let the commander occupy the most central position, a long time must elapse before his aides-de-camp can communicate with one wing or the other. Assisted by the electric telegraph, however, the general is as close to his subordinates as if he were within shouting distance. Even a brigade of horse artillery, or a cavalry division advancing at a gallop, can carry its telegraph equipment with it, the operators accompanying a flying column of this nature with but very little difficulty. The wire drums are started off at a gallop, the cable being unwound as the carts proceed, and a sergeant on horseback with a "sounder" to his ear, in connection with one end of the wire, receives the general's commands as soon almost as they are spoken. The movement countermanded or a retreat ordered, the cable is again wound up as readily as it was laid down, and the telegraphers make good their return with the rest of the troops. Where ordinary movements are executed, use is of course made of the telegraph wagon, a comfortable little office on wheels, furnished with all things necessary for the receipt and despatch of messages, but this convenience is naturally out of place where a rapid change of front, or some speedy flank movement has to be executed.

Coming next to the explosion of charges by means of the electric spark, we enter upon a phase of war-science which bids fair to grow to infinite proportions. Both Franklin and Priestley suggested the employment of electricity in this connection more than a hundred years ago, but it is very recently indeed that we have been in a position to make proper use of this valuable agent as a means of firing charges at a distance. In fact, at the present moment we have by no means exhausted research in this direction, and we find scientific soldiers and sailors still at variance with one another as to the best plan of using the electric current for firing purposes. One of the first applications made of the subtle fluid was in the removal of the wreck of the *Royal George*, at Spithead, nearly fifty years ago, when the explosion of the charges was brought about by what is termed a wire-fuse, or in other words a short piece of platinum thread stretched between two copper wires. The platinum bridge having less conducting power than the copper wires, presents a considerable amount of resistance to any current of electricity that passes, and, in doing this, becomes so heated, as to be capable of igniting any particles of gunpowder in contact with it. A wire-fuse of this description has simply to be placed in the middle of a charge, and if then a current of electricity is passed from a battery along the wire in connection with the fuse, instantaneous ignition is the result. This simple method of firing charges under water was a vast improvement over the old one in use by our engineers, which consisted in leading up a metal pipe from the charge to the surface of the water; the outlet of the pipe was placed as far as possible from the charge beneath the water, and then a ladle full of red hot shot was emptied down it, and so reached the gunpowder below, which thereupon exploded if the iron fragments had not become too cool in transit.

But for many purposes the wire-fuse is ill-adapted to the military and naval services. A voltaic battery is necessary to evolve the low-tension electricity required to yield sufficient resistance and heat, and such a battery made up of metal plates, and involving the use of acids, is an awkward apparatus to carry in the field. Already in 1853, this fact seems to have occurred to a Spanish officer, Col. Verdu, who determined to see what could be done in the way of exploding gunpowder by a spark, or in other words, by high tension electricity. Aided by a Ruhmkorff coil he succeeded in firing half-a-dozen charges simultaneously, and although the discharge was sometimes a matter of considerable uncertainty, to Verdu certainly belongs the credit of having been the first soldier to apply electricity in this way to the firing of one or more mines. Wheatstone and Abel followed in Verdu's footsteps, and while the former directed his attention to the construction of a frictional apparatus of a portable nature, which should be suitable for military use, the latter busied himself in the preparation of a fuse inclosing a compound more delicately explosive than gunpowder, a fuse, by the way, which still retains an important place among our warlike stores.

It was in the China war of 1860 that we first find an electric firing apparatus forming part of an army equipment. In this case the outfit was of a somewhat clumsy nature. A conveyance, in shape and size much resembling a baker's barrow, contained a monster horse-shoe magnet, and it was the sudden disruption of its armature from this magnet which generated the spark to fire the fuse. A few years afterwards, this ponderous conveyance gave place to a neat little mahogany box about a foot cube, which contained half a dozen small but powerful magnets, in the field of which the armatures were made to revolve with exceeding celerity; and it is by means of such an apparatus that to day we are enabled to fire a score of charges at a time, the wires branching off from the instrument to a distance of a hundred yards or more. But, nevertheless, we have yet to devise, it seems, an efficient exploding apparatus capable of igniting both low and high tension electric fuses.

As everybody knows, it is by reason of electricity being employed to fire explosive charges that torpedo warfare has of late attained to so important a position. In the Whitehead, or fish-torpedo, the electric fluid, it is true, plays no part, but this is the only notable exception. In the floating torpedo, the moored torpedo, and the spar-torpedo, electricity is the life and soul; at one moment the machine is but a floating buoy or sunken impediment, the next it is transformed into a terrible volcano. A feeble current of electricity flashing along the wire, has on the instant sufficed to bring about the fatal change.

Passing from torpedo warfare and the recent attempts that have been made to turn electricity to account in the construction of self-steering launches, we come to a scarcely less important matter, that of firing guns by the electric spark. Not only are guns at proof and those under experiment so ignited, but on board the modern ironclad it is the custom now-a-days to fire broadsides in this wise also. By leading wires from every gun to one point, which is specially adapted for observation, the double advantage is secured of bringing about the firing at the most opportune moment, and of securing a simultaneous discharge. Some experiments made in Germany have proved beyond doubt that an armour plate struck instantaneously in this way by several shot, may be effectively broken up, whereas the ordinary broadside fire, brought about by gunners at word of command is incapable of doing so. The wires may be led into an observing tower, or half way up the mainmast if need be, and here the firing officer can calmly consort his measures undisturbed by the smoke, and noise, and bustle going on below him. He is provided with proper sights, and the guns being laid in accordance with his orders, he can watch the opportunity for firing as well as if he had his eye to the weapons themselves.

Finally, we have the use of the electric light in warfare. It is the most recent application of all of this wonderful agent, and we should hesitate to say how extensive may hereafter be the employment of electricity in this connection. In the Franco-German war, the first use of this powerful source of illumination was made by the French engineers, and from the forts around Paris the electric rays were made to sweep in all directions, to watch for hostile troops engaged in the operation of mining. Bodies of soldiers upwards of a mile distant could be plainly seen by the vivid light of the electric lamp, and working parties were frequently compelled to abandon their object in the presence of this powerful detector. As a means of discovering the approach of torpedo launches at night, the electric light will obviously be of value, and already a trial of it has been made in several of Her Majesty's ships. The *Alexandra*, the flagship of the Mediterranean fleet, is provided with an electric lamp, worked by one of Wilde's powerful machines, so that the efficiency of the apparatus may be practically tested. Experiments, however, have already shown what the electric rays are capable of doing, and a low torpedo-launch cannot approach within a thousand yards without detection, while if painted of a neutral grey, so as the better to escape observation by day, the vessel, it appears, is all the more perceptible under electric illumination. Steamers, we are told, are peculiarly liable to be detected by an electric lamp, since the rays are reflected by the steam and smoke as effectively as if the latter were a solid screen. How valuable, too, the electric light on board ship must prove for signalling purposes may be gathered from the fact that the Dungeness light, which was the first one of an electric nature constructed in this country, can be seen on a clear night at a distance of thirty miles with all the brilliancy of a star of the first magnitude.

H. BADEN PRITCHARD

THE GEOLOGY OF THE VIENNA WATER SUPPLY

Geologie der Kaiser Franz-Josefs Hochquellen-Wasserleitung. Eine Studie in den Tertiär-Bildungen am Westrande des Alpinen Theiles der Niederung von Wien. By Felix Karrer. (K.K. geolog. Reichsanstalt. Vienna, 1877.)

THE publications of the Austrian Geological Institute are deservedly noted for their number, their fulness, and the beauty of their illustrations. Especially are the large quarto memoirs published under the name of *Abhandlungen* remarkable in the latter respects. Consisting usually of complete monographs, sometimes purely palæontological, but more often blending stratigraphy with palæontology in a manner which is too seldom resorted to in this country, these handsome volumes are quite independent of, whilst they frequently illustrate, the maps issued by the same authority.

The present work forms vol. ix. of this important series. In many ways it is unlike its forerunners, but it resembles them in its completeness and in the finished character of its plates. Although eminently local in interest yet so many points are touched upon—or rather fully discussed—in Dr. Karrer's memoir that it appeals to the civil engineer, the hydrologist, the archæologist, and the chemist in almost as great a degree as to the geologist and the systematic palæontologist.

This great closely-printed book of more than four hundred pages, with its numerous tables and large folding plates, is strictly what its title implies, viz., an account of the geology exposed by the engineering works recently carried out in order to bring the waters of the Kaiserbrunnen and Stixtenstein springs to Vienna, a distance of some twelve Austrian or fifty-five English miles.

All the leading features of this section could probably have been described and commented on with apparent fulness in a short paper in the *Verhandlungen* of the Institute, but the aim of the author has been to raise the character of his memoir from that of a passing pamphlet to that of a thoroughly exhaustive record of all the facts—the seemingly unimportant as well as the obviously valuable—which could be brought within the natural limits of his subject. In this object he has perfectly succeeded, and the result is an orderly collection of minute stratigraphical and other details such as, we believe, have never before been brought together with reference to so small an area.

From Kaiserbrunnen at the foot of the Schneeberg and from Stixtenstein a little further north to the very streets of the Capital, or, geologically speaking, from the triassic heights of the Noric Alps to the drift and alluvium overlying the Tertiary beds of this Alpine portion of the Vienna Basin, only those valleys across which the aqueduct replaced the cutting and the tunnel were left unsearched and unplotted by Dr. Karrer. Every bed, band, thinning, thickening, fault, slip, or flexure cut through by the artificial channel was measured and noted by him, and all these details are laid down on a true scale (except in one unavoidable instance) in twelve carefully drawn and coloured plates of sections. So far, however laborious, the work done may be said to be more or less mechanical. This is not the case with regard to the clear sketch-sections or outline views which accompany the measured lines. In these we have exhibited to us the relations in which the rocks seen in the cuttings stand to those of the surrounding country, and we perceive at once the eye and hand of the field-geologist.

Since 1859 Dr. Felix Karrer's name has been constantly before the scientific world as that of an active member of Ritter von Hauer's brilliant geological staff. His researches have lain principally among the beds of the Vienna Basin and their fauna. In conjunction with Theodor Fuchs his papers on these and allied subjects have been both numerous and valuable; but more particularly has Dr. Karrer devoted himself to the study of the Foraminifera which these deposits yield in such abundance, and now it may be said that he fitly succeeds to the honourable place so long held by the late Dr. A. E. von Reuss as one of the leading Rhizopodists in Central Europe. Accordingly we find in the present work elaborate tabular lists of the Foraminifera found in the borings and elsewhere in the course of the engineering operations, and no less than seventy-one forms figured and described as new. With reference to these it will be sufficient to observe that many of them are such as, according to the views prevalent in England, would be scarcely held to warrant specific distinction.

The Alpine Vienna Basin, the margin of which between Gloggnitz and Vienna is the region where the geology has been specially worked out, was, it seems, dry land at the time when the Older Mediterranean Sea covered the Basin beyond the Alps. Consequently the Younger Mediterranean Series, its marine sands and gravels passing into grits and conglomerates with intercalated bands of Nullipore limestone and marls, are the lowest of the Tertiary deposits present here. To this series belongs the famous "Leythakalk," about which so much has been written. The fauna of these beds is closely allied to that of the Adriatic of the present day, whilst some of its species denote a somewhat warmer sea. Upon these newer Mediterranean strata rest the Sarmatian beds, in three divisions, the fossils of which allow us to infer a great cooling of the sea accompanied by an invasion of Asiatic cold-sea forms. This was followed by a period of brackish and then of fresh water, which brings us to the well-known Congeria beds, above which only two more members of the Tertiaries occur, viz.: the Belvedere beds and the purely local but highly-interesting freshwater limestone of Eichkogel, near Mödling, which formed the subject of Dr. Karrer's first contribution to science.

It will be readily understood that the works entailed by the construction of the watercourse promised unequalled opportunities for studying in detail the shore facies of these various deposits, and comparing them with the aspects they exhibit in other parts of the basin. That these opportunities have not been lost this memoir affords abundant proof.

From Stixtenstein and Kaiserbrunnen to Ternitz, where the two head-channels meet to form a single watercourse, the rocks cut through are of much greater age. Here we have carefully described by Dr. Karrer, although he does not profess to do so as minutely as his more congenial tertiaries, micaschist and *grauwacke* of uncertain age, and, in disturbed order, the Wetterstein, Guttenstein, and Werfen divisions of the Alpine trias. At Baden and again at Mödling, short spurs running like headlands into the ancient Viennese sea, once more bring the uppermost of these formations (the Wettersteinkalk) within the line of section.

At the former of the two last-mentioned places is a group of well-known thermal springs ranging from ordinary temperatures to 95° F. Several pages of considerable interest are taken up by the discussion by Prof. Eduard Suess of a large series of observations relative to these springs carried on by Prof. Jellinek. Their topographical distribution is peculiar and is strikingly shown on a map (Plate xiii.) by means of isothermal lines, the intervals being of 1° Réaumur from 8° to 13°, then one of 3° from 13° to 16°, and lastly, one from 16° to 28°: that is to say, the spaces between the lines of the first series represent 1° each, then 3°, and lastly 12° Réaumur. This mode of dealing with thermal phenomena by means of contour-lines is new to us and seems fruitful of good results. In the present instance five distinct foci of greatest heat are well made out, with several outlying ones attaining lesser degrees of temperature.

The chemical composition not only of the hot springs, but also of the various waters referred to throughout the book, is given in numerous analyses by chemists of note.

The line of the watercourse runs more or less parallel to the Southern Railway. In 1840, when the latter was in course of construction, several discoveries of prehistoric implements were made at Potschach, and elsewhere. It is therefore not surprising that the new excavations should have given rise to similar finds. Of these the most important appears to be an old burial-ground of the bronze age at Leobersdorf, a little to the south of Baden. Here bronze rings, daggers, armlets, &c., were found associated with fairly-preserved human remains. The former are described by Baron von Sacken, the Director of the Imperial Collection of Antiquities, whilst full details respecting the latter are furnished in an anthropological chapter by Friedrich Steller. Both are well illustrated by coloured plates and woodcuts.

The question may perhaps be reasonably asked, why so much labour and money have been expended on the particular subject chosen. But when we remember the losses that British geology has sustained by the neglect of so many invaluable sections temporarily exposed in the early canal and railway days and now covered up and lost for ever, we may well regret that no devoted geologist was there to preserve the minute records of the rocks and their disturbances in as accurate and painstaking a manner as Dr. Karrer has done in the case of the Austrian Watercourse. Given the opportunity of issuing a report on so complete a scale—an opportunity which we fear will never occur in England—no objection can be made to his mode of setting forth his results. A more condensed account would have been more readable, and probably more acceptable to foreign geologists, but among the local investigators in the district to the south of Vienna, which the author delights in calling that "*stückchen Erdrinde*," the book must at once take rank as a storehouse of actual facts never to be over appreciated.

The value of the memoir is much enhanced by the long bibliographical list with which Dr. Karrer opens the work, and which is brought up to date in the appendix. This list contains the titles of 566 books and papers relating to the region traversed by the *Aqueduct*, and arranged, as all such lists should be, in chronological order. The first paper cited is one by Wolfgang Anemarinus, on the Baden springs, and dates as far back as 1511.

From what we have said it will be seen that no labour has been spared to render this report as perfect as it could be made. One serious omission, however, must be called attention to. There is no index. The late Sir Roderick Murchison was wont to deplore that many of the details contained in his "big books" remained unknown and buried within them. But books like the "Silurian System" are certain to be consulted, index or no index. To publish a work so local in character, albeit so complete in its execution as the one under review, as Dr. Karrer has done, without a key to the endless facts it contains, is deliberately to court non-recognition.

Before concluding we would note the excellent geological map of Vienna and its immediate neighbourhood, by Th. Fuchs. This map was first issued in 1874, and is conveniently reproduced in the present memoir.

G. A. LEBOUR

A CENTURY OF DISCOVERY

The Discoveries of Prince Henry the Navigator, and their Results; being the Narrative of the Discovery by Sea, within One Century, of more than Half the World. By Richard Henry Major, F.S.A. Portraits, Maps, &c. (London: Sampson Low and Co., 1877.)

Geschichte des Zeitalters der Entdeckungen. Von Oscar Peschel. Zweite Auflage. (Stuttgart: J. G. Cotta, 1877.)

THESE two works practically refer to the same period, which nearly coincides with the fifteenth century, and deal mainly with the same events. Mr. Major's work centres round Prince Henry as the initiator of the remarkable series of discoveries which were made during the century referred to, while that of the late Oscar Peschel deals with these events as forming a remarkable era in geographical discovery, and is considerably more detailed than the work of Mr. Major. Both works are virtually second editions. In its present form Mr. Major's is somewhat more popular than when first published, the discussion of certain points interesting only to the student having been omitted; Peschel's work, first published about twenty years ago, is practically unaltered. Both works are valuable contributions to the history of one of the most eventful centuries of our era; Mr. Major's is a worthy record of the life and work of a noble-minded prince, while Peschel's is a standard authority on the geographical work of the fifteenth century.

Prince Henry, aptly styled "the Navigator," was the fifth child of King João I., of Portugal, and his Queen Philippa, daughter of "old John of Gaunt, time-honoured Lancaster," and was born in 1394. He was carefully trained by his English mother, and after having distinguished himself at Ceuta, took up his abode on the promontory of Sagres in Algarve, of which kingdom he was made governor in perpetuity. It was from here that during the rest of his life he initiated and directed those discoveries with which his name will be ever associated; to Prince Henry, there is no doubt, the rapid progress of geographical exploration during the fifteenth and sixteenth centuries is mainly due. But not only in this way did he encourage the advance of knowledge; by providing professorships, and in other ways, he did much to foster the progress of science such as it was in his time; his own favourite subjects of study were astronomy and mathematics.

It is with Africa that Prince Henry's name is chiefly associated. Before commencing his great work of exploration he took every means in his power of ascertaining all that was known about Africa, though that was not much. Cape Blanco he knew, though vaguely, but all the coast south of that was practically a blank. The interior was known much farther southwards, and not a few details of Timbuctoo had reached Europe by the beginning of the fourteenth century. It does not seem to be known whether Prince Henry had the means of making himself acquainted with the work done by the Phœnicians and Carthaginians; the narrative of Hanno's famous coasting voyage would have been a treasure to him, but the likelihood is that he was totally ignorant of the work accomplished by these pre-Christian explorers. Nor is it likely that he had heard of the Norse discovery of America, though he may have heard of the famous voyages of the brothers Zeni; if he had it does not seem to have suggested to him the existence of a great continent far beyond the horizon which bounded his outlook from Sagres. Prince Henry set about the work of African exploration with intelligence, his clear object apparently having been to trace the African coast to its southernmost limit, and even discover by rounding it a practical sea-route to India.

"Very few details are left us," Mr. Major writes, "of the astronomical instruments used in the time of Prince Henry. The altitude of a star was taken by the astrolabe and the quadrant by means of an alidade, or ruled index, having two holes pierced in its extremities, through which the ray passed. The quadrant hung vertically from a ring which was held in the hand. We do not know how these instruments were graduated, but it is to be presumed very roughly. The astrolabe, the compass, timepieces, and charts, were employed by sailors in the Mediterranean at the beginning of the fifteenth century. It is quite certain that the needle was used at sea before Prince Henry's time, for he himself speaks of it when urging on one of his navigators to the rounding of Cape Bojader." During the lifetime of Prince Henry the African voyagers stuck closely to the coast, except when by accident they were driven from it.

The Prince's enthusiasm and generosity drew to him most of the adventurous spirits of his time, and thus it was that after his settlement on Sagres scarcely a year passed that he did not send out one or more expeditions to carry on the great work which he had set himself to accomplish. The first fruit of Prince Henry's enterprise was the finding of the islands of Porto Santo and Madeira, in 1418–20, by two squires of his own household, who were driven thither by a storm off Cape St. Vincent. Mr. Major has, however, proved satisfactorily, we think, that the Madeira group were discovered about the end of the previous century by an adventurous Englishman named Robert Machin.

For long had Cape Bojader proved an obstacle which the Portuguese sailors sent out by the Prince attempted in vain to pass; Cap Nun had been passed, but the increasing violence of the waves that broke upon the dangerous northern bank of Cape Bojader proved too much for the cockle-shells in which Prince Henry's explorers were hardy enough to risk their lives. It was only in 1434 that Gil Eannes, a native of Lagos, managed to pass this fancied terrible obstacle to progress, by putting well out to sea. Next year another fifty leagues were added to the stretch of coast discovered, and thus year after year, league upon league was added, and specimens of the people and products brought home, the former to be Christianised and sent back to convert their brethren. By the time of Prince Henry's death in 1460, the west coast of Africa had been explored under his auspices as far south as the Rio Grande, the Canaries, Cape Verde Islands, and Madeira discovered or rediscovered, and a large amount of substantial information obtained about the people, the products, and the country far into the interior of Northern Africa.

Mr. Major justly designates Prince Henry the originator of continuous modern discovery, for Portuguese enterprise in this direction was not stopped by his death. It was not, however, till 1471 that the equinoctial line was crossed for the first time within the memory of man, probably by an explorer named Lopo Gonsalvez. The equator was not much surpassed till Diego Cam set out in 1484 and discovered the mouth of the Congo; the celebrated Martin Behaim, the inventor of the application of the astrolabe to navigation, was with Diego Cam in this eventful voyage. In his next voyage Diego got as far south as Cape Cross in 22° south latitude, where the cross he planted is still to be seen in almost complete preservation. In 1486 Bartholomew Diaz was sent out by King João, of Portugal, to carry out the discovery of the African coast, and, without knowing it, passed the southernmost part of Africa and came to anchor in what is now known as Flesh Bay, near Guaritz river, to the east of Cape Agulhas. He turned back after reaching the mouth of the Great Fish river, and it was on this return voyage that he discovered what he called Cape Tormentoso, but which King João on his return, "foreseeing the realisation of the long-coveted passage to India," named Cape of Good Hope. It was not till ten years after this that a practical test was made of the utility of this passage to India. Vasco da Gama left Lisbon with four vessels, the largest not exceeding 120 tons, in July, 1497, and coasted south the west coast, and north the east coast of Africa, as far as Melinda, to the north of Mombassa, which was reached in April of the following year. On April 20, 1498, he sailed for Calicut, before which he anchored on May 20, thus discovering the famous "Cape route" to India.

Such are a few of the results which are directly or indirectly due to the far-seeing enterprise and noble-mindedness of Prince Henry the navigator. But these are not all. But for his initiative in the beginning of the century, it is doubtful if America would have been discovered at the end of it, and had Prince Henry been alive when Columbus began his memorable agitation, that greatest of explorers would doubtless have been saved much humiliation and misery. Magellan's circumnavigations fall also within this most eventful of eras, and not far beyond it, Mr. Major has proved, the discovery of Australia. "The coasts of Africa visited, the Cape of Good Hope rounded, the New World disclosed, the sea-way to India, the Moluccas, and China laid open, the globe circumnavigated, and Australia discovered within one century of continuous and connected exploration," begun and to a great extent carried out by the prince the story of whose life Mr. Major has told so well. We can only again commend his work and that of Peschel to our readers as not only full of interest but of much valuable information.

OUR BOOK SHELF

Chemical Handicraft. A Classified and Descriptive Catalogue of Chemical Apparatus suitable for the performance of Class Experiments, Research, and Chemical Testing. Second Edition. By J. J. Griffin, F.C.S. (Published by the Author, Garrick Street.)

MR. GRIFFIN, the well-known manufacturer of scientific apparatus, earned the thanks of all students of science in this country by the publication of his first catalogue, now some eleven years ago, when the condition of things was much less far advanced than it is now. He has earned still greater thanks for his last edition, which is much more complete, more copiously illustrated, and more carefully brought up to the present needs of the student and the present possibilities of the maker. Those who noticed the many collections of such apparatus at South Kensington, last year, among which was one sent in by the Messrs. Griffin, cannot have failed to have been struck by the complication of the apparatus now required for chemical researches, and the skill, both in glass and brass, required to produce them. Mr. Griffin is evidently doing his best to uphold English manufactures against his continental rivals, and we wish him and his book every success. As the madman said of the dictionary, it is not light reading, and the plot is feeble; but, nevertheless, the book will be of use in every laboratory.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Cretaceous Flora of America

I AM extremely obliged to Dr. Newberry for pointing out in a very kind manner what is the actual state of our knowledge at the present time respecting the American cretaceous beds. Never having travelled in America, nor having had the honour of conversing with any of the American *savants* who have investigated the remains in these beds, I am scarcely in a position to discuss with them the value of the evidence on which they have been considered cretaceous. I have, however, endeavoured to make myself acquainted with the literature of the subject, and had read most of the works mentioned by Prof. Newberry in his letter to NATURE. I in no way dispute that dicotyledonous leaves have or may be found in cretaceous strata, but have, on the contrary, endeavoured partially to account for their absence in British cretaceous rocks. The age of the supposed American cretaceous beds appear to me, however, to be determined principally from the presence of Ammonites, Hamites, and other dibranchiate cephalopoda, and other types of mollusca as Inoceramus. Now what I intended to imply was that the presence of these is not conclusive evidence that the beds in question are as old as our chalk. Between our chalk and the base of our eocene a great hiatus exists, during which almost the whole of the cretaceous fauna became extinct, at least in European seas. This extinction and complete change of fauna implies an immense interval of time which, although we have but little record of it in Europe, we may expect to find recorded elsewhere.

It is at least possible that the series in question in America may be this record. In the lower, as in the Dakota group, we have, mixed with many decidedly (as we have been wont to consider them) cretaceous types of cephalopods, gastropods and bivalves of an eocene aspect. No comparative tables of fossils nor determinations of any value of European species from these beds have been made. But assuming that a portion of the lowermost cretaceous rocks of America were synchronous with some of our upper European cretaceous rocks, we may still suppose the mass of the strata to be of younger age. As very fairly stated by Dr. Newberry, Mr. Lesquereux does not agree with him as to where the division between cretaceous and eocene in the lignitic beds should be placed. Hayden[1] says that the age of the lignite strata is obscure. "The evidence points at the present time to the conclusion that the lower portions of this group are cretaceous, passing up by gradual transition into the tertiary, and that the greater portion may be regarded as of the age of the later period." Cope and Marsh, again, from the presence of Mososaurus, considered the lignites to be cretaceous. It seems to me that as the lignitic flora has the same character throughout its entire thickness, the formation must be considered as a whole, and that instead of endeavouring to correlate portions with either the European chalk or eocene, it would be simpler for American geologists, and more in accordance with the evidence we possess, to recognise the fact that the American series more or less represents the great hiatus existing in Europe between these formations. Although the upper portion of the lignites, the total thickness of which has been estimated at 10,000 feet, may be contemporaneous with a part of our eocene, the absence of any of the types of eocene flora, such as are characteristic at Sezanne, Bournemouth, &c., is opposed to the supposition, as much as the absence of anything at all approaching the Dakota flora in our cretaceous rocks is opposed to the contemporaneity of the latter.

These series may still, however, be conveniently spoken of relatively, and for the purposes of American geology, as cretaceous, but not until further evidence is adduced can they be recognised as synchronous with any portion of ours.

J. S. Gardner

Automatism

Mr. Spalding in his able review of "The Physical Basis of Mind," alludes to the term automatism, about which there has been so much controversy. The word, however, is a most unsuitable one for designating the important doctrine inculcated so clearly by Mr. Spalding, as well as by Huxley and Clifford. The ordinary meaning of "automaton" is a machine whose actions are unattended by feeling. Now as the most striking difference between an organic machine and an inorganic one is that the movements of the living machine are accompanied by sensations, while those of the inorganic machine or automaton are without concomitant sensations, it is plainly a mistake to apply to the actions of the sentient machine a term which has as a fundamental part of its meaning the absence of sentience. The incongruity is so manifest that I think it not improbable that it is one of the sources of the facile confidence displayed by some of the opponents of "automatism"; and if the word were supplanted by a less objectionable one, it is possible that the important doctrine intended to be designated by it might be accepted with less difficulty. I would suggest that some word meaning concomitant action or synchronous procedure might be coined for the purpose. The Germans, who are so fond of long, amalgamated expressions, would perhaps use something like "associated—mutually inconvertible—processes" to designate this dual unity of the subjective and objective sides of mental action.

D. Sharp

Thornhill, Aug. 5

Local Museums

In common with Mr. Allen, and doubtless very many others, I have read the articles and letters on Local Museums with a great deal of pleasure; and I am very glad that Mr. Allen has made his practical suggestion. In February last, in a paper which I read before the Hastings Philosophical Society on "Local Museums and Libraries of Reference," I made a similar suggestion for our own locality. You may think the matter of sufficient importance to justify the insertion of the following few lines from my paper:—

"I do not wish to put such institutions as I am advocating into competition with things of a very different character; but I would ask whether a zeal somewhat akin to that which is exhibited in raising funds for religious societies ought not to be exhibited on behalf of such purposes as those under discussion? Would it be at all absurd to talk about having a *mission* to establish a public library? . . . For my own part I can conceive of few nobler aims than that of raising for one's town a permanent public institution of an intellectual character. If a committee were to take up the work with enthusiasm and were able, even though after many years of toil, to say to the people of Hastings: 'We have built for you, with your own help, a library and museum, and we have, with funds with which the public have supplied us, sufficiently endowed this institution to carry on all its legitimate work, and we now hand it over to you, the people of the town of Hastings, as the property of you and your children for ever'—I say a committee that took in hand and accomplished such a work would deserve the deepest gratitude of the borough, and would have a right to claim to have accomplished a *mission* of no small importance."

A. R.

Hastings, August 3

July Shooting Stars

I observed 197 shooting-stars in July—nearly all of them between the 6th and 20th—in twenty-four hours of watching. The weather was generally very cloudy and stormy between the 13th and 23rd, or many more would have been seen. I looked usually towards the eastern sky, and from the considerable number of meteor paths registered, am enabled to give the following table of radiant points visible in that quarter during the period of my observations. The list may be considered very fairly complete and accurate, for the great majority of the meteors were well seen, and many of them had short courses evidently near their radiant centres:—

Radiant Point.			No. of Meteors.
Approx. Star.	R.A.	Dec.	
	°		
ρ Cassiopeiæ	349	+ 53	8
β Aquarii	317	− 11	13
χ Aquarii	336	− 7	5
ι Pegasi	333	+ 26	8
ζ Pegasi	338	+ 11	11
f Lacertæ	334	+ 43	7
σ Andromedæ	4	+ 35	21
θ Persei	36	+ 47	6
ε Cygni	313	+ 33	6
α Cygni	315	+ 48	8
ζ Cassiopeiæ	6	+ 53	11
δ Cygni	290	+ 43	9
α β Persei	47	+ 45	5
θ Antinöi	298	− 8	5
δ Ursæ Minoris	295	+ 85	6
π Herculis	258	+ 37	6
ο Draconis	280	+ 57	5
λ Antinöi	285	− 12	5
ι Andromedæ	350	+ 37	7

I have given the number of meteors conformable to each position, but this detail cannot be very precise, inasmuch as in several instances the path converged back on two radiants in the same line, and near together. In such cases it is often quite impossible to assign the true focus. Of the nineteen showers included in the list, sixteen of them come near the dates and places of radiants enumerated in Mr. R. P. Greg's catalogues.

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[1] "Geological Survey of Territories," 1872.

The shower at 290° + 43° is apparently new for the first half of July, and corroborates one I deduced from the Italian meteor catalogue (1872) at 291° + 45° for June 26 to July 11 from eight meteors. This position falls near an August shower (seen on the 10th in 1871 at 293 + 42), and it is very likely to be nothing but an early indication of that system. The radiants in Perseus at 47° + 45° and 36° + 47° are too far removed from any previously known centres to be included with them. They were very swift, white meteors, rather bright, leaving streaks, and I mistook them at first for early *Perseïds* of the true August system. The positions agree singularly well with two of Prof. Herschel's cometary radiants and dates (British Association report on luminous meteors, 1875, p. 233) as follows:—

	R.A.	Dec.	Date.
Comets radiant (1764 ☋)	49°	+ 45°·5	July 25.
Meteor radiant, 1877	47	+ 45	July 12-20.
Comets radiant (770 ☋)	39	+ 45	July 8.
Meteor radiant, 1877	36	+ 47	July.

I saw several meteors from these radiants in July last year, and on examining the shooting-star catalogues of Zezioli, Schiaparelli, Denza, and others, 1867-72, have found a few additional paths clearly confirming the existence of these new meteor orbits. From twenty-eight shooting stars (including my own observations) the two showers are apparently well marked and fall sufficiently near the cometary positions to afford an inference of connection.

The most active shower of the month was from Andromeda (4° + 35°). Twenty-one swift white meteors were noted from this place, but the radiant was a little diffuse and not nearly so sharply centred as another strong system at 6 + 53, which supplied very similar meteors. They are both already well-known showers. On the early morning of the 29th a few swift meteors with streaks, observed in Auriga, indicated the commencement of the August *Perseïds* with radiant near *η Persei.*

Bristol, July 31 W. F. Denning

OUR ASTRONOMICAL COLUMN

The Total Eclipse of the Moon, August 23.—Though lunar eclipses have lost the degree of astronomical interest and utility formerly attached to them, the general observer may still be expected to find the same amount of attraction as in past times in watching the physical features of a total eclipse of our satellite, well visible at a convenient hour of a summer's night. On Thursday evening, August 23, with favourable weather such a phenomenon may be witnessed, throughout its continuance, in this country as in other parts of Europe. The first contact with the penumbra takes place at 8h. 3·7m. Greenwich mean time, and that with the dark shadow at 9h. 13·7m., about 59° from the northernmost point of the moon's limb towards the east; the total phase begins at 10h. 19·1m., and ends at 3·7m. after midnight; the last contact with the shadow occurs at 1h. 9·1m. A.M. (August 24), about 112° from the northernmost point towards the west, and that with the penumbra at 2h. 19·1m. With respect to the earth's true shadow the eclipse, therefore, continues 3h. 45m., and the moon is totally immersed in it for 1h. 45m., the middle of the eclipse at 11h. 11m. P.M. When she first encounters the earth's dark shadow her altitude at Greenwich is 18°.

Though we are accustomed to speak of a total eclipse of the moon, as is well known, it rarely happens that she disappears while in the earth's shadow. The physical features of interest to which allusion is made above consist chiefly in the variations of the coppery and other tints which spread over her surface, and in the great majority of eclipses render her more or less conspicuously visible, during her passage through the shadow; and as these variable features depend upon the state of the atmosphere at the time round the edge of the earth's disc as seen from the moon, with respect to transparency or more or less prevalence of cloud therein—the aspect which a particular eclipse is likely to present does not admit of prediction. In the eclipse of June 15, 1620, Kepler states that the moon wholly disappeared, while stars of the fifth magnitude were visible in the neighbourhood, and Hevelius failed to see her even with a telescope during the eclipse of April 14, 1642 (not April 25, as stated in many astronomical treatises). But perhaps one of the most striking instances of the kind is afforded by the eclipse of May 18, 1761, observed by the Swedish astronomer Wargentin at Stockholm; eleven minutes after the total immersion he could not perceive the slightest trace of the moon either with the naked eye or with the telescope, yet the night was very clear and the stars shining in her vicinity; but about forty minutes later, with a two-foot telescope, he discovered our satellite by a faint light on the border of the disc. As an instance of the contrary nature, where the moon has been so strongly illuminated during her presence in the shadow, as to admit of the various markings upon her surface being seen with distinctness, and even to lead persons to doubt her being eclipsed, mention may be made of the eclipse on the morning of December 23, 1703, which was observed by various astronomers in the south of France. At Avignon, during the whole duration of the passage through the earth's shadow, "the moon appeared extraordinarily illuminated, and of a very bright red, so that it might have been supposed that she was transparent, and that the sun was behind her globe, and that his rays passed through in the same manner that they are seen to traverse certain stones, which are slightly diaphanous." It is singular, however, that while this was the aspect of the phenomenon at Avignon, different features should have been noted at Montpellier, particularly the total disappearance of the moon, rather quickly towards 6h. 30m. A.M., though the night was as transparent as could have been wished; it is mentioned that the twilight was already very sensible, but that the invisibility of the moon could not be wholly attributed to this cause, since many stars were shining in the same quarter of the sky. A later instance of the same kind occurred on March 19, 1848, recorded by observers in England, Ireland, and Belgium, when the moon's disc was intensely bright, coppery red. The uninitiated were doubtful of there being any eclipse. It is worthy of mention that conspicuous aurora borealis was present during the night.

The Variable Star *χ* Cygni.—Prof. Schönfeld's ephemeris fixes the next minimum of this star to September 15, the magnitude according to his last catalogue of elements being then 12·8; but the variation has been subject to considerable irregularity of late years, and observations will be required for some time before and after any dates now predicted to determine the epochs of maxima and minima satisfactorily. The error of Argelander's formula, with one perturbation, appears to have attained a maximum of about three months, in 1870, and to have been since diminishing; as compared with the maximum of 1874, the error was little over two months.

This star is properly designated *χ* Cygni, Bayer's letter undoubtedly applying to it. *χ* of Flamsteed must then take the number he attaches to it, 17. When the British astronomer looked for Bayer's star it would be, as Argelander has pointed out, invisible; and hence his mistake in connecting another star with Bayer's letter; there is no necessity, however, to perpetuate the obvious error.

New Minor Planet.—No. 173 of the group of small planets was discovered by M. Borrelly at the observatory of Marseilles on the evening of the 2nd inst. At 9h. 0m. m.t., its position was in R.A. 22h. 40m. 30s., N.P.D. 97° 34′·8; diurnal motion in R.A. 26s., in N.P.D., + 8′, a tenth magnitude. Though several small planets detected within the last ten years are adrift, it does not appear that the present body can be identical with any one of them. Ephemerides for 1877 of a number discovered since 152 are unavoidably omitted in the *Berliner Jahrbuch* for 1879, for want of the necessary elements. Dike, which was found by M. Borrelly as far back as May, 1868, has not been observed since.

BIOLOGICAL NOTES

Temperature of Trees.—Prof. Boehm has recently investigated the temperature of trees in its relation to external influences. His conclusions are these:—1. The temperature of the tree-interior is, during transpiration, the combined expression of the air and the ground heat. 2. The air heat is conducted transversally, the ground heat longitudinally. 3. The longitudinal conduction is effected through the ascending sap-current, or rather through transpiration. 4. A lowering of the ground temperature during transpiration produces also a depression of temperature in the tree-interior. 5. The influence of the temperature of the ascending sap-current decreases in the stem from below upwards, and from within outwards. 6. The amount of this decrease is determined by the amount of the transversely-conducted solar heat, and is in direct ratio with the diminution of the volume of the stem part, and the approximation to the periphery of the stem. 7. The lower part of the stem is still under the full influence of the ground heat, or rather of the ascending sap-current. 8. The vertical limit of this influence is lost in the ramification of the tree. 9. With exclusion of transpiration, and therewith of rise of sap, the temperature of the tree is simply dependent on that of the air. 10. A simultaneous cooling of the lower and upper part of the tree completely equalises the amounts of influence (opposite according to the height of the stem) of the two cooling "moments."

Laticiferous Vessels in Plants.—We notice a very interesting Russian paper, by M. Schmahlhausen, just appeared, "Researches on the Vessels of Plants." The author shows that the growth of the vessels goes on in the same manner as that of the mycelium of parasitic Fungi in the tissues of plants, and thus refutes the often expressed opinion that vessels in plants are analogous to the blood-vessels in animals.

Flora of New Guinea.—Baron Ferdinand von Müller's fifth contribution towards a list of Papuan plants has recently reached us, and contains the remainder of the species, with few exceptions, gathered last year by Signor D'Albertis and Mr. Goldie, on their now famed New Guinea exploration. In this list several new species occur, notably a *Sloanea*, which Baron Müller has named *S. paradisearum*, from the fact that the tree, which grows to the height of forty feet on the Upper Fly River, inhabits the forest haunts of the birds of Paradise. The fruits are described as closely approaching in size those of *S. jamaicensis*, "thus far excelling any of the Sloaneas of the eastern hemisphere, so far as they are known, in the magnitude of the fruit." The discovery of *Nageia Rumphii* is interesting, from the fact that no other conifer is reported from New Guinea, except *Nageia theveteæfolia*, and an Araucaria by Beccari.

The Segmentation of the Head.—By slow degrees an approach is being made to a true understanding of this most difficult and interesting question. The old explanations by archetypes and by the structure of the highest-developed skulls, have fallen into disfavour. Attempts to settle the cranial segments by considering the distribution of nerves in the adult have been shown to be unsafe, because nerves are necessarily adaptational in their character and liable to the greatest modification on changes taking place in the organs they supply. The development of nerves, however, is a much surer guide, showing primitive and fundamental characters. The nerves behind the ear are five in fishes, although the number of strands of which the vagus is made up in some cases points to a loss of distinct nerves and segments in the hinder part of the head. The auditory and facial nerves originate as one, so that the auditory appears as a specialised portion of the facial. The trigeminal likewise arises as a single nerve, and in front of this there is no nerve having a similar history to these and the spinal nerves. Thus we have an indication of seven segmental nerves issuing from the brain-case. When the visceral clefts are considered, we find in sharks six clefts indicating seven segments, or one more if the mouth be regarded as a cleft. The head-cavities between the outer wall of the head and the mucous membrane of the throat, discovered by Mr. Balfour in sharks, furnish a similar number. They are eight in all, one premandibular, one mandibular, one hyoid, and five branchial. Thus the examination of three sets of organs leads to the assignment of eight body segments to the head. But the question is far from being settled so long as the brain-case itself and the brain cannot be satisfactorily explained.

The Capercailzie in Northumberland.—It is well known that this fine bird, originally indigenous in the British Islands, became extinct, and was reintroduced into the Scottish Highlands some forty years ago by the late Marquis of Breadalbane. Earl Ravensworth has recently been endeavouring to naturalise the capercailzie in large tracts of pine wood in Northumberland, on the edge of moors and wild crags, furnishing various berries which form its favourite food. In 1872 a cock and two hens were reared; but the male bird got destroyed. In 1873 two settings of eggs were hatched, but owing to a wet summer all the young birds perished after nearly arriving at maturity. In 1874 four fine birds were reared to their full growth, one of which, a male, still survives. In 1876 fifteen chicks were hatched out of twenty eggs, and three cocks and four hens grew to maturity. But the stock has become reduced to five individuals, three males and two females, all in good health. It appears that a difficulty arises from some deficiency in diet or conditions which is at present unknown. Although extremely wild and shy by nature, and flying long distances, capercailzies are yet most indolent, and unwilling to move from places to which they are familiarised. Their colour assimilates very closely with the Scotch fir, so that it is exceedingly difficult to distinguish the male bird when seated on a branch. The male is very ferocious, and makes extraordinary gesticulations during the season of courtship; the hen may even be killed by his fierce advances.

Evolution by Leaps.—Mr. Thomas Meehan has described before the Academy of Natural Sciences of Philadelphia a case of sudden change of characters in some branches of a "smoke-house" apple tree, which bore clusters of flowers at the ends of young shoots, flowering six weeks after the ordinary blooms from spurs, and yet maturing fruit at the same time as the old spurs. This fruit, however, was very unlike the smoke-house fruit, the fruit stems being long and slender, and the fruit flattened. The change was so great that a botanist would have no hesitation in describing the form as a new species; and there appeared no reason why the law which produced this modification might not simultaneously act on all the trees in a district. At any rate here was an appearance which served to show how new species might arise in nature. Mr. Meehan, however, did not allude to the difficulty of reasoning from a change in a cultivated variety to the operation of causes in a wild state. The case can hardly be considered as decisive, although of much interest.

The Eucalyptus in the United States.—Mr. Joseph Wharton has tried to acclimatise *E. globulus* in Philadelphia, but although the plants grow well in greenhouses, they seem incapable of surviving the severe winters, even though carefully covered with leaves and earth. The winter test was only applied after the plants had grown vigorously for some years, being protected in winter and placed in the open air in summer.

EVOLUTION OF NERVES AND NERVO-SYSTEMS[1]

III.

THE question, however, remains: Will this conductile function prove itself as tolerant towards section of the tissue as the contractile function has already proved itself to be? for, if so, any objection to the view that the passage of the *contractile* waves is due to vicarious action of rudimentary nerve-fibres will be removed. Briefly, the answer to this question is an affirmative; for I find it is quite as difficult to block the passage of stimulus waves by means of interposing cuts, as we have seen that it is to block the passage of contractile waves by the same means. For instance, here is an *Aurelia* (Fig. 5), the bell of which has been cut into the form of a continuous parallelogram of tissue, and then submitted to the tremendously severe form of section which is depicted. Yet on very gently stimulating any point in this expanse of tissue, as at the end *a*, a tentacular wave would course all the way along the margin, to *b*, thus showing that the wave of stimulation must have passed round and round the ends of all the intervening cuts. In the diagram the tentacular wave is represented as having traversed one-half of the whole distance from *a* to *b*, and near *b* there is represented a single remaining ganglion, (*g*). When, therefore, the tentacular wave reaches *g*, this ganglion will shortly afterwards discharge, so giving rise to a contractile wave, which will course back from *g* to *a* in the opposite direction to that which the stimulus wave had previously pursued.

And this, I am not afraid to say, is the most important observation, both to the physiologist and to the evolutionist, that has ever been made in the whole range of invertebrate physiology. For to the physiologist this observation proves that the distinguishing function of nerve, where it first appears upon the scene of life, is a function which admits of being performed vicariously, to almost any extent, by all parts of the same tissue-mass; while to the evolutionist the observation proves the existence of such a state of things as his theory of nervogenesis would lead him to expect. In such a symmetrically-formed animal as a Medusa, with all parts of the contractile sheet precisely resembling one another, we should expect the lines of discharge composing the hypothetical plexus to be very numerous, and all very much alike with respect to the degree of their evolution. For, as the symmetrical form of the disk does not require that any one set of lines should be used much more frequently than any other set, it follows from Mr. Spencer's theory that all the lines should more or less resemble one another as regards the extent of their differentiation.[2] That is to say, they should all be lines presenting about the same degree of resistance to the passage of a stimulus wave, and therefore it should become a matter of indifference, so to speak, through which particular set of lines such a wave takes its course.

There is still another class of facts which to my mind makes very strongly in favour of Mr. Spencer's theory. Assuming, as I think we are now entitled to assume, that the contractile waves are not merely muscle waves, but depend for their passage on the progressive passage of the stimulus waves—assuming this, the following facts become facts of great significance. When the contractile waves in a spiral strip have become suddenly blocked by section, in the great majority of cases, such blocking will be permanent—even though the strip be continously stimulated, whether artificially or by a single terminal ganglion, as represented in Fig. 4. But in the remaining cases, after a time that varies from a few minutes to a day or more, the obstruction is overcome, and the contractile waves pass forward with perfect freedom. Now, if I had time, I could prove that these facts are certainly not to be attributed to what physiologists term *shock*; and, therefore, it seems to me that only one hypothesis remains. What I have recently said about most of the lines of discharge in the supposed plexus being very much alike as regards the degree of their differentiation, does not, of course, mean that all the lines are *exactly* alike in this respect; for on *à priori* grounds such a state of things would be in the last degree improbable. Consequently, in conducting a spiral section, it must happen that at every snip the scissors cut through a number of lines of discharge presenting various degrees of differentiation; and, such being the case, the fact of the sudden and final blocking is presumably due to a well-differentiated line having been severed in a part of the tissue where no other line occurs of a sufficient degree of differentiation to conduct the stimulus forward. Now in most instances, as we should expect, the blocking so caused is permanent; for it is manifest that the formation of nervous channels, in the way suggested by Mr. Spencer, cannot proceed at so great a rate as to admit of *wholly* new lines of discharge being established during the life-time of a mutilated Medusa, *i.e.*, during the course of a few days. Nevertheless, according to the hypothesis, some small percentage of cases might be expected to occur in which such blocking of the contractile waves would only be temporary. For some cases would almost certainly occur in which the relations of the highly differentiated line just destroyed to the more slightly differentiated lines in the neighbourhood of the section, would happen to be such that the more slightly differentiated lines would be very nearly, though not quite, able to act vicariously for the more highly differentiated line which has just been destroyed (see Fig. 4, where the deep line represents the well-differentiated line which has just been severed, and the dotted line the less-differentiated one which is still intact). The contractile waves, therefore, would in the first instance become suddenly blocked at the end of the strip. But the molecular, and with them the contractile, waves still continuing to pass quite up to the end of the strip, and being there always suddenly stopped, a rude conflict of molecular forces will thus set up in the area where these waves are impeded, and each of the forces concerned will seek for itself the line of least resistance. Hence, as the successive waves beat rhythmically on the area of obstruction, more or less of the molecular disturbance must every time be equalised through those lines of discharge which from the first have been almost sufficient to maintain the physiological continuity of the tissue. Therefore, according to the hypothesis, every wave that is blocked imposes on these particular lines of discharge a much higher degree of functional activity than they were ever before required to exercise; and this greater activity causing in its turn greater permeability, a point will sooner or later arrive at which these lines of discharge from having been *almost* become *quite* able to draft off sufficient molecular motion, or stimulating influence, to carry on the contractile waves beyond the area of previous blocking. In such instances, of course, we should expect to find, what I always observed to be the case, viz., that the first contractile waves which pass the barriers are only very feeble, the next stronger, the next still stronger, and so on, according as the new passage becomes more and more permeable by use; until at last the contractile waves pour over the original barrier without any perceptible diminution of their force. In some cases, by exploring with graduated stimuli and needle-point terminals, I was able to ascertain the precise line through which this eruption of stimulating influence had taken place; so that altogether I think these facts tend very strongly to confirm Mr. Spencer's theory regarding the genesis of nerves.[1] I will only add that if this interpretation of the facts is correct, we have in them a striking instance of the uniformity with which Nature works. A scientific theory concerning the evolution of nerves, which a year or two ago it seemed impossible to verify, from the fact that it seemed as though the observations which would be required to verify it would need to extend over thousands of years—this theory is now, I believe, being verified by observations which need only extend over hours and minutes. The immensely protracted history of nervo-genesis upon this planet is thus probably reproduced in a greatly foreshortened manner in the facts which I have explained; and inconceivable as is the difference between these two histories of nervo-genesis in respect of their duration, it is nevertheless most probably in respect of their duration alone that these two histories differ.

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[1] Abstract of a Lecture delivered at the Royal Institution on Friday evening, May 25, 1877. By George J. Romanes, M.A., F.L.S., &c. Continued from p. 271.

[2] Mr. Spencer himself observes, "The average equality of the forces to which their bodies (*i.e.*, those of the Medusæ) are exposed all round is unfavourable to the formation of distinct muscles and a distinct nervous system" ("Psychology," vol. i. p. 522) Although this statement must now be modified so far as the ganglionic system of the Medusæ is concerned, I do not think that the anticipation which it embodies should on this account be deemed unwarrantable so far as it applies to other parts of the nervous system. For although it is true that a Medusa *as a whole* is "exposed all round" to an "average equality of forces," it is not true that the *excitable* portions of a Medusa are thus equally exposed. On the contrary, the margin of the excitable sheet which lines the cavity of the bell, occupies a much more exposed position than does any other part of that sheet; and whether or not this fact has anything to do with the development of the ganglia in the only part of the excitable sheet which is thus peculiarly situated, I think it is obvious that this part of a Medusa ought to be carefully excepted in the statement which I have quoted. With regard to all other parts of the excitable sheet, however, the statement is certainly correct; and it is only to such parts that the considerations in the text apply.—G. J. R.

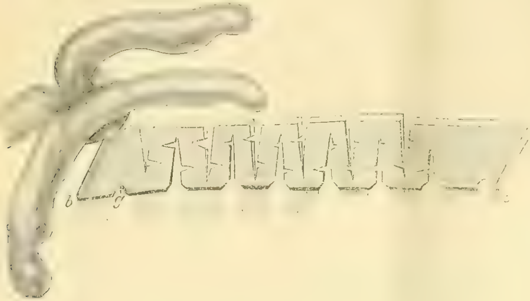

FIG 5.

I will now invite your attention to another species of Medusa, which is of a somewhat more highly evolved type than *Aurelia*, and which I have called *Tiaropsis indicans* (Fig. 7), in allusion to a highly interesting and important function which is displayed by its polypite. This function consists in that organ localising, with the utmost precision, any point of stimulation situated in the bell. For instance, if the bell be pricked with a needle at this point (*a*), the polypite immediately moves over and touches that point, as represented in the diagram. If immediately afterwards any other part of the bell be pricked, the polypite moves over to that part, and so on. Now this, you will perceive, is a highly remarkable function; for it proves that all parts of the bell must be pervaded by lines of discharge, every one of which is capable of conveying a separate stimulus to the polypite, and so of enabling the polypite always to determine which of the whole multitude is being stimulated. This localising function of the polypite, therefore, shows that the lines of discharge must be more differentiated in this species than they are in *Aurelia;* for it shows that vicarious action cannot be possible among them in so high a degree: every line of discharge must here have acquired a more specialised character, in order that the message which it conveys to the polypite when itself directly stimulated may not be confused with that which is conveyed by any other line.

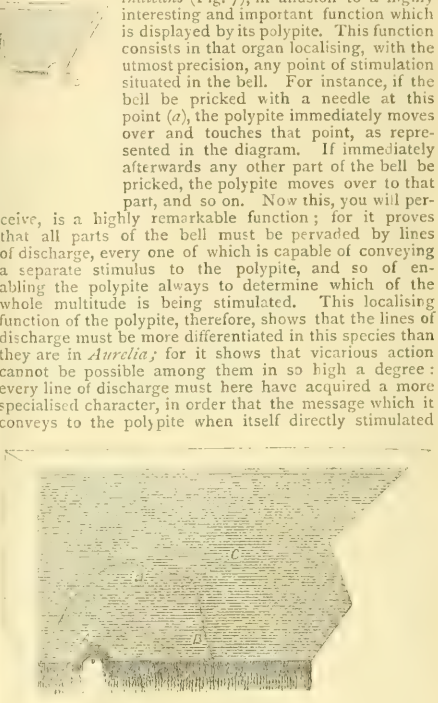

FIG. 6

Now it is easy to be wise after the event; but the state of things we here observe is just such a state of things as I think we should expect to constitute the next stage of nervo-evolution. It is no doubt a benefit to this Medusa that its polypite is able to localise a seat of stimulation in the bell; for the end of the polypite is provided with a stinging apparatus, and is besides the mouth of the animal. Consequently, when any living object touches the bell—whether it be an enemy or a creature serving as prey—it must alike be an advantage to the Medusa that its polypite is able to move over quickly to the right spot, in the one case to sting away the enemy, and in the other to capture the prey. Hence I think that natural selection would probably tend to convert lines of discharge in promiscuous directions, into lines of discharge in definite directions—thus developing the function of localisation. At first, no doubt, this function would be performed only in a general and tentative manner (as, indeed, I have observed in the case of *Aurelia*); but gradually by the combined action and mutual reaction of use and survival of the fittest, this function would come to be performed with ever-increasing precision.[1]

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[1] As additional proof that a wave of stimulation may pass over a barrier of the kind described in too small a quantity to start a wave of contraction beyond the barrier, I may mention the following facts:—In *Aurelia* the polypite is more sensitive to stimulation of the bell than is the bell-tissue itself; so that it is possible to stimulate the bell-tissue too gently to start a contractile wave in it, and yet strongly enough to cause writhing motions of response on the part of the polypite. Now, if by means of a spiral section of the bell the contractile waves have become blocked in the ribbon-shaped strip, it is sometimes possible, by strongly stimulating this strip, to cause the writhing motions of response on the part of the polypite—thus showing that although the contractile waves are blocked by the spiral section, the stimulus waves are able to pass forward with a strength sufficient to cause response in the polypite. I may here add that this fact of the contractile waves being sometimes wholly blocked by section before the stimulus waves are so, would appear to exclude, in the case of the Medusæ at all events, Kleinenberg's view as to the functions of primitive nerve and muscle being blended in the same tissue-elements. (See his work on *Hydra*.) I may also mention that in some cases I have observed that the establishing of a new line of physiological connection is a more gradual process than stated in the text. To show this, I may briefly quote one very instructive case. Seven marginal bodies having been removed, the eighth one continued to originate contractile waves, which coursed round the swimming-bell as usual. I now made a radial cut half an inch on one side of the marginal body, and extending to the centre of the swimming-bell. The contractile waves were immediately blocked—thus showing, as did a somewhat similar experiment detailed in my first Royal Society paper (p. 293), "that the influence of the marginal body had previously been communicated to the swimming-bell *from one side only*." But in the case we are now considering, the discharges of the marginal body were still rendered apparent by very local contractions of a tissue area in the immediate vicinity of that body—the area, namely, which in the figure (Fig. 6) representing one end of the strip is marked B B. Exploration by stimulus now showed that general contractile waves could only be started outside the area B B. In somewhat more than half an hour after the operation (during which time the area B B continued to contract rhythmically), the ganglionic influence for the first time extended from the area B B to the rest of the strip—the contraction being therefore general. After this first eruption of contractile influence, there succeeded a period of about a minute, during which the area B B continued to contract independently as before. Then another eruption took place, followed by another period of restricted contraction, and so on. Next, these general contractions became progressively more and more frequent, and as the rhythm always continued the same, whether the contractions were local or general, the number of the latter became increased at the expense of that of the former. Thus, while at first there were twenty or thirty local contractions between every two general contractions, this proportion gradually fell to fifteen, ten, five, &c., till the numbers became equal, after which the balance began to incline in favour of the general contractions Eventually the local contractions ceased altogether, and on now excising the marginal body and exploring by stimulus, I was able to localise very precisely the line through which physiological continuity had been established between B B and the rest of the contractile strip. This line was A C, as shown by the fact that while stimulation of any other part of the area B B was followed only by a local contraction at that area, stimulation of the line A C was always followed by a general contraction.—G. J. R.

This, then, I conceive to be an important step in the evolution of nervous systems—foreshadowing as it does the principle of co-ordination among muscular movements, which in all the higher animals is effected by reflex mechanisms precisely resembling, as to their function, the primitive reflex mechanism we are considering. But now another point of interest arises. As Spencer's theory supposes a line of discharge to become more and more definite by use, if, for the maintenance of any particular function such as the one we are considering, a certain line of discharge habitually serves as a line of communication between two points of the animal tissues; it follows that this line will offer less resistance to the passage of a stimulus between these two points than would any other line in the organism. Consequently, so long as such a line remains intact, so long we should expect what we have seen to be the case, viz., that little or no vicarious action takes place between it and other lines. But let this line be severed, and let there be a number of closely adjacent lines, as there must be in this particular instance, and should we not expect, both from Spencer's theory and from our knowledge of *Aurelia*, that at some such grade of nervous evolution as *Tiaropsis* presents, the stimulus should be able to *escape* from the severed to the unsevered lines? And this I find to be the case. For if a small cut be introduced between the base of the polypite and the seat of injury in the bell, the polypite is no longer able to *localise* the seat of injury, although it still continues to perceive, so to speak, that injury is being applied *somewhere*. For instance, if a short cut be introduced as here represented at *b c*, and you prick the bell anywhere below the cut, as at *d*, the polypite, instead of immediately applying its extremity to the exact spot that is being stimulated, now actively dodges about first to one part and then to another part of the bell, as if seeking in vain for the offending body, which, however, it cannot succeed in finding. Now I explain this marked change in the behaviour of the polypite by supposing that the wave of stimulation in this case runs along the habitual line of discharge till it reaches the cut; and that being there no longer able to pursue this habitual line of least resistance, the wave of stimulation escapes into the adjacent lines, and so spreads all over the bell. Hence a number of conflicting messages are simultaneously delivered to the polypite, which therefore executes the random movements I have described—each of these movements being presumably determined by the relative degree in which now one line and now another takes part in conveying the scattered stimulus.

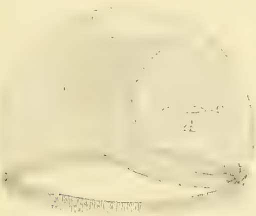

FIG. 7.—*Tiaropsis indicans*, slightly enlarged.

And now for another expectation to be realised. We should expect that the higher degree of specialisation which in these lines of discharge prevents vicarious action so long as the lines are undivided, should have the effect of rendering such vicarious action as we have seen to ensue when the lines are divided, less easy than it is in *Aurelia*, where the specialisation of the lines being less pronounced, vicarious action among them is presumably more habitual. And such I find to be the case; for while in *Aurelia*, as we have seen, stimulus-waves continue to zig-zag round and round the ends of almost any number of overlapping cuts, in *Tiaropsis* two or three such cuts are sufficient to destroy, not only the localising, but also the random movements of the polypite—the latter then remaining passive, because the stimulus-waves are wholly blocked.

And lastly, before leaving the case of *Tiaropsis indicans*, I should like to mention the noteworthy fact, that although the polypite is able to perform the intricate ganglionic *function* of localising any seat of stimulation in the bell, no signs of ganglionic *structure* can be detected with the microscope. Moreover, a portion of any size that is removed from the polypite continues to perform the localising function in just the same way as does the entire organ. In other words, this localising function, which is so very efficiently performed by the polypite of this Medusa, and which, if anything resembling it occurred in the higher animals, would certainly have definite ganglia for its structural correlative, is here shared equally by all parts of the exceedingly tenuous excitable tissue that forms the outer surface of the organ. The case of the incipient ganglia of the polypite thus resembles that of the incipient nerves of the bell in this respect—that in both cases obvious signs of characteristic function are displayed before any corresponding signs of structure can be distinguished. Nerve-cells, therefore, no less than nerve-fibres, are thus shown to have their first beginnings in differentiations of protoplasmic substance which are too refined for the microscope to analyse.

There is one other species of Medusa about which I should like to say a very few words, because it presents a still higher grade of nervous evolution than *Tiaropsis*. This is *Sarsia* (Fig. 8), a Medusa in which the lines of discharge have in some places become so far differentiated as to admit of being actually seen, and are therefore entitled to be called nerves. All round the margin, and likewise along the course of the radial tubes, these, the earliest visible nerve-fibres in the animal kingdom, may be traced. And as we might anticipate, the advance of structure which is implied by an invisible "line of discharge" becoming a visible nerve-fibre, entails a corresponding advance of function. In the first place, the rate at which a stimulus travels seems to be much greater along these fully-evolved nerve-fibres than it is in the more rudimentary nerves or lines of discharge in *Aurelia*. In the next place, this greater differentiation of nerve-tissue renders the nervous connection between any two parts of the organism much more definite, and therefore vicarious action less promiscuous, than we have seen it to be in the other jelly-fishes; so that, for instance, a tentacular wave in this species may be blocked by a single short cut through the margin of the bell. Lastly, it is in this species that I was first able to perceive any unequivocal evidence of co-ordination among the marginal ganglia. In all the other species of Medusæ the marginal ganglia appear to act independently of one another; but in this species, where the marginal ganglia are first seen to be united by a visible nerve-fibre, they always act in concert. So much, indeed, is this the case, that the animal is able to steer itself in any required direction, as proved by the experiment which

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[1] It may be here observed that Mr. Spencer, in his theory of nervo-genesis, expressly supplements his hypothesis as to the direct influence of use, with that as to the indirect influence of natural selection. (See "Biology," § 164.)—G. J. R.

I described last year, whereby individuals of this species were shown to have the power of following a moving beam of light round and round the vessel in which they were contained. I may also remark that individuals of this species present much more nervous energy than those of any other species of Medusæ which I have had the opportunity of observing.

I have now, ladies and gentlemen, communicated some of the points wherein my work has tended to elucidate the early stages in the evolution of nerves and nervous systems. And these are just the stages concerning which elucidation is most required. When once nerve-fibres and nerve-cells have been fully evolved and arranged in the form of simple reflex mechanisms, the subsequent history of their evolution into compound nervous systems is readily intelligible. The principles on which this higher evolution is effected are throughout the same, and result essentially in establishing ever more and more advanced degrees of integration. Compare, for instance, the nervous systems of an earth-worm, a centipede, an insect, and a spider; and observe the progressive fusion of ganglia which has taken place. The progressive centralisation which is thus effected is no doubt ultimately due to natural selection, if not exclusively, at any rate in large part; for this increasing consolidation of the reflex mechanisms must be of great benefit to the organisms which present it—serving as it does to render possible muscular movements ever more and more varied and combined. In the vertebrated series of animals the evolution of central nervous matter consists chiefly in adding to the size of ganglia by increasing the number of their ultimate nervous elements, nerve-cells and nerve-fibres. This progressive increase in the size of ganglia is especially remarkable in the case of the cerebral hemispheres. Now the cerebral hemispheres are the ganglia which we know to be the exclusive seat of the intellectual faculties; and their progressive increase in bulk as we ascend through the animal series, is undoubtedly to be regarded as the structural correlative of that progressive advance of the intellectual powers which is so conspicuously apparent as we ascend from the lower animals to Man.

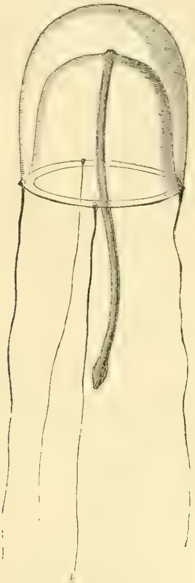

FIG. 8.—*Sarsia tubulosa*, × three times.

And now, in conclusion, I should like to observe, that even in this the highest product of nervous evolution—the supreme ganglia or cerebral hemispheres of Man—not only do we still encounter the same fundamental constituents of structure as we observe in all other ganglia; but the cells and fibres in the brain of a man do not differ in any marked degree from the cells and fibres in the ganglion of an *Aurelia*. There is, however, a prodigious difference in the product of their operation. When ordinary ganglion cells discharge their influence, the result is, as we have seen, a muscular contraction; but when cerebral cells discharge their influence, we of to-day can have no doubt that the result is a mental change. And although we freely acknowledge that we are here standing on the border-land of insoluble mystery, we are not afraid to assert with confidence, that in the amazing compliexty of the brain's structure—amid those millions on millions of interlacing cells and fibres—we have the physical aspect of all those relations, which in their psychical aspect we know as thoughts and feelings. Do you think that this sounds like materialism? I am not here to-night to discuss that point; but I may observe in passing, that even were I able to tell you the particular cerebral elements which I now use in expressing this statement to you, I should be just as much or just as little on the way towards proving materialism, as I am when I tell you that a blow on the head produces insensibility. Science can never go further than common sense in proving any *necessary* connection to subsist between mind and matter; for all that science can ever do is to ascertain numerous details with regard to such connection as undoubtedly does exist, and which, as a matter of daily experience, common-sense has already and completely recognised. However, materialism or no materialism, it is manifest that the facts being what they are, Mr. Spencer's theory as to the genesis of nerves must not be allowed to stop short just where its presence is most required. As we have seen that the cerebral hemispheres of man resemble all other ganglia in structure, we cannot hesitate in concluding that if Mr. Spencer's theory is valid in explaining the genesis of nerves in general, it can be no less valid in explaining the genesis of these supreme ganglia in particular. And as we have every reason to believe that the functional operations of these supreme ganglia are inseparably associated with our thoughts and feelings, we are driven to the yet further conclusion, that if Mr. Spencer's theory is of any validity at all, our possible as well as our actual thoughts and feelings are determined by the strictly physical conditions under which molecular waves of stimulation course through the structure of the brain. So that in this Spencerian hypothesis of lines of discharge becoming more and more definite by use, we have a physical explanation, which is perhaps as full and as complete as such an explanation can ever be, of the genesis of mind. From the time that intelligence first dawned upon the scene of life, whenever a new relation had to be established in the region of mind, it could only be so established in virtue of some new line of discharge being excavated through the substance of the brain. The more often this relation had to be repeated in the mind, the more often would this discharge require to take place in the brain, and so the more easy would every repetition of the process become; until at last the line of discharge grows into a nerve-fibre, and becomes the inherited property of the race. Thus it is, according to the theory, that there is always a precise proportion between the constancy with which any relations have been joined together during the history of intelligence, and the difficulty which intelligence now experiences in trying to conceive of such relations as disjoined. Thus it is that, even during the history of an individual intelligence, "practice makes perfect," by frequently repeating the needful stimulations along the same lines of cerebral discharge—so rendering the latter even more and more permeable by use. Thus it is that a child learns its lessons by frequently repeating them; and thus it is that all our knowledge is accumulated. In a word, if, as has been truly said, "man is a bundle of habits," we have in Mr. Spencer's theory of nervo-genesis a physical explanation of the fact. And forasmuch as it is upon this theory that Mr. Spencer may be said to found that great monument of modern thought—his "Principles of Psychology," I cannot but feel that one of the most important bearings which my work on the Medusæ has had, is that of supplying facts which tend to substantiate this theory—and this at a time when it seemed as though the theory could never have other than *à priori* considerations for its support. But if my interpretation of these facts is correct, this important theory is now receiving inductive verification from a most unexpected source. At first sight no two organic structures could well seem to have less in common than the swimming-bell of a Medusa and the brain of a Man; nor could anything seem more unlikely than that a great psychological theory should derive support from the study of polypes, where the very existence of a nervous system has only just been discovered. But here again, I believe, we may discern the uniformity of Nature; and while watching the passage of the waves of stimulation in the contractile strips of *Aurelia*—now passing freely, now stopped by an excess of resistance, and now again forcing a passage,—I have felt that I was probably witnessing, on the lowest plain of nervo-genesis, that very same play and counter-play of forces, which, on the highest plain of nervo-genesis, invariably accompanies, if it does not actually cause, the most intricate reasoning of a Newton, the most sublime emotion of a Shakespeare, the most imperious will of a Napoleon, and the most transforming thought of a Darwin.[1]

ATOMS AND EQUIVALENTS

IN the *Comptes Rendus* for the month of May and June there is a series of communications by Messrs. Wurtz and Berthelot containing a discussion of their respective views as to whether chemical changes should be expressed by elements in equivalent proportions or whether the more modern system of atomic weights should be employed.

In the first communication, which is made by M. Wurtz, he remarks on the discrepancy evidently existing between his idea and that of M. Deville, "who has criticised him in a former number of the journal," on the law of volumes of Gay-Lussac. He advances in support of the atomic argument, that free hydrogen may be regarded as a combination of two atoms of hydrogen, the peculiar reaction of hydrochloric acid on hydride of copper and in the case of oxygen with oxygen the reactions discovered by Thenard and Brodie of peroxide of hydrogen on certain oxides. He maintains that the molecular conceptions with regard to bodies in the free state are further upheld, in the case of nitrogen by the formation of nitro and dinitro compounds, and in the case of carbon by the consideration of organic chemistry when examined according to the theory of Kekulé of the grouping of several carbon atoms in the same molecule. After discussing the law of Gay-Lussac as applied to the gaseous compounds of hydrogen with chlorine, oxygen, and nitrogen, he remarks that what results from the previous discussions on this matter, is, that the system of expressing chemical reactions by equivalents which prevailed about 1840 over the atomic notation of Berzelius, has not taken into proper account the discoveries of Gay-Lussac on the combination of gases with each other; and consequently, that the maintenance of this principle in the discussion of chemical phenomena would cause a serious obstacle to the advancement of the science.

M. Berthelot on behalf of those who, like himself, retain the method of writing chemical changes by equivalents, as opposed to the atomic notation, in replying to this first communication of M. Wurtz, states that he does not think the matter to have the same importance which the latter seems to attach to it. He considers that the progress of chemical science is not entirely subordinate to a change of notation which does not strike at the foundation of the science as it had done a hundred years ago to the pneumatic chemistry of Lavoisier. He thinks that at the present day the truths are so general that all the laws may be expressed to a certain extent by both languages with equal clearness and precision. With regard to the view put forward by Wurtz, that bodies in the free state are composed of two atoms, and in support of which view he has mentioned the reactions of hydrochloric acid on hydride of copper, and peroxide of hydrogen on oxide of silver, $Cu_2H + HCl = CuCl_2 + HH$, and $Ag_2O + H_2O_2 = Ag_2 + H_2O + OO$. M. Berthelot deems the explanation given by M. Wurtz mere assumption, without sufficient proof, tending to prevent a true understanding of the real cause of the reaction. He also considers that the true explanation might be found in and explained by certain thermical considerations.

M. Berthelot passes next to a criticism of the atomic method of expressing the reactions of certain metallic salts with each other, and complains of the doubling of the equivalents of certain bodies, such as $CaCl_2$, which he thinks makes an unnecessary complication in the expression of the reactions, and gives as an instance the reaction of certain nitrates with chlorides. By the system of equivalents, he maintains they might be expressed by one reaction—$MNO_6 + M'Cl = MCl + M'NO_6$—but that by the atomic notation four different and distinct reactions are necessary to express their decomposition.

M. Berthelot then alludes to the confusion he thinks has arisen between the words law and hypothesis, in the acceptance of Avogadro's law. In this case he maintains that Avogadro and Ampère have enunciated, not a law, but an hypothesis, in saying, "All gases contain the same number of molecules in the same volume," having, in reality, nothing by which to conceive the idea of a molecule. On the other hand, he thinks the proposition, "The densities of gases or vapours are proportional to their equivalents," being deduced from two orders of properties observable by experiment, may be regarded as a true law. Partisans of the atomic notation have, he considers, substituted for this the proposition, "Molecules of simple gases contain the same number of atoms," and he complains that they thus introduce two hypothetical notions, that of the molecule, and that of the atom. On the other hand, supporters of the system of equivalents say, "Equivalent weights of simple or compound bodies occupy the same volumes;" or the volumes are to each other in the simple ratios, 1, 2, 3, 4, &c., thus:—

1 equivalent of oxygen occupies 1 volume.
1 ,, Cl, H, or Hg occupies 2 volumes
1 ,, HCl occupies 4 volumes, &c.

M. Wurtz, on the other hand, replaces the above by the statement, "All gases contain the same number of molecules;" the molecule may then be formed—

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[1] Throughout the lecture of which the above is a pretty full abstract, I have associated Mr. Herbert Spencer's theory of nervo-genesis with his name exclusively. To avoid misapprehension, therefore, I append this note to state that I am not ignorant of the fact that the theory in question has occurred to other thinkers as well as to the great English philosopher. Moreover, I am quite aware that even if this theory of nervo-genesis had never been enunciated *à priori* by any speculative thinker, some such theory would certainly have been devised *à posteriori* by any working physiologist of moderate capacity who might first happen to observe such facts as are above detailed. But considering that Mr. Spencer elaborated the theory deductively, and that he did so in a much more thorough and painstaking manner than had ever been done before, considering, too, that he has given the theory so elaborated such a prominent place in his system of objective psychology, I have not hesitated to describe this theory as being pre-eminently a product of his authorship.—G. J. R.

It may be by 1 atom, as Hg, Cd, &c.
,, 2 atoms, as H, Cl, O, &c.
,, 3 ,, ,, Ozone.
,, 4 ,, ,, P, A, &c.

What is there in this, M. Berthelot asks, more logical than in his own method? or in what may it be allowed to constitute a modern chemistry in distinction to that of Lavoisier and of Gay-Lussac?

In answer to M. Berthelot, M. Wurtz discusses in a further communication the thermic considerations brought forward by the former in his explanation of the reciprocal reactions already alluded to of hydride of copper with hydrochloric acid and peroxide of hydrogen with other peroxides. M. Wurtz, although acknowledging the value of these considerations, considers the reasoning incomplete, by merely saying that hydride of copper is an endothermic compound; it would be necessary to prove in addition why such bodies, having a greater capacity of heat than their constituent elements, can be formed or exist at all. He maintains that his own ideas admit of the intervention in the thermic considerations relative to the formation of such bodies as peroxide of hydrogen, ozone, chloride of nitrogen, &c., the work done in doubling the molecules of oxygen, O_2, chlorine, Cl_2, and nitrogen, N_2; this work causing an absorption of heat. He brings forward the ideas of M. Favre regarding the formation of bodies with an absorption of heat and alludes to the idea of this latter chemist of representing free oxygen as O_2 and nascent oxygen as O, the latter being more active in consequence of being supplied with more heat from what he calls the "segregation" of the molecule. M. Wurtz concludes that the thermical reasoning instead of being opposed to the atomic conception materially assists it.

In passing to the further objections made by Berthelot to expressing the reaction of potassium and hydrochloric acid as—

$$2HCl + K_2 = 2(KCl) + H_2,$$

M. Wurtz explains it as a necessity to render it comparable to the reaction—

$$2HCl + Zn = ZnCl_2 + H_2,$$

this representing 63·5 of zinc or two equivalents, having doubled the equivalent of zinc, as also of certain other metals for two reasons: firstly, to allow them to accord with the law of Dulong and Petit; and secondly, to satisfy Avrogadro's law. M. Wurtz evidently considers M. Berthelot in the position of almost wishing the abolition of Dulong and Petit's law, and explains that although the product of the specific heats into the atomic weights may not always prove rigorously constant, still they are sufficiently near, it being difficult to obtain one and the same metal under conditions strictly comparable. He wishes therefore to retain the law of Dulong and Petit as a check for the determination of the atomic weights. The notation of equivalents, he thinks, contains certain inconsistencies, and complains that no direct response has been made by its upholders to this point, which is really at the foundation of the discussion; they have only objected to atomic notation as introducing vexatious complications in expressing the reactions of mineral chemistry.

To M. Berthelot's reproach that he has confounded the notion of a hypothesis with that of a law, M. Wurtz replies in the following sentence:—"Je le remercie de cette leçon de philosophie; mais je ne crois pas en avoir besoin." He admits, for his own part, his knowledge that the notion of atoms and molecules is only a hypothesis, one which it is allowable to make on the constitution of matter, and essentially dependent on another, that of the existence of the ether. M. Berthelot believes the atomic hypothesis ill-founded, as atoms and molecules have never been seen, but for the same reason it would be as difficult to imagine the ether. To reject this latter hypothesis it would be necessary to adopt that of continuous matter of differing degrees of density filling all space, and M. Wurtz repeats that chemical notation, the point in question, is independent of such hypotheses.

The notion of atoms and molecules would have to be replaced by that of infinitely small vibrating masses, and at the basis of the notion of equivalent quantities lies the same idea of finite particles. What is really necessary is the choice of exact numbers to express the relative weights of these particles by whatever name they may be called; according to M. Wurtz they are of two orders: isolated, he calls them atoms; combined, molecules. Is this, he asks, an ill-defined idea?

In a discussion like the present, which has existed for so long, and which doubtless will still be continued, it is extremely difficult to balance accurately the arguments used by the upholders of the different theories. Although many and forcible reasons have been brought forward by both contending parties, we fear that no final victory has been gained by either side. M. Berthelot objects to the idea of molecules and atoms, but he evidently does not wish to exclude entirely imagination and hypothesis from scientific reasoning, and deems, as is probably the case, that the fundamental conceptions of the two chemical schools may not differ so much in that matter as perhaps M. Wurtz at present imagines.

The point on which the real difference seems to exist is, in perceiving the true importance of such representative conceptions, and placing them in the position they should occupy in human knowledge. J. M. T.

THE GREENLAND FOEHN[1]

ONE of the chief peculiarities of the meteorology of the Arctic regions, and particularly of West Greenland, is the great variability of the temperature in the cold part of the year. There may not only be a very considerable variation in the average monthly temperature from year to year, but sudden changes from the severest cold to fresh weather, and *vice versâ*, often occur several times in the course of the same month.

Dr. Pfaff has carried on meteorological observations at Jacobshavn for twenty years, and these show that the average temperature of 1872 was −8·7° C., and of 1863 − 31·6°, a difference accordingly of almost 23°—an almost inconceivable variableness to us West-Europeans; being, for instance, nearly as great as the difference between the coldest winter month and the warmest summer month at Copenhagen within the same period. The observations referred to also exhibited the most remarkable instances of daily variations. In February, 1860, the thermometer rose on three different occasions more than 25° C. in the course of twenty-four hours.

These singular and sudden rises of temperature almost always stand in connection with a veering of the wind to south-east and east. It may appear very surprising, that the temperature rises with the wind blowing from the high land in the interior of Greenland, which is covered with eternal snow and ice. We need not, therefore, be surprised that old authors have endeavoured to explain this phenomenon by supposed volcanoes in action, or even by a comparatively very mild climate in the interior of Greenland—an hypothesis which it is, however, quite impossible to maintain on meteorological grounds. For every continent in high latitudes must necessarily, from the radiation of heat, be colder in its interior than at the coast where the sea makes the climate milder.

A glance at the map shows that Greenland lies between regions of the earth where, especially in winter, the temperature is exceedingly different. "To the west and south-west there occurs at this season of the year in Labrador, the Hudson's Bay territories and the Arctic Archipelago, so great a fall of temperature, that the mean temperature of January sinks from −20° to −35° C.; to the east and south-east, on the contrary, the Gulf Stream, even in mid-winter, maintains the temperature in the Atlantic at from 0° to 5° C., so that the superincumbent air can scarcely be supposed in general to be cooled under the freezing-point. Lying between such opposite varieties of temperature, the climate of Greenland must necessarily be in a high degree dependent on the prevailing direction of the wind at every particular period; all winds from south by west to north-east may bring comparative cold, but east and south-east winds, on the contrary, heat, and this ought specially to hold good of the south-east wind, both because it comes from the warmest part of the neighbouring Atlantic Ocean, and also because it has the shortest way to travel over the ice-deserts of the interior to reach the western coast. The character of the winter in Greenland will therefore certainly depend on whether the south or the east wind has prevailed during the course of it."

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[1] Abstract of a paper by Hoffmeyer in the Danish Geographical Society's *Journal*, Part I., translated from *Naturen* of June, 1877 (Christiania).

These explanations go a great way indeed, but still are not altogether sufficient. "Thus, when at Jacobshavn, shortly before July, 9° C. of heat are recorded during a south-east storm, while the normal temperature is – 12° C.; this high temperature cannot be derived alone from the Atlantic nearest in the south-east to Greenland; for it is quite improbable that the air could have so high a temperature at this season, and even if it may be supposed to pass over Greenland in the short space of eight to ten hours, it must by the way suffer a greater or less cooling by contact with the cold ice masses. Indeed if we go down to South Greenland we will there, in the month of December, be able to observe over 14° C. of heat, a temperature which we cannot simultaneously find in the Atlantic much nearer than at the Azores, and it cannot be supposed that the air has travelled from these islands to Greenland with its temperature unchanged."

"There are also other properties, besides its high temperature, which specially characterise the south-east wind in Greenland. For it appears always to be very dry; the snow melts away from the low country without any running water being visible. The storm begins first on the mountain-tops, where the snow is seen whirling high in the air, and then it afterwards works itself down in the fiord valleys."

These relations drew the writer's thoughts to other regions of the earth. On the northern slopes of the Alps a stormy southerly wind sometimes begins to blow very suddenly, which, from the snow-covered summits, hurls itself with irresistible force through the valleys which lead towards the north, and throws the Alpine lakes into frightful commotion. This wind, which is named *Foehn*, has, although it comes from a snowy region, an unusual warmth and dryness. Prof. Dufour has shown that during a *Foehn* which raged during the 24th and 25th September, 1866, the temperature was 6° to 9° C. over the normal in northern Switzerland; indeed at the town of Zug, although it lies 440 metres above the level of the sea, the temperature was higher than it was at the same time both north and south of the Alps. The unusual heat and dryness of the Foehn is also shown by the circumstance that the boundary of the snow in the valleys is seen to have receded very considerably when the storm subsides; it is therefore called, on that account, "the great snow melter."

At the same time that the southerly wind is found as a warm and dry *Foehn* on the northern side of the Alps, there blows, on the other hand, on the southern slopes of the mountains a humid sirocco, generally accompanied by an enormous fall of snow.

Several years back Dr. Hann, of Vienna, solved this enigma in a highly satisfactory way.

We know that the pressure of the atmosphere decreases upwards; when, therefore, a mass of air is forced by any cause to raise itself from the surface of the earth to a certain height, it will be subjected to a constantly diminishing pressure, and will accordingly expand, but as every expansion is a work which is accompanied by a consumption of heat, the air is cooled as it rises. As long as the cooling is not greater than that the air can retain its watery vapour, the heat will, according to calculations which have been confirmed by observations, diminish almost exactly 1° C. for every 100 metres the air rises. On the other hand, if the dew-point is exceeded, so that the watery vapour forms clouds, rain, or snow, the moisture will pass from the form of vapour to the fluid or solid state, whereby the combined heat is set free. The cooling from this moment proceeds much more slowly, and it may, within the limits of which we have experience, be stated as about $\frac{1}{2}$° C. for every 100 metres.

When a mass of air, on the contrary, sinks towards the surface of the earth, it comes under higher pressure, is compressed, and consequently heated. Its temperature will rise more and more above the dew-point, and moisture will, with continually increasing ease, be held dissolved in the state of vapour. The heating during the whole descent will be 1° C. for every 100 metres.

These physical laws explain the properties of the *Foehn*. The air comes from the Mediterranean saturated with moisture, and passes over the summits of the Alps.

"Leaving out of consideration the cooling which goes on by the way, partly by radiation, partly by contact with mountain masses, a simple calculation will give the result that the temperature of a south wind will be about as many half degrees Centigrade higher at the north foot of the Alps than at the south foot, as the height of the mountain chain contains hectometres, for it is lowered half a degree for every 100 metres ascent, but raised one degree for every 100 metres descent."

These phenomena repeat themselves on Greenland. The writer sketches in detail a *Foehn* period which lasted eighteen to twenty days in the end of November and beginning of December, 1875. Jacobshavn was then for quite eight days warmer than North Italy. Upernivik, which lies about 10° to the south of the English North Pole Expedition's wintering station, was during the darkness of the Polar night warmer than the south of France. Unfortunately all direct observations from the uninhabited east coast of Greenland and the nearest parts of the Atlantic are wanting; but it may, however, be shown that during the period referred to a strong south-east wind blew from the sea over the land. For the so-called Buys-Ballot law in its simplest form teaches that the wind always blows so that it has the greater pressure of the atmosphere on its right, and that the more unequally the pressure is distributed the greater is the velocity of the wind. Now just during the days in question the barometer was much higher in Iceland than at Davis Straits. Over the tract lying between these places there had thus prevailed a strong south-east wind.

NOTES

OUT of above ninety candidates, Mr. James Edward Henry Gordon, B.A., of Caius College, Cambridge, has been selected by the Council of the British Association to be recommended to the Association as Mr. Griffith's successor in the important position of Assistant Secretary.

THE fiftieth *Versammlung deutscher Naturforscher und Aerzte* takes place at Munich on September 18–22. The following is the general programme:—Sept. 17, evening: Social gathering in the large saloon of the Rathhaus. 18: General session in the Odeon; address of welcome from Dr. v. Pettenhofer, addresses by Prof. Waldeyer, of Strassburg, on "C. v. Baer and his

Importance in the History of Evolution," and by Prof. Häckel, of Jena, on "The Present Theory of Evolution in its Relations to General Science." 19: Sessions of the sections. 20: General session; addresses by Prof. Tschermak, of Vienna, on "The Early History of the Earth;" by Prof. Klebs, of Prague, on "The Changes in Medical Theories during the Last Decade;" and by Dr. Neumayer, of Hamburg, on "The Relations of Meteorology to Every-day Life." Afternoon: Visits to the scientific collections. Evening: "Kellerfest." 21: Sessions of the sections. 22: Addresses by Dr. Avé Lallemant, of Lübeck, on "Animal Life on the Amazon;" by Prof. Günther, of Ansbach, on "The Latest Researches in the Mathematico-Historical Department;" and by Prof. Virchow, of Berlin. Expedition to the Stornbürger See.

THE forty-fifth annual meeting of the British Medical Association was opened on Tuesday at Manchester. A general meeting was held in the concert-hall, when the president, Dr. de Bartolome, of Sheffield, delivered an address. Dr. Wilkinson, of Manchester, was elected president for the ensuing year. In the evening a reception by the president of the Association and the Senate and Council of Owens College took place at that institution.

THE annual congress of the Royal Archæological Society of Great Britain and Ireland was opened at Hereford on Tuesday. After a public reception in the library and the presentation of a congratulatory address, the Bishop of Hereford, the local president, formally opened the proceedings. Sir Gilbert Scott delivered a lecture on the cathedral, and afterwards, in the cathedral itself, a historical and architectural description of the fabric. On the same day the annual meeting of the Cambrian Archæological Association was opened at Carnarvon, under the presidency of Lord Clarence Paget. The inaugural address was delivered by Prof. Babbington, who dwelt on the great advance of archæological science in North and South Wales. The Bishop of St. Asaph was elected president for the ensuing year, and the Hon. J. G. Wynn, hon. secretary for North Wales.

AS we announced some weeks ago, the fifth periodical international Congress of the Medical Sciences takes place in Geneva on the 9th to the 15th September proximo. Among other subjects to be treated, we note:—Influence of alcohol on mental disease, influence of immigration from the country to towns, tuberculosis on the mountains and the Mediterranean coast, physical characters of the electric discharge of the torpedo, cerebral localisations, cause of sleep, functions of the spleen, physiological antagonism.

THE German Anthropological Society holds its eighth annual congress at Constance on September 26.

THE death is announced of Mr. Robert Were Fox, F.R.S., of Falmouth, in his eighty-eighth year. Mr. Fox is known as the author of various observations, especially in connection with geology and mining. Early in the century he made important observations on the ratio of the decrease of temperature in the earth, and at a later period published various papers in connection with magnetism and electricity. Mr. Fox was widely known among men of science, and universally respected.

THE honour of knighthood is to be conferred on Vice-Admiral Erasmus Ommanney, C.B., F.R.S., Vice-Admiral Edward Augustus Inglefield, C.B., F.R.S., and Rear-Admiral George Henry Richards, C.B., F.R.S., the late Hydrographer to the Admiralty.

A NUMBER of German scientific men have united to form a committee for the purpose of collecting a sufficient sum to erect a statue of the lately deceased botanist, Alexander Broun.

IN the House of Commons, on Tuesday, Mr. A. Egerton stated that the papers relating to the transit of Venus were in the hands of the printers, and he hoped that it would not be very long before they were in the hands of members.

IN connection with Capt. Howgate's proposed polar colony, a preliminary expedition in the U.S. schooner *Florence* was to start from New London on July 25, under the command of Capt. Tyson, of the *Polaris*. The object of the expedition is to engage a dozen Esquimaux families to be conveyed to Robeson Straits by the colonial expedition, to purchase dogs, native sledges, and a supply of clothing. The place of meeting appointed is Disco, where the colonists from America, it is hoped, will arrive early next spring.

A LETTER has been received by Dr. G. Bennett (now in London) from Signor D'Albertis, dated Somerset, Northern Australia, May, 2, 1877, in which he says, "I am ready to start for the Fly River, New Guinea, and intend to leave in the steam launch *Neva* to-morrow morning, if the weather is fine. My crew consists of five Chinese, three South Sea Islanders, and an engineer. I shall write to you whenever I have an opportunity."

FROM July 1 to July 14 a Dutch pilot schooner, which was fitted out for the purpose, made a short cruise through the North Sea, having on board five gentlemen, all members of the Netherlands Zoological Society, who completed a series of about forty dredgings in different localities. Heligoland was the farthest point reached in a northern and eastern direction. There seems to be good reason to be satisfied with the results which at this moment are being worked out at the Zoological Summer Station erected at Flushing for the season of 1877. That youthful establishment was represented on board by three of its committee members, Drs. Horst, Hoek, and Hubrecht. The vessel had been put at their disposal by government.

THERE will be arranged at Havre on the occasion of the forthcoming session of the French Association for the Advancement of Science, an Archæological and Geological Exhibition of Normandy. It will be divided into six sections, one of them relating exclusively to prehistoric ages.

DR. SACHS, who was sent to Venezuela by the Berlin Academy of Sciences, for the purpose of studying the electric eel in its native haunts, and whose progress we have already chronicled, has now returned, after an absence of ten months, with a rich store of valuable observations, which will shortly be laid before the academy.

PROF. FREDRICK WAHLGREN, who has occupied the chair of zoology for twenty years in the University of Lund, died during the past month in his fifty-eighth year.

THE Dutch Geographical Society has received a report from the expedition recently sent out to explore Sumatra. One division left Padary in the middle of May for the mountainous centre of the island. They have successfully penetrated into these hitherto unknown regions, and describe them as of surpassing grandeur. The mountain sides are clothed to the very top with a most luxuriant forest growth, almost impenetrable to the sun's rays. The inhabitants consist of a few utterly degraded Malays gathered together in wretched villages. The health of the expedition is excellent.

PETERMANN'S *Mittheilungen* for August, contains an important article, with map, by Dr. Schunke, on the navigable water-ways of Germany, with special reference to the canals. Dr. Polakowsky's paper on the vegetation of Costa Rica is continued, and accompanying a large-scale chart is an account of the examination of the mouth of the Congo by Commander Medlicott and Lieut. Flood in 1875. Nearly one half of the number is occupied with Behm's monthly *résumé* of Geographical news, now one of the most important features in this valuable journal.

NEWS has been received in Europe of an eruption of the volcano Cotopaxi, near Quito. An immense quantity of ashes was ejected, principally in the directon of Guayaquil, falling on board ships sailing from Guayaquil to Panama. The distance was, in some instances, reckoned at 1,000 miles.

DURING the month of July an important series of longitudinal measurements have been carried out between the Bureau des Longitudes in Paris and the königliches geodötisches Institut in Berlin. The difference of longitude is now based on the mean of twelve carefully carried out observations. A series of observations between Paris and Bonn, and between Bonn and Berlin, which are to be undertaken during the present month, will act as a check on the work.

WE have received a "Sketch Guide to the Industrial Museum of Glasgow," by Mr. James Paton, Superintendent of the Museum. As the museum is at present incomplete but rapidly filling up, the Guide is only a temporary one. It is compiled on a somewhat novel but instructive and intelligent plan.

SEYDEL AND CO.'s hammocks, to which we referred in a recent number, have been awarded the gold medal for exellence at the International Horticultural Exhibition, Oporto.

WE have received from the enterprising firm of Mawson and Swan, of Newcastle, specimens of magic pens which not only write without ink but in different colours. It is not necessary that we should state the many arguments advanced to prove their vast superiority over those used at present, but it is very clear that they will be very useful to travellers whether the arguments in question are sound or otherwise.

AT a recent meeting of the Paris Geographical Society a letter from M. C. Wiener, who is travelling in South America at the expense of the French Government, was read, describing his ascent, on May 19, of Mount Illimani, whose height he makes out to be 20,112 feet. M. Wiener reached the summit, which he named Pic de Paris. Mr. Minchin, however, a railway engineer, who has been taking careful measurements of some of the South American peaks, gives the height of Illimani as 21,224, Wiener's figure being obtained by aneroid and boiling water.

A MOVEMENT is on foot for a union of the natural history societies in the Midland District on a similar basis to that which has worked so well in the case of the West Riding Consolidated Naturalists' Society. A number of societies, representing nearly a thousand members, have given their adhesion to the movement, and a meeting is to be held at the Birmingham Midland Institute on August 28 to discuss the programme of the union, the journal, and other matters.

THE pupils of the Parisian schools, who have obtained prizes in their respective classes, are to be sent on a pleasure trip to the seaside, under the direction of several masters, who are instructed to give them lectures on the places they may be visiting. This idea has been formerly acted on, but is now being tried on an enlarged scale.

THE Japanese Government have built, at their own expense, and through Japanese operatives, a war balloon. It has been tried successfully at Tokio, and will be sent to the southern army, which is directed against rebels. It is of thick silk, magnificently made, and will be inflated with pure hydrogen.

PARTS 50 and 60, completing the fifth volume of Mr. H. E. Dresser's great work on "The Birds of Europe," have been issued; and as there is but one more volume to come we may look forward with confidence to the completion of the entire undertaking in the course of next year. The present issue contains sixteen plates which are fully equal in accuracy and colouring to any that have preceded them, the gulls and terns, of which eight species are here figured, being especially beautiful. A provisional index to the five volumes now finished shows that 471 species of birds have now been figured and described.

IN the last number of the *Zoologische Garten* it is announced that a second specimen of *Archæopteryx lithographica* had been discovered. Twenty years have passed since the original and hitherto unique example of this wonderful bird of bygone days was obtained by Ernst Haeberlein in the quarries of Pappenheim, near Solenhofen. The second specimen, discovered in the same place and by the same observer, is said to be much more perfect than the first, and to possess the entire head—a knowledge of which is much wanted for the better understanding of the affinities of this extraordinary organism.

THE electrical illumination of the Lyons railway station is being completed. They are now using twelve electric lamps. This number will be enlarged successively to twenty-four lamps, fed with one light-producing and one light-distributing machine. It is believed that twenty-two horse-power will give a power of 2,400 gas-lamps, using 100 litres each per hour.

IN a little official guide-book to the Rothesay Royal Aquarium, Mr. Barker, the curator, has brought together, in a popular and attractive form (for non-visitors as well as visitors), a good deal of useful information about the various fishes. The example is worthy of imitation.

THE news of the discovery of a perfect mammoth in Tomsk is false. M. Polyakoff sent immediately by the St. Petersburg Academy, writes that he found only a large piece of mammoth flesh with skin and hair.

WE notice in the *Memoirs* of the St. Petersburg Academy, vol. xix., an interesting Russian paper by M. S. Lopatin, "Some Notes on the Ice-sheets in the Rocks of Eastern Siberia." The paper is the result of widely-extended observations made by the author during his numerous travels in Eastern Siberia (basin of Vitim, lower Yenissei, government Krasnoyarsk, &c.), and on the Sakhalin Island.

THE report of Dr. Schomburgk on the "Progress and Condition of the Botanic Garden and Government Plantations" at Adelaide, for 1876, has this year a similar feature to that of the recently-noticed report of Kew, inasmuch as it is illustrated; but in the case of that of Adelaide, with eight photographic views of different parts of the garden, external and internal views of the new palm-house, &c.; and a full description of this building is given. With regard to the prosperity of the garden, the greatest enemy, Dr. Schomburgk tells us, that it has had to contend with has been a very severe frost. The lowest temperature during the month of July was 28° Fahr., the lowest, indeed, ever experienced in Australia by Dr. Schomburgk. As might be imagined these severe frosts had a most disastrous effect upon most of the tropical plants, more especially on species of *Ficus*, many of which suffered so much that they were compelled to be cut down to two-thirds of their size, so that it will be years before they assume their former beauty, if ever they do. The frost made itself felt even in the glass-houses, and blackened the leaves of the plants standing near the glass, and the fountain basins were all covered with ice. Amongst useful plants which have occupied the attention of Dr. Schomburgk, the madder (*Rubia tinctorum*) seems to be amongst the most successful, so far as its rate of growth is concerned. It is stated to grow so vigorously about Adelaide that, "if not checked, it will become a nuisance, spreading everywhere." Its value is stated to be very great as a dye, and worth while cultivating, but we are under the impression that the aniline dyes have, to a great extent, and are still, indeed, driving madder out of the market. Attention will, no doubt, be centred upon other and more profitable plants. With regard to the routine work of the gardens, that is the distribution of seeds and plants, it does not compare badly with botanic gardens of greater pretension, for we learn that nineteen Wardian cases were dispatched during the year, containing about 800 stove and greenhouse plants, besides which 1,500 packets of seeds were also distributed to all parts of the world.

THERE is in the valley of the Maota in Switzerland, a grotto penetrating the mountain, and called the *Lauiloch*. It had not been explored beyond the Gorge du Loup, but recently some venturesome young people of Illgau have traversed this passage, and have penetrated, it is said, two whole leagues into the mountains, crossing various cavities where human foot had never trod before. They came at last to a deep fissure, which they could not explore, being without cords or ladders. A society has been formed for further exploration of the region, and the results will be published shortly.

IN a paper recently read to the Franklin Institute, Prof. Ennis gives the excellent advice to teachers that every day when the last half-hour of school-time arrives, the pupils should take their seats closely in front of the teacher's table, and he should then perform some scientific experiment, or exhibit some object of natural history, and tell all that can well be told about it. The pupils will make the more rapid progress in all their primary studies in consequence. The enjoyment of these scientific lectures is like dessert after dinner.

A PECULIAR kind of industry, that of breeding maggots, has lately been tried in Paris. Over the soil were spread large quantities of stale fish, dead lobsters, odorous poultry, and other refuse of the markets, as much as half a ton of large fish being taken on the premises in a single day. The maggots, which soon became abundant, were carefully picked out and packed in casks of galvanised iron, and finally were sold for fish bait and chicken food. The remaining refuse was converted into manure. Proximity to such an establishment could not have been very pleasant, and exposed provisions in the neighbourhood suffered largely from the visits of numberless flies. The police stepped in and suppressed the manufacture.

THE inhabitants of the Upper Engadine, one of the most attractive sites in Switzerland, have passed an order forbidding to sell or destroy a local wild-flower, which is called *Edelweiss*, and well-known to botanists. The destruction was so active that *Edelweiss* was fast disappearing.

THE three-yearly session of the International Congress for measuring the figure of the earth will take place at Stuttgart in the last days of September, under the presidency of Gen. Hanez, a Spaniard. The vice-president is Prof. Bauernfeind, late director of Munich Polytechnic School. It is said that France for the first time will join the Congress, and will be represented by Capt. Mouchez and Lœwy, two members of the Bureau des Longitudes.

THE following list of candidates successful in the competition for the Whitworth Scholarships, 1877, has been published by the Science and Art Department. William I. Last, Mechanical Engineer; F. Ogden, Mechanic; W. F. How, Engineer; W. S. M'Kenzie, Engineer; A. D. Ottewell, Draughtsman; D. A. Low, Engineer.

THE laboratories of the experimental farm at Vincennes, belonging to the French National School of Agriculture, were inaugurated the other day by the Minister of Public Instruction of the French Republic.

THE *Report* of the Royal Society of Tasmania contains among other papers of interest several important papers on Tasmanian shells by the Rev. J. E. Tenison-Woods.

THE bones of the bird hitherto known as *Tithornis emuinus* recently found at Sheppey, have enabled Prof. Owen to conclude that it was one with enormous wings, closely allied to, and much larger than, the albatross. The Professor, who has a paper on the subject in preparation, proposes to substitute a more appropriate name than the one given by Bowerbank. The bones are in the private collection of Mr. W. H. Shrubsole, of Sheerness-on-Sea, by whom they were found.

PROF. LANGLEY contributes to the *American Journal of Science and Arts* for July, an interesting paper "On the possibility of transit observations without personal errors."

THE Committee Report on the annual prize distribution of the French Société de Geographie appears in the Society's *Bulletin* for June. The recipients (to whom medals, &c., were awarded in April) are Lieut. Cameron, M. Roudaire, MM. de Folin and Leon Perrier, and M. Gravier; an account is given of the work of these investigators.

WE notice the appearance of a most interesting Russian work in the *Bulletin* of the Moscow Society of Friends of Natural Science, being a "Description of the various Zoological Gardens of Europe." The work is a collection of reports upon the most important zoological gardens, made by zoologists specially sent for that purpose, during 1876, by the Society above mentioned and by the Society of Acclimatisation, in order to find the best scheme for the organisation of the Zoological Gardens of Moscow. The introduction to the work is written by Prof. Bogdanoff.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseo-viridis* from Africa, presented by Mr. J. Harvey; a Weeper Capuchin (*Cebus capucinus*) from Brazil, presented by Mrs. Cameron; a Wood Brocket (*Cervus nemorivagus*) from Caura, presented by Mr. C. C. Berington; an Oil Bird (*Steatornis caripensis*) from Trinidad, presented by Mr. W. G. de Vœux; a White Goshawk (*Astur novæ hollandiæ*), a Berigora Hawk (*Hieracidea berigora*) from Australia, presented by Major Spicer; a Harpy Eagle (*Thrasaëtus harpyia*), a Great-billed Rhea (*Rhea macrorhyncha*) from South America, received in exchange; an Axis Deer (*Cervus axis*) born in the gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

SCIENCE AT KING'S COLLEGE, LONDON.—We understand that the Council of King's College have established a Science Course, including those subjects which, according to the new regulations, are required of candidates for the First B.Sc. or for the Preliminary Science Examinations of the University of London. Candidates for the Indian Civil Service, for the Home Civil Service, for the Indian Public Works Department, for the Royal Military Academy at Woolwich, and for other public examinations, will find in the course the scientific subjects which are required for those examinations. The course of study is under the direction of Prof. W. G. Adams. In addition to teaching and lectures in the several subjects, there will be included in the course Demonstrations and Practical work in the Physical, the Chemical, and the Biological Laboratories. The subjects for first year students in this course will be Mathematics, Elementary Mechanics, Physics, Chemistry, Zoology, and Botany, with practical work in each of the three laboratories. The second year's course will include these subjects with Geology.

EDINBURGH.—The Summer Session has just closed. In point of numbers the session 1876-77 has been the most prosperous the university has ever enjoyed, there being no fewer than

2,350 matriculated students. Last year the number was 2,065. The classes have consequently been large; especially is this true of the medical classes. For instance, the class of anatomy had 500 students on its roll. Sir Wyville Thomson's lectures were attended by upwards of 400 students; and Prof. Balfour had close upon 400 students in his class.

Prof. Lister has given his last lecture. At the close, Dr. J. B. Balfour in name of his fellow-students, expressed their regret at losing Mr. Lister, but at the same time honoured the motives which had led him to make the change, and wished him all success in the new sphere of work. Prof. Lister in replying said he felt very much gratified to find that his motives had not been misconstrued, and that so many of the students showed by their presence that they attributed his leaving them to a sense of duty. He thanked them for being so courteous and attentive, and appreciative of his efforts to teach, and wished them all happiness and prosperity.

TAUNTON COLLEGE SCHOOL.—Sixteenth and twenty-third in this year's list of successful candidates for Cooper's Hill are Messrs. Salter and Woollcombe, from the Taunton College School. This is an amusing commentary on the facts which we recorded some weeks ago.

BERLIN.—The magnificent new physiological laboratories are now nearly completed, and will be opened to students at the commencement of the winter semester. Prof. Dubois Raymond takes the directorship, and will be assisted by Prof. Kronecker, from Leipzig, and Prof. Baumann, from Strassburg, two of the more promising young physiological chemists of Germany. Prof. Helmholtz, who has been elected rector of the university for the coming year, will also take possession, during the coming autumn, of the spacious new physical laboratories which adjoin the physiological department.

MÜNSTER.—On July 19, the academic authorities laid the corner-stone of a spacious edifice which shall contain the lecture-rooms of the professors. The chemical laboratory of the newly-elected professor of chemistry, Dr. A. Oppenheim, is now nearly equipped, and will be opened to students during the coming autumn. It is reported that the Prussian Ministry of Public Instruction has the intention of supplying the lacking faculties of law and medicine, and of placing Münster on an equal basis with the other Prussian universities. The number of students at present is 300.

HEIDELBERG.—On July 27 the university authorities and students united in a festal celebration in honour of the twenty-fifth anniversary of the acceptance of a professorship in Heidelberg by Robert Wilhelm Bunsen. During the evening, one of those lengthy, picturesque, torchlight processions, so familiar to the residents of German university towns, led by gaily costumed marshalls, with gleaming swords, moved through the streets, to the residence of the veteran chemist, to extend to him the greeting of the students. Prof. Bunsen, who makes even shorter speeches than Gen. Grant, responded in a few modest words, accepting the honour more as a recognition of the offerings made by the university to the cause of science and especially of chemistry. The evening closed with the characteristic German *Commers*, in which ample tribute was paid to the eminent services of the great chemist in speech, poem, and song. Prof. Bunsen entered as a student at Göttingen fifty years ago. After six years of study there and at Paris, Vienna, and Berlin, he became privat-docent at Göttingen, then accepted, in 1836, a call to the Polytechnic of Cassel, as Wöhler's successor, removed in 1838 to Marburg, where he became in 1841 an ordinary professor, and from thence in 1851 to Breslau. In 1852 he followed a call to Heidelberg, where a new laboratory was built for him, and where he has remained despite many tempting offers from Berlin and other wealthier universities. His success as a teacher here has been unbounded, his laboratory and auditorium being full to overflowing, and the contingent of foreign students, from every quarter of the globe, being especially large. With the exception of his classical researches on cacodyl, and discovery of the antidotes for arsenious acid, most of Bunsen's more important discoveries occurred in the Heidelberg laboratory. These embrace researches on the absorption of gases, on diffusion, on the electrolytic preparation of metals, on photo-chemistry, on gasometric analysis, the invention of the magnesium light, the Bunsen lamp and galvanic element, &c. The most brilliant discovery of all still remains, that of spectral analysis, made in 1860 in company with Kirchhof, and leading to the immediate detection of cæsium and rubidium.

SCIENTIFIC SERIALS

THE current number of the *Journal of Anatomy and Physiology* commences with a paper by Drs. Lawes and Gilbert on the formation of fat in the animal body, in which from experiments on pigs it is shown most definitely that the amount of fat produced is not dependent on the amount of nitrogenous food ingested.—Dr. Ringer and Mr. Bury describe the influence of salacine on the healthy body with special reference to its influence on the temperature, in which it is demonstrated that the drug, like quinine, produces a slight depression for a brief period only. —Mr. T. W. Bridge writes on the cranial osteology of *Amia calva*, describing in detail the osseous elements of the skull, with a double plate illustrating it.—Prof. Rutherford and M. Vignal continue their account of experiments on the biliary secretion of the dog; the action of the sulphates of sodium, magnesium, potassium, phosphate, chloride, and bicarbonate of sodium, bicarbonate of potassium, chloride of ammonium, nitro-hydrochloric acid, and mercury are discussed.—Prof. Cleland describes a Sulu skull and gives suggestions for conducting craniological researches.—Mr. F. M. Balfour continues his valuable study of the development of elasmobranch fishes, completing the history of the primitive alimentary canal.—Mr. B. T. Lowne writes on the quantitative relation of light to sensation, as a contribution to the physiology of the retina.—Mr. W. H. Gaskell continues his observations on the vasomotor nerves of striated muscle, conducted in the laboratory of Trinity College, Cambridge, describing the normal circulation in muscle, the effects of section of the nerve, the effects of stimulating their ends, and the nature of vascular dilatation.

Reichert und Du Bois Reymond's Archiv, 1876, Part 4 (issued January, 1877).—J. Steiner, researches on the influence of temperature on the nerve and muscle current.—F. Boll on the structure of the electrical plates of torpedo.—G. Colasanti, anatomical and physiological researches on the arms of cephalopods.—E. A. Babuchin, further researches on electrical and pseudo-electric organs.

1876, Part 5.—H. Erler, on the relation between the exhalation of carbonic acid and the variation of animal temperature. —J. Hirschberg, dioptrics of the eye.—E. Dreher, on the theory of sight.

1876, Part 6 (issued April, 1877).—R. Hartman, contribution to the zoology and zootomy of the anthropoid apes.—H. Frey, on the vasomotor nerves of the extremities.—E. Hitzig, new researches on the brain.—W. Grüber, a series of papers on abnormalities of human anatomy.—G. Salomon, contribution to leukhæmia.

Zeitschrift für wissenschaftliche Zoologie, 1877, Parts 1 and 2 (in one).—F. E. Schulze, on the genus Halisarca, with five plates.—C. von Siebold, on the sexual development of urodele larvæ, referring especially to *Triton alpestris*.—F. de Filippi, on the larva of *Triton alpestris*.—A. Weismann, on the natural history of the Daphnidæ, parts 2, 3, and 4, 160 pp., with five plates.

Part 4.—H. Simroth, anatomy and fission of *Ophiactis virens*, 108 pp., four plates.—H. Dewitz, on the structure and development of the sting in ants.—L. Graff, on Neomenia and Chætoderma.—A. Brandt, on the frog's ovary, and the segmentation of the ovum.

Vol. 29, Part 1.—H. von Ihering, on the formation of ova in mollusca.—F. Vejdovsky, on the anatomy and metamorphosis of *Tracheliastes polycolpus* (parasitic copepod), three plates.—H. Ludwig, on the anatomy of *Rhizocochrinus lofotensis*.—F. E. Schulze, on sponges, part 3. Family Chondrosidæ.

Morphologisches Jahrbuch, vol. iii. Part 2.—H. von Ihering, on the nervous system of Amphineurida and Arthrocochlidæ (gastropods).—H. Strasser, on the air-sacs of birds.—E. Calberla, on the development of the spinal canal and cord in Teleosteans and Lampreys.—O. Hertwig, further contribution on the fertilisation and segmentation of the animal ovum.

Revue des Sciences Naturelles, vol. 6, June, 1877.—This number contains, in addition to its extended reviews of recent recent research in zoology, botany, and geology, articles on the classification of the animal kingdom, by A. Villot, on diatoms, by E. Guinard, on the cretaceous formation of Southern France, by M. Leymerie, and part of a catalogue of the terrestrial and fluviatile molluscs of the department of L'Hérault, by E. Dubrueil.

SOCIETIES AND ACADEMIES

VIENNA

Imperial Academy of Sciences, March 15.—The secretary presented the concluding parts (on Lepidoptera) of the work on the Novara expedition.—On the significance of Newton's construction of the order of colours of thin plates for the spectral investigation of the interference colours, by M. Rollett.—On the singularities of a conic-section system, by M. Igel.—On the development history of *equisetum*, by M. Tomaschek.—Medical observations, researches, and methods of cure, by M. Dyer.—Reciprocal linear surface systems, by M. Weyr.—Studies on the polypes and jelly fish of Trieste, by M. Claus.—Observations on the form and finer structure of the organ in the eel described as a testicle, by M. Frend.—On the central organ of the nerve system of Selachians, by M. Rohon.

May 11.—On the action of bromine on triamidophenol in presence of water, by MM. Weidel and Gruber.—A modification of Sauer's sulphur determination, by MM. Weidel and Schmidt.—On refraction and reflection of infinitely thin ray-systems on spherical surfaces, by M. Lippich.—On the discriminants of the Jacobi covariants, by M. Igel.—On the stationary flow of electricity in a plate, with use of straight electrodes, by M. Margules.—On the Turkish railways and their great economical importance, especially for Austria and Hungary, by M. Boué. He prophesies a great future for Salonica when the railway will be completed direct from the Danube along the Morava and from Vranja over Komanova to the Vardar railway. This will afford the shortest way from Austria to the Ægean Sea and the East.—On the influence of low temperatures on the life power of bacteria, by M. Fisch.—Contribution to the cryptogam flora of the Hawaiian peninsula, by M. Reichardt.—Theory and solution of irreducible transcendent equations, by M. Grossmann.

May 17.—On the inability of propylene to combine with water, by M. Linnemann. Even at 100° C. it unites neither with completely formed nor with nascent water.—Contributions to investigation of the phylogeny of plant species, by M. Ettingshausen.—On the normal surfaces to surfaces of the second order along a plane section of the same, by M. Koutny.—Production of corresponding points of two rational plane curves, by M. Zahradnik.—The northern light observations of the Austro-Hungarian Polar Expedition, 1872–4, by M. Weyprecht.—Free oblique projection, by M. Peschka.—Contributions to a knowledge of Phyllopoda, by H. Brauer.—Free temperature in its connection with external influences, by MM. Breitenlohner and Böhm.

June 7.—On an earthquake in Crete on the night of May 14, by M. Micksche. This is the most intense in the last three years. For forty-eight hours previously there was great calm in air and sea, and twenty-four hours after, violent thunderstorms occurred.—Observations on the origination of the cell-nucleus, by M. Stricker.—Small contributions to a knowledge of Annelidæ: I. The hypodermis of Lumbricidæ, by M. Mojsisovics.—On a method of determining the boiling point, by MM. Handl and Pribram.—Direct construction of the contours of rotation surfaces in general oblique projection, by M. Kuglmayr.—Theory and solution of irreducible transcendent equations with several unknown quantities and of higher order, by M. Grossmann.—On the distribution of fossil organisms in India, by M. Waagen. It is shown, *inter alia*, that the fresh-water formations extend far into the highlands towards the north-east.

PARIS

Academy of Sciences, July 30.—M. Peligot in the chair.—The following papers were read:—On the cosmic part of meteorology, by M. Faye. He throws doubt on the hypothesis connecting the sun-spots with magnetic variations, &c., on the one hand, and with actions of planets on the other, and assigns terrestrial causes for the variations.—Consequences to be drawn from experiments on the action of gases produced by dynamite, with regard to meteorites and the various circumstances of their arrival in the atmosphere, by M. Daubrée. The generic name of *Piezoglypt* is applied to the cavities produced in meteorites by the compressed gases. *Inter alia*, the fused external matter of the crust is easily carried, like sand and clay, towards the interior of the bolide, by the gas pressure outside. An extension of cupules over the large part of the surface indicates rotation of the meteorite.—Researches on the tertiary strata of Southern Europe, second part,—Tertiary strata of Vicentin, by MM. Hebert and Munier-Chalmas.—On an example of reduction of abelian integrals with elliptic functions, by Prof. Cayley.—Third note on the project of formation of a Saharan sea, by M. Cosson.—Organisation of the first scientific and hospital station of the International African Association, by M. de Lesseps. Arrangements have been made for establishing a depôt at Zanzibar, and an agency in Unyamwesi, so that the first station (under M. Crespel, accompanied by MM. Cambier and Maes) will be pretty far in the interior, perhaps on Lake Tanganyika, or further. M. Marno goes with the expedition as explorer.—Production of phylloxeric galls on the leaves of vine-stocks in the south of France, by M. Marés.—A message of sympathy was sent to M. Leverrier, who was stated to be getting better.—Observations on chemical equivalents, compared with corpuscular elements, by M. Baudrimont.—On a disease of the grape observed by M. Garcin, in the Narbonnese vineyards, by M. Macagno.—On the spectrum of the electric spark in gases submitted to increasing pressure, by M. Wüllner. Criticising a view wrongly attributed to him by M. Cazin, he distinguishes three modes of apparition of a continuous spectrum, according to the nature of the gas. The first (in hydrogen alone) is by diffusion of the spectral lines of the gas. In the second (in carbonised gases) a continuous spectrum appears between the lines, which finally disappear, without enlargement. In the third (*e.g.*, nitrogen and air), the lines continue visible and distinguishable with the continuous spectrum.—On the separation of iron from chromium and uranium, by M. Ditte. He recommends a similar treatment to that by which M. H. Sainte-Claire Deville separates alumina from iron.—On some properties of sulphides of platina from the analytic point of view, by M. Riban.—On a new mode of transformation of camphor into camphene, by M. de Montgolfier.—On some compounds of titanium, by MM. Wehrlin and Giraud.—Congenital ectopia of the heart; comparison of a graphic examination of the movements of the heart and cardiography in animals, by M. François-Franck.—On blood whose virulence resists the action of compressed oxygen and that of alcohol, by M. Bert. He is led to conclude that the blood in question contained not only *bacteridies* but septic vibrions whose corpuscular germs had this power of resistance, though the adult organisms succumbed to one or other of the two agents.—On the mechanism of deglutition, by M. Carlet. There is an interval (though very short) between the raising of the veil of the palate and the sudden ascent of the larynx.—On some points in the embryology of annelides, by M. Barrois.—A new type is here described, common at Roscoff in April.—On a new genus of the family of Tritoniades, by M. Vayssiére. It differs exteriorly from *Dendroustus* only in the terminal part of its tentacles; but there are important internal peculiarities. The name Marionia (from Prof. Marion) is applied to it.—On the determination of potash, by M. Carnot. The special character of the new method described is isolation of potash at the very first, without precipitating the other bases. This is affirmed to economise time, and obviate a loss which is considerable where these bases are in dominant proportions.

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THURSDAY, AUGUST 16, 1877

THE BRITISH ASSOCIATION

PLYMOUTH, *Tuesday*

THERE is every sign that the meeting of the British Association for 1877 will be a very successful one as far as attendance is concerned. It is too early in the day to quote figures, but in the spaces of time between the arrival of one London train and another a marked difference may be seen in the reception-room, and the clerks at the tables are already under a state of siege taking subscriptions, issuing tickets, answering innumerable questions, and indeed converting themselves into a long book-shelf of encyclopædias of useful knowledge.

It is a significant fact in connection with the numbers who have secured their tickets for the meeting, that at least half the reserved seats for the President's address and the two evening discourses, have already been applied for and appropriated. The Committee have, however, made a very fair arrangement with regard to these places. On the block plan of the noble Guildhall in which the addresses are to be delivered, a line has been drawn down the centre from the middle of the platform to the back of the hall which is thus divided longitudinally into two equal portions. One of these portions has been reserved for members coming from a distance, and will not be thrown open for general application until to-morrow. By this arrangement local residents applying for tickets have the advantage of priority over one another, but do not, through living on the spot, have any advantage over those arriving early to-morrow, which is the first day of the meeting.

Among early arrivals we noticed several familiar faces, among which may be mentioned Prof. Williamson, the general treasurer, Prof. Redfern, Mr. Glaisher, Mr. J. W. L. Glaisher, Mr. Pengelly, Mr. W. Chandler Roberts, Admiral Sir Erasmus Ommanney, K.C.B., Prof. Gladstone, Mr. Woodward, of the British Museum; but the great bulk of the visitors to the present meeting will be arriving by the late trains this evening and the early trains to-morrow (Wednesday) morning.

The following is the programme of the daily arrangements for the meeting:—

Wednesday, August 15.—The Address by the President, Prof. Allen Thomson, F.R.S., 8 P.M., in the Guildhall.

Thursday, August 16.—Sectional meetings. *Soirée* at 8 o'clock in Guildhall.

Friday, August 17.—Sectional meetings.—Lecture in Guildhall, 8.30 P.M., by Prof. Warington Smyth, F.R.S.

Saturday, August 18.—Sectional meetings. Excursions. Lecture to working men in Guildhall, 7 P.M., by Mr. Preece.

Monday, August 20.—Sectional meetings. Lecture in Guildhall, 8.30 P.M., by Prof. Odling, F.R.S.

Tuesday, August 21.—Sectional meetings.—*Soirée* in Guildhall, 8 P.M.

Wednesday, August 22.—Sectional meetings. Concluding meeting, 2.30 P.M.

Thursday, August 23.—Excursions.

The business of the sections will not be considered until to-morrow's meeting of the committees, but the excursions are pretty well settled. The first excursion-day will be Saturday next, the 18th instant, on which day there will be four excursions, in addition to a visit to Mount Edgcumbe, which will be thrown open to the members of the Association by the kindness of the Earl of Mount Edgcumbe. The other excursions are as follows:—

1. To Lee Moor and Dartmoor. An excursion will be made in waggonettes, by Plympton and Newnham Park, to the Lee Moor China Clay Works, the largest in the West of England. When the works have been inspected the company will be entertained at luncheon by the Messrs. Martin, the proprietors. After luncheon some of the party will inspect the pre-historic remains on the Plym, under the guidance of Mr. C. Spence Bate, F.R.S., one of the vice-presidents of the Association, who, besides his well-known biological memoirs, is the author of a most valuable work upon the remains, both historic and prehistoric, to be found in Devon and Cornwall. Others will visit Shell Top and Pen Beacon, from which the most magnificent views of Devonshire may be had. The party will be limited to one hundred, and application for tickets, which will be free, must be made before twelve o'clock on Friday next.

2. Another excursion for Saturday will be to the ancient city of Exeter. This excursion is by special invitation of the Mayor of Exeter on behalf of the citizens and inhabitants of the neighbourhood. The company, after visiting the Cathedral, Guildhall, and Museum, will be entertained at luncheon by the Mayor and Corporation at four o'clock. This excursion being only by special invitation partakes more or less of a private character, and therefore no tickets can be applied for.

3. One of the most interesting excursions for Saturday will be a visit to the Eddystone Lighthouse, the Plymouth Breakwater, and the Hamoaze, where a visit will be made to the gunnery ship *Cambridge*, to witness the great gun drill and torpedo practice, which will derive an especial interest from the affairs going on in Eastern Europe, in which torpedo warfare seems destined to play a hitherto unrivalled part. After leaving the *Cambridge*, the steamers which have been kindly placed at the disposal of the Association by Sir T. Symonds, the Naval Commander at this port, will go up the Hamoaze and Tamar to the Brunel's Royal Albert Bridge at Saltash, and the industrial training ship, *Mount Edgcumbe*. After leaving the Bridge the party will return down the Hamoaze, and proceed to sea, taking the breakwater on their way; here the work will be explained by one of the engineering staff, after which the boats will go on to the Eddystone Lighthouse, where some of the party will "land," if such it can be called. On the way between the Breakwater and the Lighthouse experiments will be made with Sir William Thomson's pianoforte-wire sounding apparatus, and with his very beautiful compass, which has already been described in these pages, but which we may remind our readers consists of a ring of aluminium which is supported from an agate centre, moulded in an aluminium boss, by means of a lacing of silk thread, so that the ring is kept in its place and concentric with the pivot by the tension of the threads. In this it bears some resemblance to the *tension* or "*spider*" wheels which are now fitted to the best bicycles. The magnetic portion of the instrument consists of two, four, six, or eight magnetised needles of steel wire, supported also to the ring by a lacing of silk threads. To the outer ring is attached the card or dial, which is of paper, and in order to prevent errors through the expansion or contraction of this material due to variations in the humidity of the atmosphere; it is divided radially into small sectors, each of which can expand or contract on its own account, but can have no effect on its neighbours. The advantages of this compass are extreme lightness of the movable portion, whereby friction on the pivot is reduced to a minimum, and variations in the direction of magnetic force, however slight, are indicated by the movement of the card; and owing to the smallness of the needles which are capable of performing the work, Sir William Thomson's compass comes to rest very quickly when disturbing influences are removed, and it is easily adjusted both in its permanent and in its temporary adjustments. After visiting Smeaton's masterpiece of engineering construction, which has stood the fury of the Atlantic storms for so many years, the party will return to Mill-Bay in time for the many private evening engage-

ments for that day. Tickets for this excursion will be freely given to a limited number of members, but none can be issued after Friday the 17th instant.

4. On the same day there will be a dredging excursion in Plymouth Sound and offing, under the direction of the Plymouth Institution. As this district is particularly rich in the crustacea, echinodermata, and the rarer southern fauna, there will no doubt be many applicants for tickets. The boats for this excursion will leave Mill Bay pier at ten o'clock, and the number of the tickets, which must be applied for before twelve o'clock on Friday, will be limited to fifty.

The excursions for Thursday, the 23rd inst., will consist of the following:—

1. Up the River Tamar to visit the Great Devon Consols Copper Mines.
2. To Liskeard, the Cheese-Wring, and the Phœnix and South Carradon Tin Mines.
3. To Totnes, Torquay, and Brixham, including visits to Kent's Cavern, to the Brixham Caves and the experimental works of Mr. Froude at Chelston Cross.
4. To Penryn, Falmouth, Penzance, and the Land's End.

For the first two of these excursions applications for tickets, which are limited, must be made before noon of Wednesday, the 22nd. The Totnes and Torquay excursion is by special invitation of the chairman of the Torquay Local Board of Health, on behalf of the inhabitants of the town, and, as such, is of a semi-private nature.

The "Red Lion" dinner, which is now almost as great an institution as the British Association itself, will come off on Tuesday next, at five o'clock, at Farley's Menagerie, Union Street.

As we intimated last week, Mr. John Edward Henry Gordon, B.A., late of Cambridge, has been appointed as the successor to Mr. Griffith. Mr. Griffith will, however, retain office until the Dublin meeting, Mr. Gordon being his assistant for the year, after which Mr. Gordon will be fully installed as Assistant General Secretary.

INAUGURAL ADDRESS OF PROF. ALLEN THOMSON, M.D., LL.D., F.R.S., F.R.S.E., PRESIDENT.

AFTER the long interval of six-and-thirty years the British Association for the Advancement of Science holds its annual meeting, the forty-seventh since its foundation, in this beautiful and interesting locality; and, strangely enough, on this occasion as on the former, it passes from Glasgow to Plymouth. We are delighted to be assembled here, and are even surprised that the Association has been able so long to resist the power of attraction by which it has been gravitating towards this place. While we are prepared to be charmed by the surpassing beauty of its scenery, and know the deep interest of its prehistoric vestiges, its historic memories, and its artistic associations, we have been frequently reminded of its scientific vigilance by the records of its active scientific work; and we are now ready and anxious to witness all we can behold of its energy and success in the application of scientific discovery to the practical arts. Should we, as might be expected in a place hitherto so famous in its relations to our naval and military history, find most prominent those relating to the mechanism of war, we shall still hope that the effect of greater perfection in the engines of destruction may only be the means of rendering peace more permanent and secure.

It is a source of regret to myself, and may be, I fear, a cause of detriment to this meeting that the choice of a President should have fallen upon one whose constant occupation with very special branches of science has fitted him so inadequately for the distinguished position to which he has been called. I can only derive comfort from knowing that, wherever it may be necessary, there are many others present most able to supply what may be wanting on my part; and I must, therefore, at once bespeak their assistance and your indulgence.

I have selected for the subject of the remarks which I am about to offer for your acceptance a biological topic, namely, the "Development of the Forms of Animal Life," with which my studies have been occupied, and which has important bearings on some of the more interesting biological questions now agitating the scientific world. But before proceeding with the discussion of my special subject, it is my desire to call your attention shortly to the remarkable change in the manner of viewing biological questions which has taken place in this country during the last half century—a change so great, indeed, that it can scarcely be fully appreciated except by those who have lived through the period of its occurrence.

In the three earlier decades of this century it was the common belief, in this country at least, shared by men of science as well as by the larger body of persons who had given no special attention to the subject, that the various forms of plants and animals recognized by naturalists in their systematic arrangements of genera and species were permanently fixed and unalterable; that they were not subject to greater changes than might occur as occasional variations, and that such was the tendency to the maintenance of uniformity in their specific characters that, when varieties did arise, there was a natural disposition to the return, in the course of succeeding generations, to the fixed form and nature supposed to belong to the parental stock; and it was also a necessary part of this view of the permanency of species that each was considered to have been originally produced from an individual having the exact form which its descendants ever afterwards retained. To this scientific dogma was further added the quasi-religious view that in the exercise of infinite wisdom and goodness, the Creator, when He called the successive species of plants and animals into existence, conferred upon each precisely the organization and the properties adapting it best for the kind of life for which it was designed in the general scheme of creation. This was the older doctrine of "Direct Creation," of "Teleological Relation," and of "Final Causes"; and those only who have known the firm hold which such views had over the public mind in past times can understand the almost unqualified approbation with which the reasoning on these questions in writings like the Bridgewater Treatises (not to mention older books on Natural Theology) were received in their time, as well as the very opposite feelings excited by every work which presented a different view of the plan of creation.

On the continent of Europe, it is true, some bold speculators, such as Goethe, Oken, Lamarck, and Geoffroy St. Hilaire, had in the end of the last and commencement of this century broached the doctrine that there is in living beings a continuous series of gradations as well as a consistent and general plan of organization; and that the creation, therefore, or origin of the different forms of plants and animals must have been the result of a gradual process of development or of derivation one from another, the whole standing connected together in certain causal relations. But in Britain such views, though known and not altogether repulsive to a few, obtained little favour, and, by some strange process of reasoning, were looked upon by the great majority as little short of impious questionings of the supreme power of the Almighty.

How different is the position of matters in this respect in our day!—when the cautious naturalist receives and adopts with the greatest reserve the statement of fixed and permanent specific characters as belonging to the different forms of organized beings, and is fully persuaded of the constant tendency to variation which all species show even in the present condition of the earth, and of the still greater liability to change which must have existed in the earlier periods of its formation—when the belief prevails that so far from being the direct product of distinct acts of creation, the various forms of plants and animals have been gradually evolved in a slow gradation of increasing complexity; and when it is recognized by a large majority of naturalists that the explanation of this wonderful relation of connection between previously existing and later forms is to be found in the constant tendency to variation during development and growth, and the perpetuation of such variations by hereditary transmission through successive generations in the long but incalculable lapse of the earth's natural mutations. These, as you must all be aware, are in their essential features the views now known as Darwinism, which were first simultaneously brought forward by Wallace and Darwin in 1858, and which, after being more fully elaborated in the works of the latter and ably supported by the former, secured, in the incredibly short space of ten or twelve years, the general approval of a large portion of the scientific world. The change of opinion is, in fact, now such that there are few scientific works on Natural History, whether of a special or more general character, in which the relation which the facts of science bear to the newer doctrines is not carefully pointed out; that, with the general public too, the words "Evolution" and "Development" have ceased to excite the feelings, amounting almost to horror, which they at first produced in the minds of those to whom they were equally unfamiliar and suspicious; and that even in popular literature and ephemeral effusions, direct or metaphorical illustrations are drawn in such terms of the Darwinian theory as "struggle for existence," "natural selection," "survival of the fittest, ' "heredity," "atavism," and the like.

It cannot be doubted that in this country, as on the Continent, the influence of authority had much to do with the persistence of the older teleological views; and, as has been well remarked by Häckel, one of the ablest and keenest supporters of the modern doctrine, the combined influence more especially of the opinions held by three of the greatest naturalists and biologists who have ever lived, viz., Linnæus, Haller, and Cuvier, men unsurpassed in the learning of their time, and the authors of important discoveries in a wide range of biological science, was decidedly adverse to the free current of speculative thought upon the more general doctrines of biology. And if it were warrantable to attribute so great a change of opinion as that to which I have adverted as occurring in my own time, to the influence of any single intellect, it must be admitted that it is justly due to the vast range and accuracy of his knowledge of scientific facts, the quick appreciation of their mutual interdependence, and above all the unexampled clearness and candour in statement of Charles Darwin.

But while we readily acknowledge the large share which Darwin has had in guiding scientific thought into the newer tracks of biological doctrine, we shall also be disposed to allow that the slow and difficult process of emancipation from the thraldom of dogmatic opinion in regard to a system of creation, and the adoption of large and independent views more consistent with observation, reason, philosophy, and religion, has only been possible under the effect of the general progress of scientific knowledge and the acquisition of sounder methods of applying its principles to the explanation of natural phenomena.

I have already referred to Goethe, Oken, Lamarck, and Geoffroy St. Hilaire as among the most prominent of the earlier pioneers in the modern or reformed conceptions of biological laws. But were it desirable to mark the progress of opinion by quoting other authors and labourers whose contributions have mainly supplied the materials out of which the new fabric has been constructed, I should have to produce a long catalogue of distinguished names, among which would be found those of Lyell and Owen, as earliest shaping the doctrines and guiding opinion in this country, Johannes Müller and von Baer, as taking the places of Haller and Cuvier on the Continent; and a host of other faithful workers in Biology belonging to the earlier part of this century, such as those of G. Treviranus, J. F. Meckel, Carus, and many more.[1] To Huxley more especially, and to Herbert Spencer, the greatest influence on British thought in the same direction is to be ascribed.

Let us hope that in these times, when it has been found necessary to modify the older teleological views to so great an extent, although there may still be much that is unknown, and wide differences of opinion in regard to the nature and sequence of natural phenomena and the mode of their interpretation, all naturalists will now concur in one important principle, viz., that truthful observation and candid judgment must alone be our guides in the interpretation of nature, and that that theory of creation is most deserving of our adoption which is most consistent with the whole body of facts carefully observed and compared.

To attempt to trace, within the limits to which my remarks must be confined, the influence which the progress of knowledge has exercised upon the scientific and general conception of biological doctrines would be impossible, for the modification of opinion on these subjects has proceeded not less from the rapid advance which our age has witnessed in the progress of general science, especially of physics and chemistry, than from that of departments belonging to biology itself.

Thus, to go no further than the most general laws of nature, the whole doctrine of the conservation and transmutation of force in physics, so ably expounded to this Association by Mr. Justice Grove, the theory of compound radicals and substitution, with the discovery of organic synthesis, in chemistry, and the more recent advance in speculation with regard to the molecular constitution and properties of matter, with which we must associate the names of our last President and of Clerk Maxwell, in completely changing the aspect of physical and chemical sciences within the last thirty-five years, have paved the way for views of the constitution and action of organised bodies very different from those which could be formed at the time of the first meeting of the Association in this place. And if, confining ourselves to the department of Biology, we add the discovery by microscopical observation of the minuter elementary forms of organisation, more especially as flowing from the comprehensive views of organised structure promulgated by Schleiden and Schwann nearly forty years ago, the later discovery and investigation of living protoplasmic substances, the accumulated evidence of progressive types of animal and vegetable forms in the succession of superimposed strata composing the crust of the earth, the recent discoveries as to the conditions of life at great depths in the ocean, the vast body of knowledge brought together by the labours of anatomists and physiologists as to the structure and functions of almost every plant and animal, and (still more, perhaps, than any other single branch of biological inquiry) if we note the rapid and immense progress which has been made during the last fifty years in the study of the entirely modern science of the development of living beings—we shall be able to form some conception of the enormous extension in our time of the basis of observation and fact from which biological phenomena may now be surveyed, and from which just views may be formed as to their mutual relations and general nature.

It is now familiarly known that almost all (if not indeed all) the plants and animals existing on the earth's surface derive their origin from parents or previously-existing beings whose form and nature they closely reproduce in their life's history. By far the greater number spring from germs in the form of visible and known spores, seeds, or eggs. A few may be traced to germs, or to vestiges of the parental body, the exact nature of which may be doubtful; and some, including even a certain number of those also produced from known germs, are either constantly or occasionally multiplied by budding, or by a process of cleavage, or direct and visible division of the parent body.

The germ constituting the basis of new formation, whether it have the form of spore, seed, or ovum, is of the simplest kind of organisation, and the process by which a new plant or animal is produced is necessarily one of gradual change and of advance from a simpler to a more complex form and structure: it is one of "evolution," or, as I would rather name it, "development." But before proceeding to discuss the subject of development in the higher animals, it is right to advert to the preliminary and often debated question, which naturally presents itself, viz.—Do all living or organised beings, without exception, spring from germs, or from any kind of organised matter that has belonged to parents? or may there not be some, especially among the simpler forms (with regard indeed to which alone there has of late been any question), which are produced by the direct combination of their component elements, in the way of the so-called spontaneous or equivocal generation, heterogenesis or abiogenesis?

The importance of the right solution of this problem is not confined merely to the discovery of the mode of origin of the lowly organisms which have been the more immediate object of investigation by naturalists in recent times, but is one of much wider significance, seeing that, if it shall be satisfactorily proved or even rendered probable that in the course of cosmical development all the various kinds of plants and animals have been gradually produced by evolution out of pre-existing simpler forms, and thus the whole series of organised beings in nature has been shown to be one of hereditary connection and derivation, then it would follow that the history of the origin of the simplest organisms may be the key to that of the first commencement of life upon the earth's surface, and the explanation of the relation in which the whole succeeding progenies stand to their parental stocks.

From the very lucid and masterly view of this subject, given by Prof. Huxley in his address to the Association at Liverpool, so recently as in 1870, in which the conclusion he formed was based very much on the exhaustive and admirable researches of Pasteur, I might almost have dispensed with making further reference to it now, but for the very confident statements since made by the supporters of the doctrine of abiogenesis, among whom Dr. Bastian stands most prominent in this country, and for the circumstance that the life-history of many of the lower organisms was still imperfectly known.

During the last seven or eight years, however, renewed investigations by most competent inquirers have followed one another in quick succession, from a review of which we cannot but arrive at a conclusion adverse to the theory of heterogenesis, viz., that no development of organisms, even of the most simple kind, has been satisfactorily observed to occur in circumstances which entirely excluded the possibility of their being descended from germs, or equivalent formative particles, belonging to pre-existing bodies of a similar kind. I can do no more now than name the authors of the most conclusive experiments on this subject, which I do nearly in the order of the publication of their researches, as those of Mr. W. N. Hartley in 1872, Messrs. Pode and Ray Lankester in 1873, Dr. Burdon Sanderson in that and the following years, Dr. W. Roberts in 1874, Prof. Lister in 1875, and most recently of Prof. Tyndall, Prof. Cohn, and of Messrs. Dallinger and Drysdale.[1]

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[1] It would also be unjust to omit to mention here one of the earliest attempts to bring British opinion into a new channel, by the remarkable work entitled "Vestiges of Creation," which appeared in 1844, nor to conceal from ourselves the unmerited ridicule and obloquy attempted to be thrown upon the author, not perhaps so much on account of the many inaccuracies unavoidable in an attempt at the time to overtake so large a field, as directed against the dangerous tendencies supposed to lurk in its reasoning.

But, admitting that the evidence from direct experiment is such as entirely to shut us out from entertaining the view that spontaneous generation occurs in the present condition of the earth, we are not relieved from the difficulty of explaining how living organisms or their germs first made their appearance; nor are we debarred from attempting to form hypotheses as to how this may have taken place. First, upon the theory of evolution, which, strictly carried out, supposes the more complex organisms to be derived from the more simple, it might be held that the conditions affecting the combination of the primary elements of matter into organic forms may at one time have been different from those which now prevail; and that, under those different conditions, abiogenesis may have been possible, and may have operated to lay the foundations of organic life in the simpler forms in which it at first appeared—a state of things which can only be vaguely surmised, but in regard to which no exact information can be obtained. Or, secondly, evading the difficulty of strict cosmical evolution, we might suppose that vital conditions may have been coeval with the first existence of physical and chemical properties in the rest of natural bodies. But this hypothesis would be exposed to the objection that, according to the cosmical view generally held by physicists, the whole materials composing the earth have originally been subject to incandescent heat. Nor is the difficulty abolished, but only removed to a more remote period by the supposition of the transport of germs from another planet or their introduction by means of meteorites or meteoric dust; for, besides the objection, arising from the circumstance that these bodies must have been subjected to a very high temperature, we should still have everything to learn as to the way in which the germs arose in the far distant regions of space from which they have been conveyed.

The incompleteness of the geological record leaves us in the dark as to the time at which the first dawnings of life appeared in the lower strata of the earth's surface. The most recent researches tend to carry the origin of life back to a much earlier period than was at one time believed, and if the famous *Eozoon* be admitted as evidence, even into that of the Laurentian strata. But even if doubts should prevail with regard to the presence of definite organised forms in the older sedimentary strata, the occurrence in them of carbon in the form of graphite in large quantities makes the previous existence of living organisms at least possible, and it may be that the complete metamorphosis which these rocks have undergone has entirely removed all definite traces of organisation.

Nor have we the means from geological data of determining whether the beings of the vegetable or of the animal kingdom first made their appearance. If we adopt the view which has for some time been entertained by physiologists that animals are entirely dependent, directly or indirectly, on plants for the material which constitutes their living substance, and that plants, as constructive agents, alone have the power to bring together the elements of lifeless matter from such states as carbonic acid, water, and ammonia, into the condition of the living solid, the inference would be inevitable, at least for the great majority of the animal creation, that they must have been preceded by plants. But palæontology is as yet silent on this interesting question; and, if we consider the remarkable approach which is made in structure and properties between the lowest and simplest members of the two kingdoms of organic nature, so that at last all distinction between them seems entirely to vanish, and a set of organisms is found which partake equally of animal and vegetable characters, or, rather, exhibit properties which are common to them both, we shall hesitate to postulate confidently for the primitive antecedence of vegetable life, although, perhaps, in later epochs the pre-existence of vegetables may be looked upon as necessary to the life of more developed animal organisms.

The reflection forces itself upon us that we are just as ignorant of the mode of first origin of all the compounds of the inorganic elements as we are of that of living matter; and we may therefore be excused if we suspend all theory and conjecture until we shall be guided to more reliable hypotheses through the plain track of observation and experiment.

The practical applications of the increased knowledge of the origin of minute animal and vegetable organisms are so numerous that it would occupy a much longer time than is at my disposal to give any detailed account of them; but they are of such immense importance in their commercial, social, and sanitary relations that they ought never to be lost sight of.

It is now proved beyond doubt that the origin of putrefaction and fermentation is dependent on the presence in the substances which are the seat of change in these processes, or in the surrounding air, of the germs of minute organisms of an animal or vegetable nature, and that the maintenance of the chemical changes in which these processes mainly consist is coincident with and casually (if not essentially) dependent upon the growth and multiplication of these organisms.

Prof. Lister had the merit of being the first to apply the germ-theory of putrefaction to explain the formation of putrid matters in the living body; and he has founded on this theory the now well-known antiseptic treatment of wounds, the importance of which it would be difficult to over-estimate.

The success or failure of plans for the preservation of meat and other articles of food without question depends on the possibility of the complete exclusion of the germs which are the cause of putrefaction and fermentation; and their management must therefore be founded on the most accurate knowledge of these organisms, and the circumstances influencing the persistence of their vitality and the vigour of their growth.

The theory of biogenesis has also lately been the guide in the investigation of the causes of various forms of disease, both in the lower animals and in man, with the result of showing that in many of them the infective substance consists in all probability of germs of minute animal or vegetable organisms.

There is very great probability, indeed, that all the zymotic diseases, by which we understand the various forms of fevers, have a similar origin. As has been well remarked by Baxter in an able paper on "The Action of Disinfectants," the analogies of action of contagia are similar to those of septic organisms, not to processes simply of oxidation or deoxidation. These organisms, studied in suitable fluids, multiply indefinitely when introduced in all but infinitesimal proportions. Thus they are, as near as we can perceive, the very essence of contagia.[1]

Leaving, however, these and many other general questions regarding the origin of the lowest forms of animal and vegetable life, let us now turn our attention to the mode of development of a new being in those belonging to the higher groups. The general nature of the formative process, in all instances where fertilised germs are produced, will be best understood by a short sketch of the phenomena ascertained to occur in different kinds of plants.

In the higher or phanerogamic plants it is generally well known that the combination of two parts of the flower is necessary to the production of a seed containing the embryo or young plant. Beginning with the discovery of the pollen tubes by Amici in 1823, the careful and minute investigations of a long line of illustrious vegetable physiologists have brought to light the details of the process by which fertilisation is effected, and have shown in fact how the minute tube developed from the inner membrane of the pollen-granule, as soon as it falls upon the stigmatic tissue of the seed-bearing plant, insinuates itself by a rapid process of development between the cells of the style, and reaches at last the ovule, in the interior of which is the embryo-sac; how, having passed into the micropyle, or orifice of the ovule, it makes its way to the embryo-sac; how a minute portion of the fertilising substance of the fovilla transudes from the pollen tube into the cavity of the embryo-sac, in which by this time a certain portion of the protoplasm has become differentiated into the germinal vesicle, thereby stimulating it to further growth and development, the earliest phenomena of which manifest themselves by the formation of an investing cell-wall, and by the occurrence of cell-division, which results in the formation of the embryo or plantule of the seed.

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[1] I may refer to Dr. Bastian's paper in NATURE of June 30, 1870, and to his two works, "The Origin of the Lowest Organisms," and "The Beginnings of Life." Mr. Hartley's researches, which were commenced in 1865, are described in a paper printed in the *Proceedings* of the Royal Society for 1872, and in his "Lectures on Air," 2nd edition, 1876, where an interesting account of the whole subject will be found. The experiments of Mr. Pode, of Oxford, and Prof. Ray Lankester are described in a paper on the "Development of Bacteria in Organic Infusions" in the Roy. Soc. *Proc.* for 1873, p. 349. Dr. Burdon Sanderson's researches are contained in the Reports of the Medical Officer of the Privy Council, and in various papers in NATURE; Dr. W. Roberts's paper is printed in the *Transactions* of the Royal Society for 1874, vol. clxiv. p. 457. Prof. Lister's "Contribution to the Germ Theory of Putrefaction and other Fermentative Changes," &c., is contained in the *Transactions* of the Royal Society of Edinburgh for 1875, p. 313, and is also given in NATURE. Prof. Tyndall's researches are described in his papers in the *Proceedings* of the Royal Society during the last two years. The work of Prof. Cohn, of Breslau, entitled "Beiträge zur Biologie der Pflanzen," 1873-76, contains many memoirs bearing upon this subject, which have been partly published in abstract in the *Microscopical Journal*, in which also will be found, in a series of contributions extending from 1873 to the present time, the interesting observations of Mr. W. H. Dallinger and Dr. J. Drysdale.

[1] For the most interesting information on this subject, I cannot do better than refer to the very able reports by Dr. Burdon Sanderson in the "Reports of the Medical Officer of the Privy Council," 1873, 1874, and 1875.

Thus it appears that the essential part of the process of production in phanerogamic plants is the formation in the parent plant of cells of two different kinds, which by themselves have little or no independent power of further growth, but which, by their union, give rise to a product in which the power of development is raised to the highest degree.

By further researches it is now known that the same law prevails in all the remaining members of the vegetable kingdom, with the exception only of the very simplest forms.[1]

In viewing the reproductive process in the series of cryptogamic plants, two facts at once strike us as remarkable in the modifications which are observed to accompany the formation of a productive germ, viz.: First, that the difference between the two productive elements becomes, as it were, more prominent, or more highly specialised, in the cryptogamic than in the phanerogamic plants; and second, that in the simpler and lower forms this difference gradually disappears till it is lost in complete uniformity of the productive elements.

Thus in the whole tribe of the ferns and vascular cryptogams, in the higher algæ and fungi, in the characeæ and in the mosses, the differentiation of the productive elements is carried to a very high degree; for while that belonging to the embryo or germ presents the structure of a simple cell which remains at rest, or in a comparatively passive state, and, absorbing into itself the substance of the other, becomes the seat of subsequent development; the other, corresponding to the pollen of the staminiferous phanerogam, is usually separated from the place of its formation, and having undergone a peculiar modification of structure by which it acquires active moving cilia, it changes place, and is directed towards the germinal structure, and coming in contact with its elementary cell, is more or less absorbed, or lost in the fertilising process. The protoplasm of the germinal cell, thus acted on and fertilised, then proceeds to undergo the changes of development by which the foundation is laid for the new plant.

In the algæ and fungi, however, there are gradations of the differentiation of the two reproductive cells, which are of the greatest interest in leading to a comprehension of the general nature of the formative process. For in the lower and simpler forms of these plants, such as the Desmidiæ, Mesocarpeæ, and other Conjugatæ, we find that there is no distinction in structure or form to be perceived between the two cells which unite or undergo conjugation; and a complete fusion or intermixture of the two masses of protoplasm results in the production of a single, usually spherical, mass holding the place of an embryo. And that there is an absence of specialisation between the two uniting cells is clearly shown in both *Desmidium* and *Mesocarpus*, by the fact that the embryo or zygospore is formed in the mass resulting from the union of the protruded portions of the two cells; and in more ordinary cases, as in *Spirogyra*, where the embryo is formed in one of the two cells, it seems to be indifferent in which of them it is formed.

From this, which may be regarded as the most elementary type of new production by the union of the two cells, the transition is not a great one to the development of a progeny without any such union. We might conjecture, then, that the capacity for separate or individual existence extends in the lowest organisms to the whole, or to each structural element of their organisation, while as we rise in the scale of vegetable life (and the same view might apply to the animal kingdom) this capacity is more and more divided between the two productive elements, or, at least, is only called into full action by their combination.

The germinal element consists of a simple primordial cell, varying in different kinds of plants, but in all of them probably containing the essential substance protoplasm, and the most immediate result or effect of fertilisation is the multiplication by repeated fissiparous division of the previously-existing cells. The new individual resulting from this cellular growth usually remains within the parent body, without, however, direct union or continuity of tissue, till the embryo has attained some advancement, as in the well-known case of the seeds of a phanerogam; but there are many varieties in the mode of its disposal among the lower plants.

A remarkable exception to the more direct relation of the process of fertilisation to the formation of the new individual or embryo occurs in some plants, simulating in some respects that kind of variation in animal reproduction which has been named alternate generation. A well-known instance of this belongs to the vascular cryptogams. The prothallium of the ferns, for example, results from the development of so-called spores or unicellular buds, which are familiar as being formed in small capsules on the lower leaf-surface; and in this prothallium, when it has reached a certain stage of vegetation, there are formed the archegonia, containing the oospheres, or germ cells, which are fertilised by the moving ciliated particles developed in the cells of the antheridia, leading to the production of a new spore-bearing plant.

Recent researches have also called attention to the remarkable arrangements in phanerogamic plants for the prevention of fertilisation of the pistils by pollen from the same flower, or even from the same plant. In the latter case this is effected by the separation of stamens and pistils in different flowers on the same, or on different plants. In the former case, where both organs occur in the same flower, the adaptations, whether of a mechanical or of a physiological character, by which self-fertilisation is prevented, as ascertained by numerous recent investigations (among which those of Darwin are most conspicuous), are of the most varied and often the most complicated kind.

Let us now turn to the consideration of the development of animals, and let me say in the outset that it will be necessary for me to confine my remarks chiefly to the higher or vertebrated animals, and to certain parts only of the history of their development; more particularly the structure and formation of the ovum or egg, its earlier developmental changes, and the relation of these to the formation of the new animal.

I cannot enter upon the consideration of this topic without adverting to the very recent acquisition of some of the most important facts upon which this branch of knowledge is founded; and I feel it to be peculiarly appropriate, in the year of his death, to refer to a biologist whose labours contributed more powerfully than those of any other person to give to animal embryology the character of a systematic branch of science, and to whom we owe some most important original discoveries,—I mean Karl Ernst von Baer of Königsberg, St. Petersburg, and Dorpat.

Of observers who, previous to von Baer, were mainly instrumental in preparing the way for the creation of a more exact modern science of embryology only two can be mentioned, viz., Caspar Frederick Wolff of St. Petersburg, well known as the author of a work entitled "Theoria Generationis," published in 1759, by which the *epigenesis*, or actual formation of the organs in a new being, was first demonstrated, and Christian Pander, who by his researches made at Würzburg explained, in a work published in 1817, the principal changes by which the embryo arises and is formed.

Von Baer was born in the Russian province of Esthonia on February 29, 1792. After having been fifteen years professor in the Prussian University of Königsberg, he was called to St. Petersburg, and having some years later been appointed to a newly-established professorship of Comparative Anatomy and Physiology, he remained in that city for nearly thirty years as the most zealous and able promoter of scientific education and research, stimulating and guiding all around him by his unexampled activity, comprehensive and original views, sound judgment, and cordial co-operation. In 1868, at the age of seventy-six, he retired to Dorpat, from the University of which he had received his degree in 1814, and continued still to occupy himself with working and writing in his favourite subjects, as well as interesting himself in everything that was related to educational and scientific progress, to very near the time of his death, which occurred on November 28, 1876, in his eighty-fifth year.

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[1] It will be observed that I leave entirely out of view the whole subject of the multiplication of plants by budding or simple division.

Although von Baer's researches, according to the light in which we may now view them, contributed in no small degree to the introduction of the newer views of the morphological relations of organic structure which have culminated in the Theory of Descent, yet he was unwilling to adopt the views of Darwin; and one of his latest writings, completed in the last year of his life, was in vigorous opposition to that doctrine.

It would have been most interesting and instructive to trace the history of the progress of discovery in embryology from the period of von Baer down to the present time, but such a history would not be suitable to the purpose of this address; and I can only venture here, in addition to Rathke, the colleague of Baer in Königsberg, to select two names out of the long list of distinguished workers in this field during the last forty years, viz.: Thomas Bischoff, of Giessen and Munich, to whom we owe the greatest progress in the knowledge of the development of mammals, by his several memoirs, appearing from 1842 to 1854; and Robert Remak, of Berlin, whose researches on the development of birds and batrachia, appearing from 1850 to 1855, gave greatly-increased exactness and extension to the general study of development.

The germinal element, from which, when fertilised, the new animal is derived, is contained within the animal ovum or egg—a compact and definite mass of organic matter, in which, notwithstanding great apparent variations, there is maintained throughout all the members of the animal kingdom, excepting the protozoa, which are destitute of true ova, a greater uniformity in some respects than belongs to the germinal product of plants.

Usually more or less spherical in form, the animal ovum presents the essential characters of a "complete cell," in the signification given by Schwann to that term. The germinal substance is inclosed by an external vesicular membrane or *cell-wall*. Within this covering the *cell-substance*, generally named yolk or vitellus, from the analogy of the fowl's egg, consists, to a greater or less extent, of a mass of protoplasm, and imbedded in this mass, in a determinate situation, there is found a smaller internal vesicular body, the *germinal-vesicle* or nucleus, with its more or less constant or variable *macula* or nucleolus.

Now the first thing which strikes us as remarkable connected with the ovum is the very great variation in size as compared with the entire animal, while in all of them the same simple or elementary structure is maintained. The ovum of mammals is, for example, a comparatively small body, of which the average diameter is about the $\frac{1}{160}$th of an inch, and which consequently scarcely weighs more than a very minute fraction of a grain, which may be calculated perhaps only at the $\frac{1}{12000}$th part. And further, in two animals differing so widely in size as the elephant and the mouse, the weights of which may be held to stand towards each other in the proportion of 150,000 to 1, there is scarcely any difference in the size of the mature ovum.

On the other hand, if we compare this small ovum of the mammal with the yolk of the egg in the common fowl, the part to which it most nearly corresponds, it may be estimated that the latter body would contain above three millions of the smaller ova of a mammal.

The attribute of size, however, in natural objects ceases to excite feelings of wonder or surprise as our knowledge of them increases, whether that be by familiar observation or by more scientific research. We need not, at all events, on account of the apparent minuteness of the ovum of the mammifer, or of any other animal, have any doubts as to the presence of a sufficient amount of germinal substance for explaining in the most materialistic fashion the transmission of the organic and other properties and resemblances between the parent and offspring. For we are led to believe, by those who have recently given their attention to the size of molecules composing both living and dead matter, that in such a body as this minute ovum of the mammal, there may be as many as five thousand billions of molecules, and even if we restrict ourselves to the smaller germinal vesicle, and, indeed, to the smallest germinal particle which might be made visible by the highest microscopic enlargement, there are still sufficient molecules for all the requirements of the most exacting material biologist.[1]

This great disparity of size is, however, connected with an important difference in the disposition of the yolk-substance, according to which ova may be distinguished as of two kinds—the large- and the small-yolked ova, between which there are also many intermediate gradations. The larger yolked ova belong to the whole tribe of birds, scaly reptiles, osseous and cartilaginous fishes, and the cephalopods among the invertebrates; and are distinguished by the strictly germinal part or protoplasm being collected into a small disc, known familiarly as the cicatricula of the fowl's egg, and to be seen as a whitish spot on that side of the yolk which naturally floats uppermost; while the rest of the yolk, of a deeper yellow colour, contains a large quantity of vitelline granules or globules of a different chemical nature from the protoplasm.

The phenomena of embryonic development are, in the first instance at least, confined to the germinal disc, and the rest of the yolk serves in a secondary or more remote manner to furnish materials for nourishment of the embryo and its accessory parts. Thus we distinguish the germinal from the nutritive or food-yolk, or, as the younger Van Beneden has named them, the *protoplasm* and the *deutoplasm*.

In the smaller ovum of the mammal, on the other hand, it seems as if the whole, or nearly the whole, of the yolk were protoplasmic or germinal. There may be some admixture of yolk-granules; but there is not the marked separation or limitation of the protoplasmic substance which is so distinct in birds, and the earliest changes of development extend to the whole component substance of the yolk, or, in other words, the yolk is entirely germinal. Hence some have given the names of *meroblastic* and *holoblastic*, meaning partially and entirely germinal, to these two contrasting forms of ova. There are many of the invertebrate animals of which the ova present the same entirely germinal arrangement as in those of mammals, and the *amphioxus* may be included in the same group.

The amphibia stand in some measure between the two extremes; the purely protoplasmic or germinal part occupying one side, and the nutritive or vitelline the other. But among the invertebrates the gradations are often such as to make it difficult to determine under which group the ova should be placed.

The genesis or formation of the ovum itself, if it be considered with reference to its first origin, carries us back to a very early period of the formation of the parent in which it is produced; and it is one of the most interesting problems to determine what is the source of the cells in the parent from which the ova originally spring. All that I can venture to say at present in regard to this point is, that the primordial ova or germs appear in the parental body while still embryonic, at a very early period of its development, and clearly derive their origin from a deeply-seated part of the formative cells which are undergoing transformation into the primitive organs; but the exact seat of the origin of the reproductive cells is still a matter of doubt.

When the ovum attains its full maturity in the ovary, the seat of its formation within the parent, it is separated from that organ, and when perfected proceeds to undergo embryonic development; a marked difference in this respect existing between the germinal product of the higher plants and animals.

The period of maturation of the ovum is marked in the greater number of animals by a series of phenomena which have generally been interpreted as the extrusion or absorption of the germinal vesicle; and various observers have actually traced the steps of the process by which that vesicle appears to leave the yolk and is lost to sight, or has passed into the space betwene the yolk and its membrane in the shape of the peculiar hyaline bodies named the *polar* or *directing* globules. But recent researches, afterwards to be referred to, tend to show that some part at least of the substance of the germinal vesicle remains to form, when combined with the fertilising element, the newly-endowed basis of future development.

Among the earliest changes to which the perfect animal ovum is subject, I have first to refer to the segmentation of the germ, a series of phenomena the observation of which has been productive of most important results in leading to a comprehension of the intimate nature of the formative process, and which is of the deepest interest both in a morphological and histological point of view. This process, which was first distinctly observed by Prevost and Dumas more than fifty years ago, and is now known to occur in all animal ova, consists essentially in the cleavage or splitting up of the protoplasmic substance of the yolk, by which it becomes rapidly subdivided into smaller and more numerous elements, so as at last to give rise to the production of an organised stratum of cells out of which by subsequent changes the embryo is formed.

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[1] According to a calculation made by Mr. Sorby, the number of molecules in the germinal vesicle of the mammalian ovum is such that if one molecule were to be lost in every second of time, the whole would not be exhausted in seventeen years. See Address to the Microscopic Society in *Journ. of Microscop. Science*, vol. xv. p. 225, and NATURE, vol. xiii.

p. 332. See also Darwin on Pangenesis, in his work on "Variations," &c. (1868), vol. ii. p. 374, and the Review by Ray Lankester of Haeckel's work, "Perigenesis der Blastidule," &c., in NATURE for 1876, p. 235, an Ray Lankester's Essay on "Comparative Longevity," 1870.

The process of yolk segmentation may at once be distinguished as of two kinds, according as it affects in the small-yolked ova the whole mass of the yolk simultaneously, or in the large-yolked ova is limited to only one part of it. The cleavage process, in fact, affects the germinal and not the food-yolk; so that to take the two most contrasting instances of the bird and mammal to which I have before referred, it appears that while the mammal's ovum undergoes entire segmentation, this process is confined to the substance of the cicatricula or germinal disc of the bird's egg. This process is essentially one of cell-division, but it is also in some measure one of cell-formation. The best idea of its nature will be obtained from a short description of the total segmentation occurring in the mammal's ovum.

When, as before mentioned, the germinal vesicle has been in part extruded or lost to sight, the whole yolk-substance of the ovum forms a nearly uniform mass of finely granular protoplasm, inclosed within the external cell-membrane. Within a few hours later a clear nucleus has arisen in this mass. To this more definite form of organisation, assumed by the germinal substance of the future animal which is about to be the subject of the segmenting process, the name of the first segment-sphere may be given.

By the process of cleavage, which now begins, this first segment-sphere and its nucleus undergo division into two nucleated spheres of smaller size, the whole substance of the yolk, in a holoblastic ovum, such as that of the mammal, being involved in the segmenting process.

The second stage of division follows after the lapse of a few hours, and results in the formation of four nucleated segment spheres; and the process of division being repeated in a certain definite order, there result in the succeeding stages, that is, the third, fourth, fifth, and up to the tenth, the numbers of 8, 12, 16, 24, 32, 48, 64, and 96 nucleated yolk-spheres, germ-spheres, or formative cells.

In the rabbit's ovum the tenth stage is reached in less than three days; and as during that time the size of the whole ovum has undergone very little increase, it follows that the spheres of each succeeding set, as they become more numerous, have diminished greatly in size. These segment-spheres are all destitute of external membrane, but are distinctly nucleated; and their protoplasmic substance is more or less granular, presenting the usual histological characters of growing cells.

By the time that segmentation has reached the seventh or eighth stage, when 32 or 48 spheres have been formed, the ovum has assumed the appearance of a mulberry, in which the outer smaller spheres, closely massed together, project slightly and uniformly over the whole surface; while the interior of the ball is filled with cells of a somewhat larger size and a more opaque granular aspect, also resulting from the process of segmentation.

Already, however, the mutual compression of the spheres or cells on the surface, by their crowding together, has led to the flattening of their adjacent sides; and by the time the tenth stage is reached, when the whole number of the cells is about 96, the more advanced superficial cells having ranged themselves closely together, form a nucleated cellular layer or covering of the yolk, inclosing within them the larger and more opaque cells, derived like the first from the segmenting process. In a more advanced stage, the deeper cells now referred to having also taken the form of a layer, there results at last the bilaminar blastoderm or embryonic germinal membrane.

The process of partial segmentation, such as occurs in the bird's egg, though perhaps fundamentally the same as that of the mammal previously described, stands in a different relation to the parts of the whole yolk or egg, and consequently differs in its general phenomena. The segmentation is mainly restricted in the meroblastic ova of birds to the germinal disc or cicatricula, and does not immediately involve any part of the larger remainder of the yolk. This takes place during the time of the descent of the yolk through the oviduct, when the yolk is receiving the covering of the white or albumen, the membrane and the shell, previous to being laid—a progress which, in the common domestic fowl, usually occupies less than twenty-four hours. Corresponding essentially to the more complete segmentation of the mammal's ovum, the process leads to the same result in the production of two layers of nucleated formative cells in the original seat of a protoplasmic disc; a bilaminar blastoderm resulting as in the mammal's ovum, though in a somewhat different relation to the yolk.

I will not fatigue you with a description of the details of these phenomena, interesting as they may be, but only mention generally that they consist in the formation of deep fissures running from the surface into the substance of the germ-disc. The first of these fissures crosses the disc in a determinate direction, dividing it into two nearly equal semicircular parts. In the next stage another fissure, crossing the first nearly at right angles, produces four angular segments. Then come four intervening radial fissures, which subdivide the four segments into eight; and next afterwards the central angles of these eight radial segments are cut off from their peripheral portions by a different fissure, which may be compared to one of the parallels of latitude on the globe near the pole where the radial or *longitude* fissures converge. And so thereafter, by the succession and alternation of radial and circular clefts, which, however, as they extend outwards, come soon to lose their regularity, the whole germinal disc is divided into the two layers of nucleated cells, constituting the blastoderma or germinal membrane of Pander and all subsequent embryologists.[1] If a laid egg be subjected to the heat of incubation for eight or ten hours, the cicatricula, now converted into this segmented blastoderm, is found to be considerably expanded by a rapid multiplication of its constituent cells, and in as many more hours, by further changes in its substance, the first lineaments of the chick begin to make their appearance. Similar changes affect the blastoderm of the mammal, and thus it appears that the result of segmentation, in the bird as well as in the mammal and other animals, is the production of an organised laminar substratum, which is the seat of the subsequent embryonic development.

I must still request your attention to some details connected with the process of segmentation, which bear upon the question of the origin of the new cells, and on which recent research has thrown a new and unexpected light.

With respect to the nature of the first segment-sphere of the ovum and the source of its nucleus, as well as of the other segment-spheres or cells which follow each other in the successive steps of germ-subdivision, it appears probable from the researches of several independent observers, and more especially of Edward Van Beneden and Oscar Hertwig, that in the course of the extrusion of the germinal vesicle, a small portion of it remains behind in the form of a minute mass of hyaline substance, to which Van Beneden has given the name of *pronucleus*, and that, as the result of the fertilising process, there is formed a second similar hyaline globule or pronucleus, situated near the surface, which gradually travels towards the centre and unites with the first pronucleus, and that these two pronuclei, being fused together, form the true nucleus of the first segment sphere. According to this view the original germinal vesicle, when it disappears, or is lost to sight, as described by so many embryologists, is not dissipated, but only undergoes changes leading to the formation of the new and more highly endowed nucleus of the first embryonic or segmental sphere. It further appears that the sub-division of each segmenting mass is preceded by a change and division of the nucleus, and that this division of the nucleus is accompanied by the peculiar phenomenon of a double conical or spindle-shaped radial lineation of the protoplasm, which, if we were inclined to speculate as to its nature, seems almost as if it marked out the lines of molecular force acting in the organising process. These lines, however, it will be understood, if visible with the microscope, even of the highest magnifying power yet attained, belong to much larger particles than those of the supposed molecules of the physicist; but considered in connection with what we know of the movements which frequently precede the act of division of the yolk-spheres, we seem in this phenomenon to have made some near approach to the observation of the direction in which the molecular forces operating in organisation may be supposed to act.[2]

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[1] The more exact nature of the process of segmentation was first made known by the interesting researches of Bagge in 1841, and more especially of Kölliker in 1843. The phenomena of complete segmentation were first fully described in the mammal's ovum in Bischoff's "Description of the Development of the Rabbit," 1842, and followed out in his succeeding "Memoirs on the Dog, Guinea-pig, and Roe-deer." The phenomena of partial segmentation were first made known, in their more exact form, by Kölliker's "Researches on the Development of the Cephalopoda," published in 1844. In birds the process was first described by Bergmann in 1846, and more fully by Coste in 1848.

[2] The observations referred to above as to the division of the nucleus are so novel and of such deep interest that I am tempted to add here a short abstract of their more important results from a very clear account given of them by Dr. John Priestley, of Owens College, Manchester, in the *Journal of Microscopical Science* for April, 1876.

With respect to the nature of the blastoderm, the organised cellular stratum resulting from segmentation, and its relation to the previous condition of the ovum on the one hand, and the future embryo on the other, there is presented to us, by modern research, the interesting view that the blastoderm consists, after completion of the segmenting process, of two layers of cells, an outer or upper, usually composed of smaller, clearer, and more compact nucleated cells, named *ectoderm*, or *epiblast*, and an inner or lower, consisting of cells which are somewhat larger, more opaque, and granular, but also nucleated, and named *endoderm*, or *hypoblast*.

In the meroblastic ova, such as those of birds, the bilaminar blastoderm is discoid and circumscribed, as it lies on the yolk surface, and only comes to envelope the whole of the food-yolk in the progress of later development; while in the holoblastic ova, and more especially in mammals, the blastoderm from the first extends over the whole surface of the yolk, and thus forms an entire covering of the yolk known as the "vesicular blastoderm;" the space within being occupied by fluid.

Huxley long ago presented the interesting view that these two layers are essentially the same in their morphological relations and histological structure with the double wall of the body in the simplest forms of animals above the protozoa; and Haeckel has more recently followed out this view, and supported it by his researches in the Calcareous Sponges, and has founded upon it his well-known *Gastræa* theory. According to this view all animals take their origin from a form of *Gastrula*. In the simpler tribes, as in the instance of the common fresh-water polype or hydra, they proceed no further than the gastrula stage, unless by mere enlargement and slight differentiation of the two primitive layers of cell, representing the persistent ectoderm and endoderm.[1]

If, pursuing this idea, we take a survey of the whole animal kingdom in its long gradation of increasing complexity of form and structure from the simplest animal up to man himself, we find that all the various modifications of organic structure which present themselves are found, in the history of the individual or ontological development of the different members of the series, to spring originally from two cellular laminæ, ectoderm and endoderm, the component elements of which may again be traced back to the first segment-sphere and primitive protoplasmic elements of the ovum.

Time does not admit of my conducting you through the chain of observation and reasoning by which Haeckel seeks to convince us of the universal applicability of his theory, but I cannot avoid calling your attention to the extremely interesting relation which has been shown to exist between the primary phases of development of the ovum and the foundation of the blastoderm in very different groups of animals, more especially by the researches of Haeckel himself, of Kowalevsky, Edward Van Beneden, and others, and which has received most efficient support from the investigations and writings of E. Ray Lankester in our own country; so that now we may indulge the well-grounded expectation that, notwithstanding the many and great difficulties which doubtless still present themselves in reconciling various forms with the general principle of the theory, we are at least in the track which may lead to a consistent view of the relations subsisting between the ontogenetic, or individual, and the phylogenetic, or race, history of the formation of animals and of man.[1]

In all animals, then, above the protozoa, the ovum presents, in some form or other, the bilaminar structure of ectoderm and endoderm at a certain stage of its development, this structure resulting from a process of segmentation or cell cleavage; and there are three principal modes in which the double condition of the layers is brought about. In one of these it is by inward folding or invagination of a part of the single layer of cells immediately resulting from the process of segmentation that the doubling of the layers is produced; in the second, perhaps resolvable into the first, it may be described rather as a process of inclosure of one set of cells within another; while in the third the segmented cells arranged as a single layer round a central cavity of the ovum, divide themselves later into two layers. But the distinction of ectodermic and endodermic layers of cells is maintained, whether it be primitive and manifested from a very early period, or acquired later by a secondary process of differentiation. Thus, in many invertebrates, as also in *Amphioxus* among the vertebrates, a distinct invagination occurs, while in mammals, as recently shown by Van Beneden's most interesting observations in the rabbit's ovum, and probably also in some invertebrates, the cells of the ectoderm gradually spread over those of the endoderm during the progress of segmentation, and thus the endodermic comes to be inclosed by the ectodermic layer of cells.

From the very novel and unexpected observations of Van Beneden it further appears that from the earliest period in the process of segmentation in the mammal's ovum it is possible to perceive a distinction of two kinds of segment-spheres, or cells, and that when this process is traced back to its first stage it is found that the whole of the cells belonging to the ectoderm are the progeny of, or result from the division of the upper of the two first formed segments, and that the whole of the endodermic cells are the descendants of the lower of the two first segmented cells. This, however, is not an isolated fact belonging only to mammalian development, but one which very nearly repeats a process ascertained to occur in a considerable number of the lower animals, and it seems to promise the means of greatly advancing the comprehension of the whole process of blastodermic formation. Thus, ectoderm and endoderm, or the primordial rudiments of the future animal and vegetative systems of the embryo, are traced back as distinct from each other to the first stage of segmentation of the germ.

Accepting these facts as ascertained, they may be regarded as of the deepest significance in the phylogenetic history of animals; for they appear to open up the prospect of our being able to trace transitions between the earliest embryonic forms occurring in the most different kinds of ova, as between the discoid or meroblastic, and the vesicular or holoblastic, through the intermediate series which may be termed amphiblastic ova.

In the lowest animals, the two layers already mentioned, viz., ectoderm and endoderm, are the only ones known to constitute the basis of developmental organisation; but as we rise in the scale of animals we find a new feature appearing in their structure which is repeated also in the history of the formation of the blastoderm in the higher animals up to man. This consists in the formation of an intermediate layer or layers constituting the *mesoderm*, with which, in by far the greater number, is connected the formation of some of the most important bodily structures, such as the osseous, muscular, and vascular systems.

I will not stop to discuss the very difficult question of the first origin of the mesoderm, upon which embryologists are not yet entirely agreed, but will only remark that a view originally taken of this subject by the acute von Baer appears more and more to gain ground; and it is this—that the mesoderm, arising as a secondary structure, that is, later than the two primary layers of ectoderm and endoderm (corresponding to the serous and mucous layers of Pander), is probably connected with or derived from both of these primitive layers, a view which it will afterwards appear is equally important ontogenetically and phylogenetically.

The researches now referred to are those of Auerbach, Butschli, Strasburger, Hertwig, and Edw. Van Beneden, and the following may be stated as the points in which they mainly agree:—

The nucleus when about to divide elongates into a spindle-shaped body, becomes irregular and indistinct, acquires a granular disc or zone in the plane of its equator; this divides into two, and each half moves towards the pole of the spindle on its own side, there being radiated lines of protoplasm between the poles and the equatorial disc.

The disc segments are the new nuclei, and the subsequent division of the cell takes place in the intermediate space.

Although these observers still differ in opinion upon some of the details of this process, and especially as to the fate of the germinal vesicle, all of them seem to agree that there are two pronuclei or distinct hyaline parts of the yolk protoplasm, a superficial and a deep one, engaged in the formation of the new nucleus, and both Hertwig and Van Beneden are of opinion that the two proceed from different productive elements.

The radiated structure of the nuclei had been previously recognised by Fol and Flemming, and further observed by Oellacher.

1. Butschli's researches are published in the *Nov. Act. Nat. Cur.*, 1873, and in the *Zeitschr. für wissensch. Zool.*, vol. xxv.

2. Auerbach's observations in his *Organolog. Studien*, 1874.

3. Strasburger's observations in his memoir "Ueber Zell-bildung und Zelltheilung," Jena, 1875.

4. Edward Van Beneden's researches, partly in his memoir "On the Composition and Significance of the Egg," &c, presented to the Belgian Academy in 1868, and more particularly in the extremely interesting preliminary account of "Researches on the Development of Mammalia," &c., 1875; and in a separate paper in the *Journ. of Microscopical Science* for April, 1876.

5. Oscar Hertwig's Memoirs are contained in the *Morpholog. Jahrbuch*, 1875, and his most interesting and novel observations in the same work, 1877.

[1] At this place I will only refer to one of the most recent of Haeckel's works, in which the views alluded to above are fully exposed in a series of most interesting memoirs, viz., "Studien zur Gastræa-Theorie," Jena, 1877.

[1] I ought here to refer to the elaborate memoirs of Prof. Semper on the morphological relations of the vertebrate and invertebrate animals contained in the "Arbeiten aus dem zoolog. zootom. Institut in Würzburg," 1875 and 1876, in which the conclusions arrived at do not coincide with the views above stated

But whatever may be the first origin of the mesoblast, we know that in the vertebrata this layer, separating from between the other two, and acquiring rapidly by its cell multiplication larger proportions and much greater complexity than belongs to either ectoderm or endoderm, speedily undergoes further subdivision and differentiation in connection with the appearance of the embryonic organs which arise from it, and in this respect contrasts greatly with the simplicity of structure which remains in the developed parts of the ectodermic and endodermic layers. Thus, while the ectoderm supplies the formative materials for the external covering or epidermis, together with the rudiments of the central nervous organs and principal sense-organs, and the endoderm by itself only gives rise to the epithelial lining of the alimentary canal and the cellular part of the glands connected with it, the mesoblast is the source of far more numerous and complex parts, viz., the whole of the true skin or corium, the vertebral column and osseous system, the external voluntary muscles and connective tissue, the muscular walls of the alimentary canal, the heart and blood-vessels, the kidneys, and the reproductive organs thus forming much the greatest bulk of the body in the higher animals.[1]

There is, however, a peculiarity in the mode of the earliest development of the mesoblast which is of great importance in connection with the general history of the disposition of parts in the animal body to which I must now refer. This consists in the division of the mesoblast in all but its central part into two laminæ, an outer or upper and an inner or lower, and the separation of these by an interval or cavity which corresponds to the space existing between the outer wall of our bodies and the deeper viscera; and which from the point of view of the vertebrate animals is called the pleuro-peritoneal cavity, but viewed in the more extended series of animals down to the annuloida, may receive the more general appellation of pleurosplanchnic or parieto-visceral cavity, or, shortly, the *cœlom.* Thus, from an early period in the vertebrate embryo, and in a considerable number of the invertebrate, a division of the mesoderm takes place into the somato-pleural or outer lamina, and the splanchno-pleural or inner lumina; the outer being the seat of formation of the dermal, muscular, and osseous systems —the volunto-motory of Remak; and the inner of the muscular wall of the alimentary canal, as well as of the contractile substance of the heart and the vascular system generally.

It is interesting to find that there is a correspondence between the later division of the mesoderm of the higher animals derived from the two primitive blastodermic laminæ, and the original absence of mesodermic structure in the lowest animals, followed by the gradual appearance, first of one layer (the external muscular in the higher cœlenterata), and soon afterwards by the two divisions or laminæ with the intermediate cœlom.

In this account of what may be termed the organised foundation of the new being, I have entered into some detail, because I felt that our conception of any relation subsisting between the ontogenetic history of animals and their phylogenetic evolution can only be formed from the careful study of the earliest phenomena of embryonic organisation. But, notwithstanding the many difficulties which unquestionably still block the way, I am inclined to think that there is great probability in the view of a common bilaminar origin for the embryo of all animals above the protozoa, and that the vertebrate equally with the invertebrate animals may be shown to possess in the first stages of their blastodermic or embryonic formation the two primitive layers of ectoderm and entoderm.

To attempt, however, to pursue the history of the development of animals in detail would be equivalent to inflicting upon you a complete system of human and comparative anatomy. But I cannot leave the subject abruptly without an endeavour to point out in the briefest possible manner the bearing of one or two of the leading facts in embryology upon the general relation of ontogeny and phylogeny.

We are here brought into the contemplation of those remarkable changes, all capable of being observed and demonstrated, by which the complex organisation of the body is gradually built up out of the elementary materials furnished by the blastodermic layers,—a process which has been looked upon by all those who have engaged in its study with the greatest interest and admiration. And if, by comparing these phenomena as observed in individuals belonging to different classes and orders of animals, it is found not only that they are not different, but on the contrary, that they present features of the most remarkable resemblance and conformity, we shall be led to conclude that there is a general plan of development proved to extend to the members of considerable groups, and possibly capable of being traced from one group to another. But this is clearly nothing else than another way of stating that there is a similar type of structure pervading the animals of each group, and a probability of a common type being ascertained to belong to them all. The main question, therefore, to be answered is whether there is or is not a general correspondence between the phenomena of development and the gradation of type in animal structure upon which anatomists and zoologists are agreed; and my object will now be to bring rapidly before you one or two of the most marked illustrations of the correspondence, drawn from the early history of development in the higher animals.

As one of the examples of the earlier phenomena of development I may refer to the change which is perceptible as early as the eighteenth or twentieth hour of incubation in the chick, and which is reproduced in the course of development of every member of the vertebrate sub-kingdom. It consists in the formation of cross clefts on each side of the primitive neural cavity which divide off from each other a number of segments of this wall in the length of the axis of the embryo. At first there are only one or two such clefts; but they rapidly increase in a backward direction in the body of the embryo, and as development proceeds they extend into the tail itself. These are the *protovertebræ* of embryologists, not corresponding, as might at first be supposed, with the true or actual vertebræ which are formed later, but representing in an interesting manner transverse *vertebral segments* of the body, and containing within each the elements of a great part of the structure belonging to the body-wall afterwards to be developed, including the true cartilaginous or osseous vertebral arches, and the muscular plates.

This change, however, belongs to the mesodermic lamina, and occurs in an elongated thick portion of it, which makes its appearance on each side of the primitive neural canal between the epiblast and the hypoblast. The transverse cleavage is ascertained to commence near what afterwards forms the first cervical vertebra, but does not extend into the base of the cranium. And it is most interesting to note in this cleavage the formation at so early a period of the succession of *metameres* or series of similar parts, which forms a main characteristic of vertebral organisation.

As intimately connected with the formation of the vertebral column, the appearance of the chorda dorsalis, or *notochord*, presents many points of peculiar interest in embryological inquiries.

The notochord is a continuous median column or thread of cellular structure, running nearly the whole length of the rudimentary body of the embryo, and lying immediately below the cerebro-spinal canal. It occupies in fact the centre of the future bodies of the vertebræ. It exists as a primordial structure in the embryo of all vertebrates, including man himself, and extending down to the amphioxus, and, according to the remarkable discovery of Kowalevsky in 1866, it is to be found among the invertebrates in the larva of the ascidia.[1]

In amphioxus and the cyclostomatous fishes the notochord, growing with the rest of the body into a highly developed form, acts as a substitute for the pillar of the bodies of the vertebræ, no vertebral bodies being developed; but in cartilaginous and osseous fishes various gradations of cartilaginous and osseous structures come to surround the notochord and give rise to the simpler forms of vertebral bodies, which undergo more and more distinct development in the higher vertebrates. In all instances the substance forming the vertebral bodies is deposited on the surface of or outside the notochord and its sheath, so that this body remains for a time as a vestigial structure within the vertebral bodies of the higher animals.

The observations of Kowalevsky with respect to the existence of a notochord in the ascidia, which have been confirmed by Kupfer and others, have produced a change little short of

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[1] If we reserve the words ectoderm and endoderm to designate the two layers of the primary bilaminar blastoderm, we may apply the terms epiblast and hypoblast to their derivatives after the formation of the mesoderm, and indicate the relations of the whole to the secondary or quadrilaminar blastoderm, by the accompanying Table:—

Primary Blastoderm	Ectoderm ...	....	Epiblast..........	Secondary Blastoderm.
		Mesoderm ...	Somatopleure ...	
			Splanchnopleure.	
	Endoderm ...	....	Hypoblast........	

[1] *Mém. de l'Acad. de St. Pétersbourg*, vol. x.

revolutionary in embryological and zoological views, leading as they do to the support of the hypothesis that the ascidia is an earlier stage in the phylogenetic history of the mammal and other vertebrates. The analogy between the amphioxus and ascidian larva is certainly most curious and striking as regards the relation of the notochord to other parts, and it is not difficult to conceive such a change in the form and position of the organs in their passage from the embryonic to the adult state as is not inconsistent with the supposition that the vertebrates and the ascidia may have had a common ancestral form. Kowalevsky's discovery opens up at least an entirely new path of inquiry; and we must be prepared to modify our views as to the entire separation of the vertebrates from the other groups of animals, if we do not at once adopt the hypothesis that through the ascidian and other forms the origin of the vertebrates may be traced downwards in the series to the lower grades of animal organisation.

The notochord extends a short way forward into the cranial basis, and an interesting question here presents itself, beginning with the speculations of Goethe and Oken, and still forming a subject of discussion, whether the series of cranial or cephalic bones is comparable to that of the vertebræ. On the whole it appears to me that it is consistent with the most recent views of the development and anatomy of the head to hold the opinion that it is composed of parts which are to some extent homologous with vertebral metameres.[1]

The history of the formation of the vertebral column presents an interesting example of the correspondence in the development of the individual and the race, in that all the stages which have been referred to, as occurring in the gradual evolution of the vertebral column in the series of vertebrates, are repeated in the successive stages of the embryonic development of the higher members of the series.

There is perhaps no part of the history of development in the vertebrates which illustrates in a more striking manner the similarity of plan which runs through the whole of them than that connected with what I may loosely call the region of the face and neck, including the apparatus of the jaws and gills. The embryonic parts I now refer to consist of a series of symmetrical pairs of plates which are developed at an early period below the cranium, and may therefore, in stricter embryological terms, be styled the *subcranial plates*.

Without attempting to follow out the remarkable changes which occur in the development of the nose and mouth in connection with the anterior set of these plates, which, from being placed before the mouth, are sometimes named *preoral*, I may here refer shortly to the history of the plates situated behind the mouth, which were discovered by Rathke in 1826, and formed the subject of an elaborate investigation by Reichert in 1837.

These plates consist of a series of symmetrical bars, four in number in mammals and birds, placed immediately behind the mouth, separated by clefts passing through the wall of the throat, and each traversed by a division of the great artery from the heart; thus constituting the type of a branchial apparatus, which in fishes and amphibia becomes converted into the well-known gills of these animals, whilst in reptiles, birds and mammals they undergo various changes leading to the formation of very different parts, which could not be recognised as having any relation to gill structure but for the observation of their earlier embryonic condition. The history of this part of development also possesses great interest on account of the extraordinary degree of general resemblance which it gives to the embryos of the most different animals at a certain stage of advancement—so great, indeed, that it requires a practised eye to distinguish between the embryos of very different orders of mammals, and even between some of them and the embryos of birds or reptiles, as well as in connection with the transformations of the first pair of branchial apertures, which lead to the formation of the passage from the throat to the ear in the higher vertebrata. There is equal interest attached to the history of the development of the first pair of arches which include the basis of formation of the lower jaw with the so-called *cartilage of Meckel*, and which, while furnishing the bone which suspends the lower jaw in reptiles and birds, is converted in mammals into the hammer-bone of the ear.

The other arches undergo transformations which are hardly less marvellous, and the whole series of changes is such as never fails to impress the embryological inquirer with a forcible idea of the persistence of type and the inexhaustible variety of changes to which simple and fundamental parts may be subject in the process of their development.

It is also of deep significance in connection with the foregoing phenomena, to observe the increase in the number of the gill-bars and apertures as we descend in the scale to the cartilaginous fishes and lampreys, and the still further multiplication of these metameres or repeated parts in the amphioxus; and it is, perhaps, also interesting to note that in the ascidia the arrangement of the gills is exactly similar to that of the amphioxus.

The study of the comparative anatomy of the heart and its mode of formation in the embryo furnishes also most striking illustrations of the relation between ontogenetic and phylogenetic development in the vertebrates, and is not without its applications to some of the invertebrate groups of animals.

I need only recall to your recollection the completely double state of this organ in warm-blooded animals, by which a regular alternation of the systemic and pulmonary circulations is secured, and the series of gradations through the class of reptiles by which we arrive at the undivided ventricle of the amphibian, and the further transition in the latter animals by which we come at last to the single heart of fishes; and to state that in the embryo of the higher animals the changes by which the double heart is ultimately developed out of an extremely simple tubular form into which it is at first moulded from the primitive-formative cells are, in the inverse order, entirely analogous to those which I have just now indicated as traceable in the descending series of vertebrate animals; so that at first the embryonic heart of man and other warm-blooded animals is nothing more than a rhythmically contractile vascular tube. By the inflection of this tube, the constriction of its wall at certain parts, and the dilatation at others, the three chambers are formed which represent the single auricle, the single ventricle, and the aortic bulb of the fish. By later changes a septum is formed to divide the auricles, becoming completed in all the air-breathing animals, but remaining incomplete in the higher animals so long as the conditions of fœtal life prevent the return of arterialised blood to the left auricle. The growth of another septum within the ventricular portion gradually divides that cavity into two ventricles, repeating somewhat in its progress the variations observed in different reptiles, and attaining its complete state in the crocodile and warm-blooded animals.

I must not attempt to pursue this interesting subject further, but I cannot avoid making reference to the instructive view presented by the embryological study of the nature of the malformations to which the heart is subject, which, as in many other instances, are due to the persistence of transitory conditions which belong to different stages of progress in the development of the embryo. Nor can I do more than allude to the interesting series of changes by which the aortic-bulb, remaining single in fishes, and serving as the channel through which the whole stream of blood leaving the heart is passed into the gills, becomes divided in the higher animals into the roots of the two great vessels, the aorta and the pulmonary artery, and the remarkable transformations of the vascular arches which proceed from the aortic-bulb along the several branchial arches, and which, in the gills of fishes and aquatic amphibia, undergo thati minute subdivision which belongs to the vascular distribution o gills, but which in the higher non-branchiated animals are the subject of very different and various changes in the partial obliteration of some, and the enlargement of others, by which the permanent vessels are produced.

These changes and transformations have for many years been a subject of much interest to comparative anatomists, and will continue to be so, not only from their presenting to us one of the most remarkable examples of conformity in the plan of development and the type of permanent or completed organisation in the whole series of vertebrated animals, but also because of the manifest dependence of the phenomena of their development upon external influences and atmospheric conditions which affect the respiration, nutrition, and modes of life of the animal.

Nor is the correspondence to which I now refer entirely limited to the vertebrata. For here, again, through the amphioxus and the ascidia, we come to see how an affinity may be traced between organs of circulation and respiration which at first appear to belong to very different types. The heart of vertebrates is, as is well known, an essentially concentrated form of vascular development in the ventral aspect of the body; while the heart of the invertebrate, whether in the more concentrated form existing in the articulata and muscula, or in a more subdivided shape prevalent in the annelida, is most frequently dorsal; yet the main aorta of the vertebrates is also dorsal; and it is not impossible through the intermediate form of amphioxus, to understand how the relation between the vertebrate and the invertebrate type of the blood-vascular system may be maintained.

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[1] See the interesting and valuable memoirs of W. K. Parker, "On the Anatomy and Development of the Vertebrate Skull," in *Trans.* of Roy. Soc., the researches of Gegenbaur, Mihalkovics, and more particularly the Memoir by F. M. Balfour, "On the Development of the Elasmobranchs," in the *Journ. of Anat. and Physiol.*, vols. x. and. xi.

But I am warned by the lapse of time that I must not attempt to pursue these illustrations further. In the statement which I have made of some of the more remarkable phenomena of organic production—too long, I fear, for your endurance, but much too brief to do justice to the subject—it has been my object mainly to show that they are all more or less closely related together by a chain of similarity of a very marked and unmistakable character; that in their simplest forms they are indeed, in so far as our powers of observation enable us to know them, identical; that in the lower grades of animal and vegetable life they are so similar as to pass by insensible gradations into each other; and that in the higher forms, while they diverge most widely in some of their aspects in the bodies belonging to the two great kingdoms of organic nature, and in the larger groups distinguishable within each of them, yet it is still possible, from the fundamental similarity of the phenomena, to trace in the transitional forms of all their varieties one great general plan of organisation.

In its simplest and earliest form that plan comprises a minute mass of the common nitrogenous hydrocarbon compound to which the name of protoplasm has been given, exhibiting the vital properties of assimilation, reproduction, and irritability; the second stage in this plan is the nucleated and inclosed condition of the protoplasmic mass in the organised cell. We next recognise the differentiation of two productive elements, and their combination for the formation of a more highly-endowed organising element in the embryonic germ-sphere or cell; and the fourth stage of advance in the complexity of the organising phenomena is in the multiplication of the fertilised embryo-cell, and its conversion into continuous organised strata, by further histological changes in which the morphological foundations of the future embryo or new being are laid.

I need not now recur to the further series of complications in the formative process by which the bilaminar blastoderm is developed, and becomes trilaminar or quadrilaminar, but only recall to your recollection that while these several states of the primordial condition of the incipient animal pass insensibly into each other, there is a pervading similarity in the nature of the histological changes by which they are reached, and that in the production of the endless variations of form assumed by the organs and systems of different animals in the course of their development, the process of cell-production, multiplication and differentiation remains identical. The more obvious morphological changes are of so similar a character throughout the whole, and so nearly allied in the different larger groups, that we are led to regard them as placed in some very close and intimate relation to the inherent properties of the organic substance which is their seat, and the ever-present influence of the vital conditions in which alone these properties manifest themselves.

The formative or organising property, therefore, resides in the living substance of every organised cell and in each of its component molecules, and is a necessary part of the physical and chemical constitution of the organising elements in the conditions of life; and it scarcely needs to be said that these conditions may be as varied as the countless numbers of the molecules which compose the smallest particles of their substance. But, setting aside all speculation of a merely pangenetic kind, it appears to me that no one could have engaged in the study of embryological development for any time without becoming convinced that the phenomena which have been ascertained as to the first origin and formation of textures and organs in any individual animal are of so uniform a character as to indicate forcibly a law of connection and continuity between them; nor will his study of the phenomena of development in different animals have gone far before he is equally strongly convinced of the similarity of plan in the development of the larger groups, and, to some extent, of the whole. I consider it impossible, therefore, for any one to be a faithful student of embryology, in the present state of science, without at the same time becoming an evolutionist. There may still be many difficulties, some inconsistencies, and much to learn, and there may remain beyond much which we shall never know; but I cannot conceive any doctrine professing to bring the phenomena of embryonic development within a general law which is not, like the theory of Darwin, consistent with their fundamental identity, their endless variability, their subjugation to varying external influences and conditions, and with the possibility of the transmission of the vital conditions and properties, with all their variations, from individual to individual, and, in the long lapse of ages, from race to race.

I regard it, therefore, as no exaggerated representation of the present state of our knowledge to say that the ontogenetic development of the individual in the higher animals repeats in its more general character, and in many of its specific phenomena, the phylogenetic development of the race. If we admit the progressive nature of the changes of development, their similarity in different groups, and their common characters in all animals, nay, even in some respects in both plants and animals, we can scarcely refuse to recognise the possibility of continuous derivation in the history of their origin; and however far we may be, by reason of the imperfection of our knowledge of palæontology, comparative anatomy, and embryology, from realising the precise nature of the chain of connection by which the actual descent has taken place, still there can be little doubt remaining in the minds of any unprejudiced student of embryology that it is only by the employment of such an hypothesis as that of evolution that farther investigation in these several departments will be promoted so as to bring us to a fuller comprehension of the most general law which regulates the adaptation of structure to function in the universe.

SECTION A.

MATHEMATICAL AND PHYSICAL.

Opening Address by the President, Prof. G. Carey Foster, F.R.S.

When any one fears that he has accepted a duty that is too difficult for him, or that he has allowed himself to be placed in a position, the responsibilities of which are greater than he can properly discharge, probably the very worst thing he can do is to proclaim his misgivings to the world. But though I fully believe in this rather obvious maxim, I cannot avoid saying that I enter upon my duties here to-day with very great diffidence, and that I feel the necessity of asking your indulgence at the outset for what I fear will be my inevitable shortcomings in discharging the functions of the honourable post that has been assigned to me. And I am sure that no one who calls to mind the names of some of those who, within recent years, have occupied the Chair of this Section, and who knows—however imperfectly—what those names stand for in connection with mathematics and physics, will be surprised that I should deprecate comparisons which might tend to degenerate into contrasts, or that I should shrink from having my performances measured by the standard of such predecessors. But I have neither the right nor the desire to detain you longer with this purely personal topic, and I therefore proceed to ask your attention to matters more closely connected with the business which has brought us here.

The periodically recurring character of these meetings unavoidably suggests, at each recurrence, a retrospect at the scientific work of the year, and an attempt to estimate the advances which have been the result of this work. At first sight nothing would seem to be more natural or appropriate than that each president of a section should occupy the introductory remarks, which the custom of the Association demands from him, with an account of the chief forward steps made during the past year in the branches of science represented by his Section.

Very little consideration, however, is sufficient to show that, in the case at least of Section A, to give anything like a general report of progress would be a task which few, if any, men could perform single-handed. To say nothing of the enormous *amount* of the material which is now the result of a year's scientific activity, the *variety*—or I might even say the unlikeness—of the subjects of which this Section takes cognizance is so great that, in most cases, it would be safe to conclude, from the mere fact of a man being able adequately to expound the recent advances in one of these subjects, that he must have given so much attention to this one as to have made it impossible for him to have followed carefully the progress of the rest.

But even supposing that all presidents of Section A were able to discourse with full and equal knowledge of hyper-Jacobian surfaces, the influence of temperature on the capillary constant of dilute sulphuric acid, or the latest improvement in the construction of aneroid barometers, some consideration would still be due to their audience. And, long-suffering as British Association audiences have often shown themselves to be, there is no doubt that before a tenth part could be read of a report on the year's work on the subjects included in this Section, the room would be cleared and most of those who came to hear about mathematics and physics would have gone to try whether they could not find in Section E or F something appealing more directly to the common sympathies of mankind.

But although a serious report of progress would thus be both impossible and unsuitable in the form of an Address to the Section, it remains none the less true that such reports are in themselves of the utmost scientific value, and, as has been pointed out repeatedly, there are few ways in which the British Association could more effectually fulfil its function of promoting the advancement of science than by aiding in their preparation and publication. But when one tries to think out in detail the way in which the Association could do this, the practical difficulties of the scheme are seen to be neither few nor trifling. It may be sufficient to point out that there is no evident reason why help of this kind should be afforded to one branch of science rather than to another, and that the publication of reports upon all branches would completely overtax the resources of the Association.

In the case of some important sciences, however, the work of reporting recent advances is already undertaken by other bodies, —thus there are the "Abstracts" published monthly in the *Journal* of the Chemical Society, and there are the *Zoological Record*, the *Geological Record*, and other publications of a like nature,—but hitherto nothing of the kind has been done in this country for those departments of science with which this Section is specially concerned. But without attempting to commit the Association to any burdensome outlay, or to any larger scheme than it would be practicable to carry out, it seems to me possible that a systematic series of reports might be established in connection with this Section which would have a very high value. In the early volumes of the British Association's *Transactions* we find more frequently than in recent ones, reports, not merely on some special investigation, but on the recent progress and present state of some more or less comprehensive branch of science. Thus in the first four volumes we find the following, among other reports, presented to this Section:—On the Progress of Astronomy, On the Present State of Meteorology, On the Present State of the Science of Radiant Heat, On the Progress of Optics, On the Magnetism of the Earth, On Capillary Attraction, On Physical Optics, On the Recent Progress and Present Condition of the Mathematical Theories of Electricity, Magnetism, and Heat. Now I venture to think that this form of the activity of the Association might with great advantage be revived and systematised. I would suggest, as a plan that seems to me worth consideration by the Committee of this Section, the appointment of committees charged to report to the Section periodically on the advances made in each of the chief departments of science of which we here take cognizance. For example, to confine myself specially to physics, we might have a committee on Optics, a committee on Acoustics, one on Heat, one on Electricity, and so on. It would not be in accordance with the usages of the Association to nominate these as standing committees, but they might be made virtually such by annual reappointment. I would suggest that they should not report annually, but at intervals of perhaps five or six years, the times being so arranged that different committees should report in different years, the report in each case being a systematic account of all the work of any importance done on the subject and within the period to which it related. In order not to make the work too heavy, it would probably be needful to make each committee comparatively numerous, so that individual members might each undertake to report upon some limited part of the general subject. Some one member of each committee would also require to act as editor; his function would be not merely to put together the detached fragments sent in by his colleagues, but to distribute to them the materials on which they would have to report. For this purpose it would be needful that copies of all the important scientific periodicals relating to Physics should be supplied to the committee; but besides providing these and printing the reports, I do not see that the Association need be put to any expense; and if it were thought well to sell the reports independently of the yearly volumes of the Association, probably a good part even of this expense might be recovered.

The mutual relations subsisting between the two great groups of sciences, which we discuss in this Section under the names Mathematics and Physics, offer so many deeply interesting points for consideration that, at the risk of reminding you how admirably and with what fulness of knowledge the same subject has been treated by more than one of my predecessors in this Chair, I venture to ask your attention once more to a few remarks on this topic.

The intimate connection between Mathematics and Physics arises out of the fact that all scientific knowledge of physical phenomena is based upon *measurements*,—that is to say, upon the discovery of relations of number, quantity and position, of the same kind as those which form the subject matter of mathematics. It is true that in studying physics we have to learn much about the quality of phenomena and of the conditions under which they occur, as well as about their purely quantitative relations; but even in the qualitative study of physical phenomena we find it impossible to determine what is really characteristic and to distinguish the essential from the accidental, except by the aid of measurements. In fact if we take the most elementary treatise upon any branch of physics that we can meet with, a book it may be which aims at giving a purely descriptive account of phenomena, we find, when we examine it, that numberless careful measurements have been required to establish the truth of the merely qualitative statements which it contains. To take a simple and well-known example, the old question whether the ascent of water in a pump was due to the pressure of the atmosphere, or to Nature's horror of a vacuum, was not conclusively settled by Torricelli's discovery that mercury would not rise beyond a certain height in a glass tube, even to prevent a vacuum being formed at the top of it, for the same thing was already known about the water in a pump. But, when he measured the height of the mercury-column in his tube and found that if he multiplied it by the specific gravity of mercury, the product was equal to thirty-two feet, the height to which, as Galileo said (probaby between jest and earnest) nature's abhorrence of a vacuum in a pump extended, it was clear that the ascent both of water and of mercury depended upon the particular depth of each liquid that was needed to produce some definite pressure; and when Pascal had persuaded his brother-in-law to carry a Torricelli's tube to the top of the Puy de Dôme, and he had measured the height of the mercury-column at the top of the mountain as well as at the foot, the proof was completed that the pressure which determined the height of both the water and the mercury was the pressure of the atmosphere.

Again, let us examine a still more familiar phenomenon, the falling of heavy bodies to the ground. So long as we consider this merely under its general, or, as we may call them, its qualitative aspects, we might reasonably infer that it is the result of some inherent tendency of bodies; and, so far from its seeming to be true, as stated in Newton's "First Law of Motion," that bodies have no power to alter their condition of rest or of motion, we might infer that however indifferent they may be as to horizontal motion, they have a distinct tendency to move downwards whenever they can, and a distinct disinclition to move upwards. But when we measure the direction in which bodies tend to fall and the amount of the tendency in different places, and find that these vary in the way that they are known to do with geographical position and distance from the sea-level, we are obliged to conclude that there is no inherent tendency to motion at all, but that falling is the result of some mutual action exerted between the earth and the falling body. For if we suppose falling to be due to any internal cause, we must imagine something much more complicated than a mere tendency to motion in one direction, else, how could a stone that has always fallen in one direction in England, fall in almost exactly the opposite direction as soon as it is taken to New Zealand?

These two simple examples illustrate a principle that we meet with throughout Physics: namely that, in the investigation of the causes of physical phenomena, or in other words, of the connection between these phenomena and the conditions under which they occur, the really decisive guidance is afforded by the study of their measurable aspects.

The consequence is that from the very outset of his investigations the physicist has to rely constantly on the aid of the mathematician, for even in the simplest cases, the direct results of his measuring operations are entirely without meaning until they have been submitted to more or less of mathematical discussion. And when in this way some interpretation of the experimental results has been arrived at, and it has been proved that two or more physical quantities stand in a definite relation to each other, the mathematician is very often able to infer, from the existence of this relation, that the quantities in question also fulfil some other relation, that was previously unsuspected. Thus when Coulomb, combining the functions of experimentalist and mathematician, had discovered the law of the force exerted between two particles of electricity, it became a purely mathematical problem, not requiring any further experiment, to ascertain how electricity is distributed upon a charged conductor, and this problem has been solved by mathematicians in several cases.

It thus happens that a very large part of our knowledge of physics is due in the first instance to the mathematical discussion of previous results, and is experimental only in the second, or perhaps still more remote degree.

Another way in which the mathematician co-operates in the discovery of physical truths is almost exactly the converse of that last-mentioned. In very many cases the most obvious and direct experimental method of investigating a given problem is extremely difficult, or for some reason or other untrustworthy. In such cases the mathematician can often point out some other problem more accessible to experimental treatment, the solution of which involves the solution of the former one. For example, if we try to deduce from direct experiments the law according to which one pole of a magnet attracts or repels a pole of another magnet, the observed action is so much complicated with the effects of the mutual induction of the magnets and of the forces due to the second pole of each magnet, that it is next to impossible to obtain results of any great accuracy. Gauss, however, showed how the law which applied in the case mentioned can be deduced from the deflections undergone by a small suspended magnetic needle when it is acted upon by a small fixed magnet placed successively in two determinate positions relatively to the needle; and being an experimentalist as well as a mathematician, he showed likewise how these deflections can be measured very easily and with great precision.

It thus appears not only that mathematical investigations have aided at every step whereby the present stage in the development of a knowledge of physics have been reached, but that mathematics has continually entered more and more into the very substance of physics, or, as a physiologist might say, has been assimilated by it to a greater and greater extent.

Another way of convincing ourselves how largely this process has gone on would be to try to conceive the effect of some intellectual catastrophe, supposing such a thing possible, whereby all knowledge of mathematics should be swept away from men's minds. Would it not be that the departure of mathematics would be the destruction of physics? Objective physical phenomena would, indeed, remain as they are now, but physical science would cease to exist. We should no doubt see the same colours on looking into a spectroscope or polariscope, vibrating strings would produce the same sounds, electrical machines would give sparks, and galvanometer needles would be deflected; but all these things would have lost their meaning; they would be but as the dry bones—the *disjecta membra*—of what is now a living and growing science. To follow this conception further, and to try to image to ourselves in some detail what would be the kind of knowledge of physics which would remain possible, supposing all mathematical ideas to be blotted out, would be extremely interesting, but it would lead us directly into a dim and entangled region where the subjective seems to be always passing itself off for the objective, and where I at least could not attempt to lead the way, gladly as I would follow any one who could show where a firm footing is to be found. But without venturing to do more than look from a safe distance over this puzzling ground, we may see clearly enough that mathematics is the connective tissue of physics, binding what would else be merely a list of detached observations into an organised body of science.

In my opinion, however, it would be a very serious misconception to suppose that on this account an elaborate apparatus of technical mathematics is in general needful for the proper presentation of physical truths. The ladders and ropes of formulæ are no doubt often essential during the building up of a newly-discovered physical principle, but the more thoroughly the building is finished, the more completely will these signs that it is still in progress be cleared away, and easy ascents be provided to all parts of the edifice. In an address delivered from the Chair of this Section four years ago, Prof. Henry Smith quoted the saying of an old French geometer, "that a mathematical theory was never to be considered complete till you had made it so clear that you could explain it to the first man you met in the street." Very likely Prof. Smith was right to call this "a brilliant exaggeration," at any rate I know of no reason for disputing his opinion, but I believe the exaggeration would really be very small if the dictum were applied to the theories of physics instead of to those of pure mathematics. When a physical principle or theory is grasped with thorough clearness, I believe it is possible to explain it to the man in the street; only he must not be hurrying to catch a train; and it would, I think, be difficult to find a more wholesome maxim to be kept in mind by those of us whose business it is to teach physics, than that we should never think we understand a principle till we can explain it to the man in the street. I do not say that our modes of exposition should always be adapted to him, for as a rule, he forms but a small part of our audience, but even when the conditions are such that a teacher is free to avail himself to the fullest extent of mathematical methods, I believe he would find his mathematical discussions gain marvellously in freshness and vigour if he had once made up his mind how he would treat his subject supposing all use of mathematical technicalities denied him.

So far, in considering the mutual relations of mathematics and physics, I have placed myself, as it was natural for me to do, at a physical point of view, and, starting from the fact that the existence and progress of the latter science are essentially dependent upon help derived from the former, I have tried to point out some of the ways in which this help is rendered. If we turn now to inquire in what light the relations between the two sciences appear from the side of mathematics, we find that mathematicians are not slow to admit the advantages which their science derives from contact with physics. It was a saying of Fourier that "a more attentive study of nature is the most fruitful source of mathematical discoveries;" and Prof. Henry Smith, in the Address I have already referred to, says that "probably by far the greater part of the accessions to our mathematical knowledge have been obtained by the efforts of mathematicians to solve the problems set to them by experiment." We may perhaps regard such expressions as equivalent to the statement that the law of inertia is not without application even to the mind of the mathematician, and that it, too, continues to move in a straight line "except in so far as it may be compelled by impressed forces" to change its direction; or, to put the matter a little differently, may we not look upon the fact as illustrating what is probably a general principle of mental action, namely, that the human mind has no more power to create an idea than the hand has to create matter or energy—our seemingly most original conceptions being in reality due to suggestions from without? But however this may be, the fact remains that the origin of many most important mathematical theorems, and even entire departments of mathematics, can be distinctly traced to the attempt to express mathematically the observed relations among physical magnitudes. By way of illustration of this statement, it may suffice to refer to the well-known cases of the theory of fluctions, to Fourier's theorem and the doctrine of harmonic analysis, to spherical harmonics, and to the theory of the potential.

The way in which physics reacts, so as to promote the advancement of a knowledge of mathematics, finds in many respects a close parallel in the influence exerted by the practical industrial arts on the progress of physics. This influence shows itself very distinctly, first, in the new conceptions and new points of view which practical pursuits supply to scientific physics, and, secondly, in the new subjects and opportunities which they offer for physical investigation.

A very remarkable and important example of the former kind of influence is afforded by the idea of Work and the correlative one of Energy. These ideas, which have been found to have a most far-reaching significance, and have exerted a transforming effect upon every branch of physics, owe their recognition, not to the spontaneous growth of science, but to their having been forced on the attention of physicists by the cultivators of practical mechanics.[1] Very much the same thing may also be said of the modern conception of the nature of heat, and of the relation between thermal phenomena and those of other branches of physics. The notion of heat as a measurable magnitude, of which definite quantities could be given to or taken away from bodies, was fully established by the researches of Black and Wilke on latent and specific heat. This was at the time when the idea of chemical composition was just taking its modern shape through the recognition of aëriform bodies as possible constituents of solids and liquids, and it was natural that the new knowledge with regard to heat should be embodied in the conception of a matter of heat, or caloric, capable of entering into or separating from combination like fixed air or dephlogisticated air. And in fact this conception not only took the place of philosophical speculations upon the nature of heat such as those of Bacon and Locke, but it withstood the experimental onslaughts of Rumford and Davy, as well as the penetrating scientific criticism of Thomas Young. It is to the steam-engine, and to the attempt to find out the connection obviously existing between the amount of heat supplied and the work done by the engine, that we must trace the downfall of the idea of the materiality of heat and the origin of our modern views.[1]

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[1] See, on this point, Dühring, "Kritische Geschichte der allgemeinen Principien der Mechanik" (Berlin, 1873), pp. 483–486.

Probably it would be impossible to find a more remarkable instance of what I referred to just now as the second way in which practice may react upon science so as to promote its advancement, than is presented to us in the case of electric telegraphy. This is an example of an industrial undertaking which is the direct offspring of scientific research, and could not have co-existed in its actual state of development with a less advanced condition of electrical science; but if it were possible to establish any common measure for such things, it may be doubted whether it would not be found that telegraphy has repaid to science benefits equal to those it has received. For instance, the discovery of earth-currents was a direct result of the large scale of the instrumental arrangements which are needed for telegraphic purposes, and is one which would probably have long remained unmade in the absence of some inducement to make experiments on a scale greater than that indicated by the visible wants of scientific inquiry. The same is true of the discovery of the influence of electro-static induction upon the transmission of electric currents through metallic conductors, and of the consequent additions to our knowledge of the specific inductive capacity of insulators and of the whole subject of electrostatic capacity. But by far the most important of the benefits conferred by electric telegraphy upon electrical science have resulted from the necessity under which the practical electrician found himself, of not only being able to produce certain results, but of producing them under definitely ascertained conditions as to the expenditure of time and material. When it was perceived that slight variations in the electrical conductivity, insulating power, or specific inductive capacity of certain materials might affect the pecuniary return upon investments reckoned in millions of pounds sterling, measuring instruments were devised which far surpassed in delicacy and accuracy those that had been previously made for purely scientific purposes, or the cost of which exceeded the means usually at the disposal of scientific investigators. The multiplication and wide diffusion of such instruments has led to the rapid accumulation of numerical data of great scientific importance, and has largely contributed to the spread of accurate conceptions as to the quantitative laws of electrical phenomena. But the further necessity experienced by practical electricians, that, besides being able to make accurate measurements, they should be able mutually to communicate and to understand each other's results, has probably done more than anything else to hasten the introduction for scientific purposes of so-called "absolute" measurements, instead of mere comparisons of each quantity to be estimated with a standard magnitude of its own kind. The use of absolute measures constitutes one of the most characteristic differences between the physics of to-day and that of the time when the British Association was instituted, and it may be even said to lie at the base of the doctrine of the Conservation of Energy, which implies the principle that every kind of energy can be reduced to the same denomination.

Perhaps, after speaking as I have done of the necessity for the co-operation of mathematics in the advancement of physics, it is not inappropriate that I should, in conclusion, refer to the possibility that, by a too implicit reliance upon mathematical guidance, the physicist may be led away from the discovery of fresh truth, or even into actual error. Mathematics is seen to be so indispensable and usually so powerful an aid in physical investigation, that there is a danger of forgetting that there are after all limits to its power. Partly from want of sufficient knowledge of the physical data on which mathematical discussion must be based, and partly from the imperfection of mathematical methods themselves, it happens that it is not possible to give a thoroughly complete mathematical account of even the simplest physical phenomenon. In all real cases, although some one effect may often predominate so greatly as alone to attract attention on a cursory view, the actual complexity is so great that it is only by deliberately leaving out of consideration what we believe to be the accidental accompaniments of a phenomenon, and confining our attention to what seems to be its essential and characteristic part that it is possible to make it the subject of mathematical calculation. The consequence is that the problems treated of in mathematical physics are not the problems presented by nature, but are problems suggested by these, and derived from them by a process of ideal simplification. There is, therefore, always a possibility that, in this simplifying process, some apparently trivial but really important feature of the actual phenomenon, to which the ideal one is meant to correspond, may have been overlooked. When this is the case, the fact will reveal itself sooner or later by the occurrence of discrepancies between the results of mathematical theory and those of experimental investigation. Such discrepancies are the finger-posts, pointing to new discoveries; but the experimenter who forgets the inevitable limitation of the authority of theoretical conclusions, arising from the conditions I have alluded to, is apt to disregard them, and, perhaps conscious of laziness and want of care in his method of working, or sometimes from a want of proper self-confidence, he attributes all anomalous results to "the unavoidable errors of observation."

Two classes of experimenters are safe from falling into this danger. There are first, those who, the first time they observe anything that is not provided for in their text-books, conclude that the law of gravitation ought to be reconsidered. Secondly, there are those who, with scrupulous care, take account of all the conditions which are known to be able to affect the phenomenon they are investigating, and are thus able to say, with well-founded confidence, when they meet with some unforeseen result, that it must indicate the operation of some unrecognised cause.

A brilliant example of this latter mode of working and of the discoveries to which it may lead has recently been afforded to us by the researches of Mr. Crookes, some of whose results, as embodied in the now well-known instrument which he has called the *radiometer*, have attracted much attention. It has appeared to me however, that the surprising nature of these results has to some extent called off attention from the remarkable character of the scientific investigation which led to them, and it was at one time my intention to take advantage of the present opportunity for the purpose of trying, on the one hand, to render to Mr. Crookes the credit which I think his researches deserve, and, on the other hand, to give a connected account of the further investigations, both experimental and theoretical, to which these researches have given rise. There seemed to be the more reason for endeavouring to carry out the former part of my intention, inasmuch as an eminent and accomplished scientific man had published, within the last few months, an account of the discovery of the radiometer, the unmistakable tendency of which was, either intentionally or unintentionally, to depreciate Mr. Crookes's merits, and to make it appear that he had put a wrong interpretation upon his own results. I found, however, that the time at my disposal would not enable me to make myself sufficiently master of the whole subject to treat it in the way that I wished, and I have therefore been obliged to content myself with merely making this allusion to it as an illustration of the more general considerations to which I have ventured to ask your attention.

SECTION B.

CHEMICAL SCIENCE.

OPENING ADDRESS BY THE PRESIDENT, PROF. ABEL, F.R.S.

THE subject which my predecessor in the honourable position of President of this Section, made the chief topic of his interesting and instructive address, affords excellent illustrations of the operation of purely scientific research in creating and developing important branches of industry. Mr. Perkin, whose name has from the very commencement of the history of coal-tar colours been identified with their discovery and their scientific and technical history, referred to several series of researches, each one of which formed a link in a chain of discoveries in organic chemistry of the highest value, as establishing, illustrating, or extending important chemical theories, but, at the time, and for long afterwards, of value purely from a scientific point of view. These researches, undertaken and pursued by ardent and philosophical investigators under more or less formidable difficulties, and solely in the interests of science, resulted in the discovery of certain organic bodies which were produced originally only on a very small scale and at great cost, but which, after the lapse of years, have been readily manufactured from abundant sources, and have constituted important elements in the development of the industry of artificial colouring matters. In fact, this industry, which owes its origin to the discovery of mauve by Mr. Perkin about twenty years ago, and which is second to no branch of chemical industry in regard to the rapidity of its development, and its influence upon other important branches of manufacture, affords more copious illustrations than any other of the immediate influence of pure science upon industrial progress. It therefore affords a topic which the chemist may well be excused for continually recurring to, with an interest bordering on enthusiasm, when illustrating the material advantages which accrue to communities from the promotion of scientific training and the encouragement of chemical research.

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[1] Conf. Dühring, *loc. cit.*

The iron and steel industry presents a great contrast to that of the artificial colours in regard to the extent of influence which the labours of purely scientific investigators have exerted upon its development. The efforts of scientific men to unravel such problems as, for instance, the true chemical constitution of steel, or the precise differences between the various combinations known as cast iron, and the conditions which determine their individual production or conversion from one to another, have hitherto been attended by results not at all proportionate to the patient experimental investigation of which from time to time they have been made the subject. Thus, the protracted experiments and discussion carried on by Frémy and Caron some years back, with reference to the dependence of the characteristics of steel upon the existence in it of nitrogen, cannot be said to have led to results of a more conclusive or even definite nature, regarding the conditions which regulate the production, composition, and properties of steel, than those arrived at by previous distinguished experimenters;—and the same must be said, with respect to cast iron, of such experiments as those carried on for several years by Matthiessen (in which I also took some part) under the auspices of the Association, with the view to eliminate many existing points of doubt regarding the chemical constitution of cast iron, by preparing chemically pure iron, and studying its combination with carbon and other elements occurring in cast iron.

The prosecution of purely scientific investigation may, therefore, of itself fail to bear *direct* fruit in regard to the development of new metallurgic achievements, or even to the elucidation of the comparatively complicated and numerous reactions which occur in furnaces, either simultaneously or in rapid and difficultly controllable succession, between materials composed of a variety of constituents in variable proportions. There can, however, be no question regarding the important benefits which have accrued from the application of chemical knowledge to the study and the perfection of furnace-operations by those who happily combine that knowledge with practical experience, and with the power of putting to the test of actual practice, the results of reasoning upon an intelligent observation of the phenomena exhibited in such operations, and upon the data which chemical analysis has furnished. In the hands of such men, the scientific results arrived at by Karsten, Berthius, Bunsen, Scheerer, Percy, and other eminent investigators, acquire new value, and by them the fruits of the labours of the patient toiler at analytical processes meets with that appreciation which their solid and permanently valuable work does not always command at the hands of their numerous brother-workers in chemical science, who follow the far more attractive paths of organic research.

Naturally, the brilliant results achieved from time to time by investigators in organic chemistry, the rapidity with which, by those results, theories are established or extended, types founded, their offspring multiplied, and their connection with other families traced and developed, impart to organic research a charm peculiarly its own. This, and the general ease with which new results are obtained by the pursuit of methods of research comparatively simple in their nature and few in kind, have for many years not only secured to organic chemistry an overwhelming majority of workers, they also appear to have had a tendency to lead the younger labourers in the field of organic research to under-estimate the value and importance, in reference to the advancement of science, of the labours of the plodding investigator of analysis. Yet no higher example can be furnished of the patient pursuit of scientific work purely for its own sake than that of the deviser or improver of analytical processes, who, undeterred by failure upon failure, indefatigably pursues his laborious work, probing to its foundation each possible source of error, carefully comparing the results he obtains with those furnished by other methods of analysis, and patiently accumulating experimental data, till they suffice fully to establish the value and trustworthiness of the process which he then publishes for the benefit of his fellow-workers in science. Truly, the results of such labours do not stand in unfavourable contrast, from whatever light they may be viewed, to those of the investigator of organic chemistry. It is not to be denied that the labourer at organic research may, so far as the analytical work which should fall to his share in the course of his investigations is concerned, be tempted to reduce this, the least attractive portion of his work, to within the smallest possible limits; and having, for example, by a boiling point determination, or a single analytical operation of the simplest kind, such as the examination of a platinum-salt, obtained a numerical result approximative to that which his theory demands, may hasten on to the further development of his airy structure, possibly not without risk to its stability. Unquestionably there are instances of frequent occurrence, in the pursuit of a particular line of organic research, in which more is not required than the identification of a particular product by some such simple means as above indicated. It is certain moreover that the labours of the organic investigator also not unfrequently afford bright examples of indomitable perseverance under formidable difficulties, and this alone should constitute a strong bond of union between the worker in organic research and his brother worker in analytical chemistry, if one did not already exist in the active interest which each, if a true lover of science, must take in the work of the other.

It has been remarked by one of the most distinguished investigators and, at the same time, one of the most brilliant lecturers and successful teachers of our time, that the contrivance of a new and good lecture-experiment may rank in importance with the preparation of a new organic compound; and it may certainly be said with equal truth that the elaboration of a new and good method of analysis may rank in importance with a good research in organic chemistry, in reference both to the part it plays in the advancement of science, and to its influence upon industrial progress.

An excellent illustration of this is afforded by reference to the Proceedings of the British Association when it met in this town thirty-six years ago. In a letter to Dr. Playfair, Liebig, who took a very active part in the proceedings of the Association in the earlier years of its existence, reports that Doctors Will and Varrentrapp have devised an excellent method for determining the amount of nitrogen in organic bodies, "very exact and easily performed." He then described in a few lines the process so well known to chemists, which not only has been, and continues to be, invaluable to those engaged in organic research, but which, as may be testified by such researches as those of Lawes and Gilbert, has borne a most important and indispensable part in the advancement of agricultural chemistry. It is, I believe, but an expression of the unanimous conviction of chemists to say that the achievements in analytical chemistry of such men as Berzelius, Heinrich, Rose, and Fresenius, take equal rank with the brilliant researches and theoretical expositions of such chemists as Liebig, Laurent, Gerhardt, and Berthelot; and that of all the important contributions to the development of organic chemistry which we owe to Liebig, there is none which has exerted so great an influence on the progress of this branch of chemical science as his beautifully simple method of organic elementary analysis.

Reverting to the industry of iron and steel, which, in regard to some of the most important branches, cannot fail to be a subject of special interest in Plymouth and Devonport, it is not difficult to demonstrate that the labours of the analytical chemist have exercised, and continue to exert, an important influence on the very considerable advance which has in recent years been made, and still proceeds towards securing complete control over the quality and character of the products obtained. The epoch is well within the recollection of chemists of my generation, when the British iron-master first awoke to the benefits which might accrue to him from an application of the labours of the analytical chemist in connection with iron-smelting.

When the last great stride was made in the manufacture of cast-iron by the introduction of the hot blast, the iron-smelter was naturally led to seek profit, to the fullest extent, with respect both to the great increase in the rate of production of pig-iron attainable thereby and to the economy achievable in regard to the proportions and characters of the materials employed in the production of pig-iron. But after a time the great falling-off in the quality of a large proportion of the products of the blast-furnace, and the difficulties experienced in the production of malleable iron of even very moderate quality, aided by the great impetus to competition in respect of quality, given by the first International Exhibition in 1851, directed the attention of our more enlightened iron-masters to the likelihood of their deriving important aid from chemical science, and more especially from the investigations of the analytical chemist.

Among the earliest to realise the importance of trustworthy and detailed information regarding the composition of the iron ores of the country was Mr. S. H. Blackwell, who, in presenting to the Royal School of Mines a very extensive and interesting series of British ores which he had collected with great labour and expense for exhibition in 1851, placed at the disposal of Dr. Percy the requisite funds for engaging the services of competent analysts (Messrs. J. Spiller and A. H. Dick) who, under his direction and with subsequent pecuniary aid from himself and from Government funds, carried out a very careful and complete examination of this series, the results of which have been of great value, for purposes of reference, to those actively interested in the iron industry. It was, however, the first connection of Messrs. Nicholson and D. S. Price and of Mr. E. Riley with two of the most important iron works of this country, about a quarter of a century ago (*i.e.* at the time when the above investigation was commenced), that marked, I believe, the commencement of systematic endeavours to apply the results of analytical research to the improvement and regulation of the quality of the products of our iron works.

It is, perhaps, but natural that the primary object sought by applications of the knowledge of the analytical chemist should have been to eliminate or reduce the existing elements of uncertainty in obtaining the most abundant yield of pig-iron capable of conversion into railway-bar sufficiently good to meet the minimum standard of quality, and to reduce still further the cost of production of such bar-iron by utilising materials concerning the composition of which (richness in iron, &c.) the iron-smelter was completely in the dark. The information accumulated by the analyst respecting the composition of the ores, fuel and fluxes available at the works, and the composition of the pig-iron and slags or cinders, produced under varied conditions, in regard to materials employed, and to the proportion of ore, fuel, and flux used in the blast furnace, could not, however, exist long without exerting a marked beneficial influence upon the quality of iron produced, and generally upon the iron industry of the country.

Percy's invaluable work of reference on Metallurgy furnishes abundant evidence of the scientifically interesting, as well as practically useful, nature of the results obtained at that time by the chemists above named, and others, working under Dr. Percy, with respect both to the elaboration of important analytical processes (in which direction Mr. Riley has continued to the present day to do valuable work) and to the elucidation of the reactions occurring in the processes of reduction and refining of the metal. It is needless to dwell upon the fact that the aid of the analyst has now long since become absolutely indispensable to the iron and steel manufacturer; but I may, perhaps, be allowed briefly to refer to one or two recent illustrations of the indispensable part which analytical research has played, and continues to play, in the extension of our knowledge of the chemical reactions involved in the production of cast and wrought iron and of steel, and of the influences which the chief associates of iron in its mercantile forms exert upon its physical characters.

Among the many valuable communications made to that most important body, the Iron and Steel Institute of Great Britain, by men who combine great practical knowledge and experience in iron and steel manufacture with high attainments in mechanical science and such knowledge of chemical science as insures a full appreciation of its value at their hands, one of the most interesting and suggestive to the chemist is that on the separation of carbon, sulphur, silicon, and phosphorus in the refining and puddling furnace and in the Bessemer Converter, contributed to the *Transactions* of the Institute's recent meeting, by Mr. Lowthian Bell, whose valuable investigations in connection with the iron industry are as interesting to the chemist as they are useful to the manufacturer. Mr. Bell has brought together the results of an extensive series of practical experiments on the treatment of different kinds of pig-iron of known composition, in the finery, the puddling-furnace, and the Bessemer Converter, and, by comparing the results of analytical investigation of the products of those experimental operations with each other and with those of the materials operated upon, he has obtained valuable confirmation of the views already held by metallurgic chemists regarding the succession in which carbon, silicon, sulphur, and phosphorus are attacked when pig-metal is submitted to the above purifying processes, and the extent to which those foreign associates of iron are abstracted or resist removal, by the more or less thorough application of those several modes of treatment. He has also thrown new light on the reasons why the most difficultly-available impurity, phosphorus, obstinately resists all attempts to effect even a slight diminution in its amount by application of the Bessemer treatment. The earnestness with which Mr. Bell wages war against this enemy of the iron-master in one of its most favourite haunts, the Cleveland District, not simply with the old British pluck, which acknowledges not defeat, but systematically, on scientific principles, calling to his aid all the resources which the continual advances in applied mechanical and chemical research place within his reach, cannot fail to contribute importantly, if it does not of itself directly lead, to the complete subjection of this most untractable of the associates to which iron becomes linked in the blast-furnace. Indications have lately not been wanting that the existence of phosphorus in very notable proportion in iron may not of necessity be inimical to its conversion into steel of good quality, and it may be that this element, which is now turned to useful account to impart particular characteristics to the alloys of copper and tin, is even destined to play a distinctly useful part in connection with the production of steel possessed of particular characters valuable for some special purpose.

In the great development which steel manufacture has received within the last few years, one most prominent feature has been the production, with precision, upon a large scale, of steel of desired characteristics, in regard to hardness, &c., by first adding to fluid cast-iron of known composition the requisite proportion of a rich iron ore (with or without the addition of scrap iron) to affect a reduction of the carbon to the desired amount, concurrent with a refining of the metal by the oxidising action of the ore, and then giving to the resulting steel the desired special qualities by the addition of suitable proportions of iron compound of known composition, rich in manganese and carbon (Spiegeleisen and the similar product called ferro-manganese). The germ of this system of producing steel varieties of predetermined characteristics exists in crucible processes like that of Uchatius, which have been in more or less extensive use for many years past, but it is to such invaluable arrangements as are most prominently represented by the Siemens-Martin Furnace—wherein several tons of metal may be fused and maintained at a very high temperature with a little liability to change from causes not under control, as if the operation were conducted in a crucible—that we are indebted for the very great expansion which the direct application of the analytical chemist's labours to the development of the steel industry is now receiving.

The production of steel upon the open hearth, to the elaboration of which Dr. C. K. Siemens has so largely contributed since he first established the process at Llandore in 1868, has in fact, become assimilated in simplicity of character and precision of results to a laboratory operation, and may be justly regarded as a triumph of the successful application of chemical principles and of the power of guidance and control afforded by utilising analytical research, to the attainment of prescribed results upon a stupendous scale, with an accuracy approaching that which the experienced chemical operator secures in the laboratory upon a small scale, under conditions which he can completely control. The production of steel by a large number of small separate operations in pots has now become supplanted with great advantage by the Siemens-Martin system of working at some of our largest establishments at Sheffield; this system has also secured a footing at highly renowned Continental works, which are formidable competitors with us in the manufacture of steel, such as those of Essen, Creusot, and Terrenoire. It is specially interesting to notice that, in the hands of those who, on the Continent at least equally with ourselves, have learned to combine the results of practical experience with the teachings of chemical science, the facilities now existing for dealing in a single receptacle with large masses of fluid steel have greatly facilitated the application of chemical means to the production of *solid* masses of considerable size, thereby reducing, if not altogether dispensing with the necessity for submitting large steel castings to costly mechanical operations with the object of closing up cavities caused by the escape of occluded gas as the liquid metal cools. The success in this direction which appears to have attended the addition of silicon, in combination with iron and manganese, to the steel before casting in preventing the formation of so-called *blow-holes*, and in contributing at the same time to the production of the particular character of steel required, bids fair to be of special importance in connection with the application of steel to the production of projectiles for use against armour-plates, as affording ready and comparatively very economical means of ensuring the production of perfectly sound castings, or which in compactness of structure will, it is asserted, compete successfully with carefully forged castings, and even with the magnificent material which Whitworth produces by submitting the fluid metal to powerful pressure.

The part which silicon plays by its comparatively high susceptibility to oxidation, in promoting the production of sound steel castings is readily intelligible, but the functions of the manganese compounds which are an indispensable adjunct to the *Bessemer* process, and the application of which has become an integral part of steel manufacture, are still far from being thoroughly understood, and there is ample scope for chemical research, in co-operation with practical experiment, in the further study of the influence not only of manganese in the production, and upon the properties of steel, but also of elements such as titanium, tungsten, and boron, and of chromium, which exists, associated in considerable quantities with iron, in a very abundant Tasmanian ore, to which prominent attention has lately been directed. The achievements of the mechanical engineer have so facilitated the handling and perfected the means of production and the mechanical treatment of malleable iron and of steel, that the full advantage may now be reaped of any improvement of a chemical nature which may be effected in the production of those materials; and it must be a source of pride to the chemist to observe with what success the teachings of his science are being applied by practical men of the present day in the construction of furnaces capable of withstanding the high temperatures required for the production and working of iron and steel in large masses, and in combining the perfect consumption and consequent great economy of fuel with the attainment of those high temperatures and with a thorough control over the character of the gaseous agents to which the fluid metal is exposed in the furnace. I need not quote the names of those men who have already rendered themselves prominent by their services in this particular direction, but may refer in special illustration of the results achieved by purely practical men to the success in applying very simple furnace-arrangements to the attainment of the above results which has recently attended the labours of Mr. William Price, a principal foreman in the Royal Gun Factories at Woolwich.

A few experiments made in the early days of the application of armouring to ships and forts appeared to demonstrate on the one hand that steel was quite incapable of competing with malleable iron of even very moderate quality as a material for armour-plates, and, on the other hand, that the penetrative power of projectiles made of chilled iron upon the Palliser system could not be surpassed or even attained with any degree of certainty, by projectiles of steel produced at comparatively very great cost. But some recent results obtained on the Continent, and especially in the course of the important experiments instituted by the Italian Government at Spezzia, have afforded decisive indications that steel, the application of which to the construction of ordnance has since that time been very greatly extended, may now be looked to hopefully as capable of affording greater protection against the enormous projectiles of the present day than can be secured by proportionately large additions to the stupendous iron-armouring of the most modern ironclads, and also as applicable at a cost very moderate, when compared with that of ten years ago, to the production of projectiles of large dimensions superior in point of penetrative power and of uniformity in this respect to those of chilled iron, the difficulties in the production of which are very greatly increased by the formidable increase which has lately been made in their size. Promising results have also quite recently been obtained at Shoeburyness with a new system of applying steel in conjunction with malleable iron, by which a perfect union of the two materials at one of their surfaces is effected by the aid of heat.

The superiority of soft and very homogeneous steel over wrought iron of the best quality in regard to lightness, combined with strength and toughness, are leading to its very advantageous employment in the construction of a particular class of vessels for the navy; and the perfect confidence which can be placed in the uniformity in structure and strength of steel of such character as is produced by the Whitworth system of manufacture has greatly facilitated the production of air-chambers of small weight, but capable of being quite safely charged with sufficient air, under a pressure of 1,000 pounds on the square inch, to carry the Whitehead torpedo through water to a distance of 1,000 yards in little more than a minute and a half.

Thus, the results of the recent development of steel-industry, to which the labours of the chemist have not unimportantly contributed, give promise of erecting a great influence upon the resources of nations for defence and attack. Although the necessity for the continual expansion of such resources cannot but be deeply deplored, there can be no doubt that the problems which it presents, and the special requirements to which it gives rise, must operate, and perhaps as importantly as the demands created by peaceful industries and commercial enterprise, in encouraging the metallurgist, the chemist, and the engineer to continue their combined work in following up the successes, to the achievement of which the results of scientific research have greatly, though indirectly, contributed.

If it were necessary to add to the illustrations which Mr. Perkin gave in his address last year of the practical fruits of research in *organic* chemistry, I might be tempted to dilate upon the importantr esults which have, especially during the last ten years, grown out of the discovery and study of the products of the action of nitric acid upon cellulose and glycerine. During the six years which have elapsed since I had the honour of bringing before the members of the British Association the chief points of scientific interest and practical importance presented by the history of those remarkable bodies, their application to technical and war purposes has been greatly developed. Nitro-glycerine and gun-cotton may now be justly classed among the most interesting examples of the practical importance frequently attained by the results of chemical research, while the history of the successive steps by which their safe manipulation and efficient application have been developed affords more than one striking illustration of the achievements effected by combined physical and chemical research in the solution of problems of high scientific interest and practical importance, and in the vanquishment of difficulties so formidable as for a time to appear fatal to the attainment of permanently practical success.

It is to a careful study of the influence which the *physical* character of gundowder (its density, hardness, &c.) and its *mechanical* condition (*i.e.*, form and size of the masses and condition of their surfaces), exert upon the rapidity of its explosion under confinement, that we chiefly owe the very important advance which has been made of late years in controlling its explosive force; in its applications as a propelling agent, and the consequent simple and effectual means whereby the violence of action of the enormous charges now used in siege and ship-guns is effectually reduced to within their limits of endurance without diminution of the total explosive force developed. But, concurrently with these important practical results, the application of combined chemical and physical research to a very extended and comprehensive investigation of the action of fired gunpowder has furnished results which possess considerable interest from a purely scientific point of view, as in many respects modifying, in others supplementing, the conclusions based upon earlier experiments and theoretical considerations with respect to the nature and proportions of the products formed, the heat developed by the explosion, the tension of the products of combustion with the conditions which regulate it both when the explosion is brought about in a close vessel and when it occurs in the bore of a gun. The results of these physico-chemical researches have, moreover, already acquired practical importance in regard to the light they have thrown upon the influence exerted by variable conditions of a mechanical nature upon the action of and pressure developed by fired gunpowder in the bore of a gun, and in demonstrating that modifications in the *composition* of gunpowder, not unimportant from an economical point of view in dealing with the very large charges now employed, may importantly contribute to render the storing of the maximum of work in the projectile, when propelled from a gun, compatible with a subjection of the gun to comparatively very moderate and uniform strains.

Other interesting illustrations of the intimate manner in which physical and chemical research are linked together, and of the important extent to which some of our most illustrious workers in chemistry have contributed to demolish the semblance of a barrier which existed in past times between the two branches of science, are furnished and suggested by the recently published List of Grants of Money which the Government has made to scientific men, on the recommendation of the Royal Society, from the fund which, for the first time last year, was added to the very modest sum previously accorded from national resources in support of research. The perusal of that list, representing as it does a most carefully considered selection by the highest representatives of science in the country, from a very large number of applications, affords important evidence, on the one hand, of the active pursuit of science in Great Britain, and, on the other, of the very wide range of subjects of interest and importance, the full investigation of which demands the provision of adequate resources. That the necessity for such resources needs but to be thoroughly made known to ensure their provision, even from other than national sources, has been demonstrated by the success which, in a comparatively brief space of time, has attended the efforts of the Chemical Society to establish, upon the foundation patriotically laid by one of its original members, Dr. Longstaff, a special fund, to be administered by the Society for the advancement of chemical science. An inspection of the list of contributors to this special fund in aid of chemical research which, in about two years, has reached the sum of four thousand pounds, and from the proceeds of which the first applications for grants have recently been met, is suggestive of two observations. One is, that the proportion and amount of contributions hitherto received are comparatively small from the source whence the greatest support of such a fund may naturally be looked for, namely, from those who most directly benefit by the results of chemical research. It is to be hoped that there are many prominent representatives of the chemical and metallurgic industries in this country who still intend to give practical effect to their natural desire to aid in the advancement of chemical science, and to the appreciation which they can hardly fail to entertain of the usefulness of this fund. On the other hand, it is a matter well meriting special notice that a very prominent section of the contributors to the fund is composed of some of the most ancient corporate bodies of the city of London. Most welcome evidence is thereby afforded of the readiness with which the City Companies are prepared to respond to appeals for the substantial support of measures well calculated to promote progress in science. This evidence, and the combined action which they are even now contemplating for promoting the application of scientific research to the advancement of industry and commerce, by establishing an institution for technical education upon a scale worthy to serve as a monument of the true usefulness of wealthy confederations, must be cordially hailed as very substantial proofs that these representatives of our national wealth and commercial supremacy are entering upon a new sphere of activity which will more than restore their ancient prestige, by according them a new rank, more elevated than any which their civic importance could, in the past or future, confer upon them—a rank high among the chief promoters of our national enlightenment.

SECTION C.

GEOLOGY.

Opening Address by the President, W. Pengelly, F.R.S., F.G.S.

When, as long ago as 1841, the British Association made its only previous visit to Plymouth, some of us, now amongst its oldest members, thought ourselves too young to take any part in its proceedings. If the effects of that meeting are still traceable in this district, it will be admitted, of course, that the seed then sown was of excellent quality and that it fell on good soil. Be this as it may, the hope may be cherished that thirty-six years will not again be allowed to elapse between two consecutive visits to the capital of the two south-western counties.

One effect of this wide hiatus is the loss of almost all the human links whose presence on this occasion would have pleasantly connected the present with the past. A glance at the lists of Trustees and the General, Sectional, and Local officers in 1841 will show that the presence of scarcely one of them can be hoped for on this occasion; and there is but little probability that any of those who prepared Reports or Papers for the last Plymouth Meeting will have done so for that which is now assembled.

Nor are these the only changes. In 1841 Section C embraced, as at the beginning, the geographers as well as the geologists; but ten years later the geographers were detached, whether to find room for themselves, or to make room for the students of an older geography, it is not necessary to inquire.

Some years afterwards came an innovation which, until entering on the preparation of this address, I always regarded as a decided improvement. The first Presidential Address to this Section was delivered at Leeds in 1858 by the late Mr. Hopkins, so well known to geologists for his able application of his great mathematical powers to sundry important problems in their science; and from that time to the present, with the exception of the Meetings of 1860 and 1870 only, the President of this Section has delivered an address.

None of the local geological papers read in 1841 appear to have attracted so much attention as those on Lithodomous Perforations, Raised Beaches, Submerged Forests, and Caverns (see *Athenæum* for 7th to 28th of August, 1841); and, as an effort to connect the present with the past, I have decided on taking up one of these threads, and devoting the remarks I have now to offer to the History of Cavern-Exploration in Devonshire. I am not unmindful that there were giants in those days; and no one can deplore more than I do our loss of Buckland and De la Beche, amongst many others; nor can I forget the enormous strides opinion has made since 1841, when, in this Section, Dr. Buckland "contended that human remains had never been found under such circumstances as to prove their contemporaneous existence with the hyænas and bears of the caverns," and added that "in Kent's Hole the Celtic knives . . . . were found in holes *dug by art*, and which had disturbed the floor of the cave and the bones below it" (*Athenæum*, 14th Aug. 1841, p. 626). This scepticism, however, did the good service of inducing cavern explorers to conduct their researches with an accuracy which should place their results, whatever they might prove to be, amongst the undoubted additions to human knowledge.

The principal caverns in South Devon occur in the limestone districts of Plymouth, Yealmpton, Brixham, Torquay, Buckfastleigh, and Chudleigh; but as those in the last two localities have yielded nothing of importance to the anthropologist or the palæontologist, they will not be further noticed on this occasion. In dealing with the others it seems most simple to follow mainly the order of chronology; that is to say, to commence with the cavern which first caught scientific attention, and, having finished all that the time at my disposal will allow me to say about it, but not before, to proceed to the next, in the order thus defined; and so on through the series.

Oreston Caverns.—When Mr. Whidbey engaged to superintend the construction of the Plymouth Breakwater, Sir Joseph Banks, President of the Royal Society, requested him to examine narrowly any caverns he might meet with in the limestone-rock to be quarried at Oreston, near the mouth of the river Plym, not more than two miles from the room in which we are assembled, and have the bones or any other fossil remains that were met with carefully preserved (see *Phil. Trans.*, 1817, pp. 176—182). This request was cheerfully complied with, and Mr. Whidbey had the pleasure of discovering bone-caves in November, 1816, November, 1820, August and November, 1822, and of sending the remains found in them to the Royal Society.

It is, perhaps, worthy of remark that, though cavern-researches received a great impulse from the discoveries in Kirkdale, Yorkshire, and especially from Dr. Buckland's well-known and graphic descriptions of them, such researches had originated many years before. The request by Sir Joseph Banks was made at least as early as 1812 (see *Trans.* Devon. Assoc., v., pp. 252, 253), and a paper on the Oreston discoveries was read to the Royal Society in February 1817, whereas the Kirkdale Cavern was not discovered until 1821. British cave-hunting appears to have been a science of Devonshire birth.

The Oreston Caverns soon attracted a considerable number of able observers; they were visited in 1822 by Dr. Buckland and Mr. Warburton; and in a comparatively short time became the theme of a somewhat voluminous literature. Nothing of importance, however, seems to have been met with from 1822 until 1858, when another cavern, containing a large number of bones, was broken into. Unfortunately, there was no one at hand to superintend the exhumation of the specimens; the work was left entirely to the common workmen, and was badly done; many of the remains were dispersed beyond recovery; the matrix in which they were buried was never adequately examined;

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and we are utterly ignorant, and must for ever remain so, as to whether they did or did not contain indications of human existence. I visited the spot from time to time, and bought up everything to be met with; but other scientific work in another part of the county occupied me too closely to allow more than an occasional visit. The greater part of the specimens I secured were lodged in the British Museum, where they seem to have been forgotten, whilst a few remain in my private collection.

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The conclusion I arrived at, after studying so much of the roof of the cavern of 1858 as remained intact, was that Dr. Buckland's opinion was fully borne out by the facts; that, in short, the Oreston Caverns were *Fissure Caverns*, not *Tunnel Caverns*.

The cavern of 1858 was an almost vertical fissure, extending a length of about 90 feet from N.N.E. to S.S.W. It commenced at about 8 feet below the surface of the plateau, continued thence to the base of the cliff, but how much further was not known, and its ascertained height was about 52 feet. It was 2 feet wide at top, whence it gradually widened to 10 feet at bottom. The roof, judging from that part which had not been destroyed, was a mass of limestone-breccia, made up of large angular fragments, cemented with carbonate of lime, and requiring to be blasted as much as ordinary limestone. The cavern was completely filled with deposits of various kinds.

The uppermost 8 feet consisted of loose angular pieces of limestone, none of which exceeded 10 lb. in weight, mixed with a comparatively small amount of such sand as is common in dolomitised limestone districts, but without a trace of stalagmite or fossil of any kind. The 32 feet next below were occupied with similar materials, with the addition of a considerable quantity of tough, dark, unctuous clay. Between this mass and the outer wall of the cavern was a nearly vertical plate of stalagmite, usually about 2 feet thick, and containing, at by no means wide intervals, firmly cemented masses of breccia identical in composition with the adjacent bed just mentioned. The bones the cavern yielded were all found within these 32 feet; and were met with equally in the loose and the coherent breccia, as well as in the stalagmite. A somewhat considerable number of ellipsoidal balls of clay, from 1·5 to 2·5 inches in greatest diameter, occurred in the clay of this bone-bed, but not elsewhere. Still lower was a mass of dark, tough, unctuous clay, containing a very few, small, angular stones, but otherwise perfectly homogeneous, and known to be 12 feet deep, but how much more was undetermined.

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Kent's Hole.—About a mile due east from Torquay harbour and half a mile north from Torbay there is a small wooded limestone hill, the eastern side of which is, for the uppermost 30 feet, a vertical cliff, having at its base, and 54 feet apart, two apertures leading into one and the same vast cavity in the interior of the hill, and known as Kent's Hole or Cavern. These openings are about 200 feet above mean sea-level, and from them the hill slopes rapidly to the valley at its foot, at a level of from 60 to 70 feet below.

There seems to be neither record nor tradition of the discovery of the cavern. Richardson, in the 8th edition of "A Tour through the Island of Great Britain," published in 1778, speaks of it as "perhaps the greatest natural curiosity" in the county; its name occurs on a map dated 1769; it is mentioned in a lease 1659; visitors cut their names and dates on the stalagmite from 1571 down to the present century; judging from numerous objects found on the floor, it was visited by man through mediæval back to pre-Roman times; and, unless the facts exhumed by explorers have been misinterpreted, it was a human home during the era of the mammoth and his contemporaries.

In 1824 Mr. Northmore, of Cleve, near Exeter, was led to make a few diggings in the cavern, and was the first to find fossil bones there. He was soon followed by Mr. (now Sir) W. C. Trevelyan, who not only found bones, but had a plate of them engraved. In 1825, the Rev. J. MacEnery, an Irish Roman Catholic priest residing in the family of Mr. Cary, of Tor Abbey, Torquay, first visited the cavern, when he, too, found teeth and bones, of which he published a plate. Soon after, he made another visit, accompanied by Dr. Buckland, when he had the good fortune to discover a flint implement; the first instance, he tells us, of such a relic being noticed in any cavern (see *Trans.* Devon Assoc., iii., p. 441). Before the close of 1825, he commenced a series of more or less systematic diggings, and continued them until, and perhaps after, the summer of 1829 (*ibid.*, p. 295). Preparations appear to have been made to publish the results of his labours; a prospectus was issued, numerous plates were lithographed, it was generally believed that the MS. was almost ready, and the only thing needed was a list of subscribers sufficient to justify publication, when, alas! on February 18, 1841, before the printer had received any "copy," before even the world of science had accepted his anthropological discoveries, before the value of his labours was known to more than a very few, Mr. MacEnery died at Torquay.

After his decease his MS. could not be discovered, and its loss was duly deplored. Nevertheless, it was found after several years, and, having undergone varieties of fortune, became the property of Mr. Vivian, of Torquay, who, having published portions of it in 1859, presented it in 1867 to the Torquay Natural History Society, whose property it still remains. In 1869 I had the pleasure of printing the whole, in the *Transactions* of the Devonshire Association.

Whilst Mr. MacEnery was conducting his researches, a few independent diggings, on a less extensive scale, were taken by other gentlemen. The principal of these was Mr. Godwin-Austen, the well-known geologist, whose papers fully bore out all that MacEnery had stated. (See *Trans.* Geol. Soc. Lond., 2nd series, vi., p. 446). In 1846 a sub-committee of the Torquay Natural History Society undertook the careful exploration of very small parts of the cavern, and their Report was entirely confirmatory of the statements of their predecessors—that undoubted flint implements did occur, mixed with the remains of extinct mammals, in the cave-earth, beneath a thick floor of stalagmite. The sceptical position of the authorities in geological science remained unaffected, however, until 1858, when the discovery and systematic exploration of a comparatively small virgin cavern on Windmill Hill, at Brixham, led to a sudden and complete revolution; for it was seen that whatever were the facts elsewhere, there had undoubtedly been found at Brixham flint implements commingled with remains of the mammoth and his companions, and in such a way as to render it impossible to doubt that man occupied Devonshire before the extinction of the cave mammals.

Under the feeling that the statements made by MacEnery and his followers respecting Kent's Hole were perhaps, after all, to be accepted as verities, the British Association, in 1864, appointed a committee to make a complete, systematic, and accurate exploration of the cavern, in which it was known that very extensive portions remained entirely intact. This committee commenced its labours on March 28, 1865; it has been reappointed, year after year, with sufficient grants of money, up to the present time; the work has gone on continuously throughout the entire thirteen years; and the result has been, not only a complete confirmation of Mr. MacEnery's statements, but the discovery of far older deposits than he suspected—deposits implying great changes of, at least, local geographical conditions; changes in the fauna of the district; and yielding evidence of men more ancient and far ruder than even those who made the oldest flint tools found in Kent's Hole prior to the appointment of the committee.

The cavern consists of a series of chambers and passages, which resolve themselves into two main *divisions*, extending from nearly north to south in parallel lines, but passing into each other near their extremities, and throwing off branches, occasionally of considerable size.

The successive deposits, in descending order, were:—

1st, or uppermost. Fragments and blocks of limestone from an ounce to upwards of 100 tons weight each, which had fallen from the roof from time to time, and were, in some instances, cemented with carbonate of lime.

2nd. Beneath and between these blocks lay a dark-coloured mud or mould, consisting largely of decayed leaves and other vegetable matter. It was from 3 to 12 inches thick, and known as the *black mould*. This occupied the entire eastern division, with the exception of a small chamber in its south-western end only, but was not found in the other, the remoter, parts of the cavern.

3rd. Under this was a stalagmitic floor, commonly of granular texture, and frequently laminated, from less than an inch to fully five feet in thickness, and termed the *granular stalagmite*.

4th. An almost black layer, about four inches thick, composed mainly of small fragments of charred wood, and distinguished as the *black band*, occupied an area of about 100 square feet, immediately under the granular stalagmite, and, at the nearest point, not more than thirty-two feet from one of the entrances to the cavern. Nothing of the kind has occurred elsewhere.

5th. Immediately under the granular stalagmite and the black band lay a light red clay, containing usually about 50 per cent. of small angular fragments of limestone, and somewhat numerous blocks of the same rock as large as those lying on the black mould. In this deposit, known as the *cave-earth*, many of the stones and bones were, at all depths, invested with thin stalagmitic films. The cave-earth was of unknown depth near the entrances, where its base had never been reached; but in the remoter parts of the cavern it did not usually exceed a foot, and in a few localities it "thinned out" entirely.

6th. Beneath the cave-earth there was usually found a floor of stalagmite having a crystalline texture, and termed on that account the *crystalline stalagmite*. It was commonly thicker than the granular floor, and in one instance but little short of 12 feet.

7th. Below the whole occurred, so far as is at present known, the oldest of the cavern deposits. It was composed of sub-angular and rounded pieces of dark-red grit, embedded in a sandy paste of the same colour. Small angular fragments of limestone, and investing films of stalagmite, both prevalent in the cave-earth, were extremely rare. Large blocks of limestone were occasionally met with; and the deposit, to which the name of *breccia* were given, was of a depth exceeding that to which the exploration has yet been carried.

Except in a very few small branches, the bottom of the cavern has nowhere been reached. In the cases in which there was no cave-earth, the granular stalagmite rested immediately on the crystalline; and where the crystalline stalagmite was not present the cave-earth and breccia were in direct contact. Large isolated masses of the crystalline stalagmite, as well as concreted lumps of the breccia, were occasionally met with in the cave-earth, thus showing that the older deposits had, in portions of the cavern, been partially broken up, dislodged, and re-deposited. No instance was met with of the incorporation in a lower bed of fragments derived from an upper one. In short, wherever all the deposits were found in one and the same vertical section, the order of superposition was clear and invariable; and elsewhere the succession, though defective, was never transgressed.

Excepting the overlying blocks of limestone, of course, all the deposits contained remains of animals, which, however, were not abundant in the stalagmites.

The black mould, the uppermost bed, yielded teeth and bones of man, dog, fox, badger, brown bear, *Bos longifrons*, roe-deer, sheep, goat, pig, hare, rabbit, and seal—species still existing, and almost all of them in Devonshire. This has been called the *Ovine* bed, the remains of sheep being restricted to it. In it were also found numerous flint flakes and "strike-lights," stone spindle-whorls, fragments of curvilineal pieces of slate, amber beads, bone tools, including awls, chisels, and combs; bronze articles, such as rings, a fibula, a spoon, a spear-head, a socketed celt, and a pin; pieces of smelted copper, and a great number and variety of potsherds, including fragments of Samian ware.

The granular stalagmite, black band, and cave-earth, taken together as belonging to one and the same biological period, may be termed the *Hyænine* beds, the cave hyæna being their most prevalent species, and found in them alone. So far as they have been identified, the remains belong to the cave hyæna, *Equus caballus*, *Rhinoceros tichorhinus*, gigantic Irish deer, *Bos primigenius*, *Bison priscus*, red deer, mammoth, badger, cave bear, grizzly bear, brown bear, cave lion, wolf, fox, reindeer, beaver, glutton, *Machairodus latidens*, and man—the last being a part of a jaw with teeth, in the granular stalagmite. In the same beds were found unpolished *ovate* and *lanceolate* implements made from *flakes*, not *nodules*, of flint and chert; flint flakes, chips, and "cores;" "whetstones," a "hammer-stone," "dead" shells of *Pecten*, bits of charcoal, and bone tools, including a needle or bodkin having a well-formed eye, a pin, an awl, three harpoons, and a perforated tooth of badger. The artificial objects, of both bone and stone, were found at all depths in each of the hyænine beds, but were much more numerous below the stalagmite than in it.

The relics found in the crystalline stalagmite and the breccia, in some places extremely abundant, were almost exclusively those of bear, the only exceptions being a very few remains of cave lion and fox. Hence these have been termed the *Ursine* beds. It will be remembered that teeth and bones of bear were also met with in both the hyænine and the ovine beds; and it should be understood that this biological classification is intended to apply to Kent's Cavern only. The ursine deposits, or rather the breccia, the lowest of them, also yielded evidences of human existence; but they were exclusively tools made from *nodules*, not *flakes*, of flint and chert.

Ansty's-Cove Cavern.—About three furlongs from Kent's Hole towards N.N.E., near the top of the lofty cliff forming the northern boundary of the beautiful Ansty's Cove, Torquay, there is a cavern where, simultaneously with those in Kent's Cavern, Mr. MacEnery conducted some researches, of which he has left a brief account (see *Trans.* Devon. Assoc., vi., pp. 61–69). I have visited it several times, but it seems to be frequently kept under lock and key, as a tool and powder-house, by the workmen in a neighbouring quarry. It is a simple gallery, and, according to Mr. MacEnery, 63 feet long, from 3 to 9 feet high, and from 3 to 6 feet broad. Beneath some angular stones he found a stalagmitic floor 14 inches thick, and in the deposit below remains of deer, horse, bear, fox, hyæna (?), coprolites, a few marine and land shells, one white flint tool with fragments of others, a Roman coin, and potsherds.

In a letter to Sir W. C. Trevelyan, dated 16th December, 1825, Dr. Buckland states that Mr. MacEnery had found in this cave "bones of all sorts of beasts, and also flint knives and Roman coins; in short, an open-mouthed cave, which has been inhabited by animals of all kinds, quadruped and biped, in all successive generations, and who have all deposited their exuviæ one upon another" (*ibid.*, p. 69).

Yealm-Bridge Cavern.—About the year 1832 the workmen broke into a bone-cavern in Yealm-Bridge Quarry, about one mile from the village of Yealmpton, and eight miles E.S.E. from Plymouth; and through their operations it was so nearly destroyed that but a small arm of it remained in 1835, when it was visited by Mr. J. C. Bellamy, who at once wrote an account of it, from which it appears that, so far as he could learn, the cavern was about 30 feet below the original limestone surface, and was filled to from 1 foot to 6 feet of the roof (see "Nat. Hist. S. Devon," 1839, pp. 86–105). In the same year, but subsequently, it was examined by Capt. (afterwards Col.) Mudge, who states that there were orginally three openings into the cave, each about 12 feet above the river Yealm; that the deposits were, in descending order:—

1	Loam with bones and stones	3·5 feet
2.	Stiff whitish clay	2·5 ,,
3.	Sand	6·0 ,,
4.	Red clay	3·5 ,,
5.	Argillaceous sand	6 to 18·0 ,,

and that, where they did not reach the roof, the deposits were covered with stalagmite.

On the authority of Mr. Clift and Prof. Owen, Capt. Mudge mentions relics of elephant, rhinoceros, horse, ox, sheep, hyæna, dog, wolf, fox, bear, hare, and water-vole. The bones, and especially the teeth, of the hyæna exceeded in number those of all the other animals, though remains of horse and ox were very abundant. Mr. Bellamy, whilst also mentioning all the foregoing forms, with the exception of dog only, adds deer, pig, glutton, weasel, and mouse. He also speaks of the abundance of bones and teeth of hyæna, but seems to regard the fox as being almost as fully represented; and next in order he places horse, deer, sheep, and rabbit or hare; whilst the relics of elephant, wolf, bear, pig, and glutton are spoken of as very rare. The bones, he says, were found in the uppermost bed only. They were frequently mere fragments and splinters, some being undoubtedly gnawed, and all had become very adherent through loss of their animal matter. Those of cylindrical form were without their extremities; there was no approach to anatomical juxtaposition; and the remains belonged to individuals of all ages. Reliquiæ of carnivorous animals greatly exceeded those of the herbivora, and teeth were very abundant. Coprolites occurred at some depth below the stalagmite, in the upper bed, which also contained granitic and trappean pebbles, and lumps of breccia made up of fragments of rock, bones, pebbles, and stalagmite. The bones found prior to 1835 had been removed as rubbish, and some good specimens were recovered from materials employed in making a pathway. Nothing indicating the presence of man appears to have been found.

The Ash-Hole.—On the southern shore of Torbay, midway between the town of Brixham and Berry Head, and about half a mile from each, there is a cavern known as the *Ash-Hole.* It was partially explored, probably about, or soon after, the time Mr. MacEnery was engaged in Kent's Hole, by the late Rev. H. F. Lyte, who, unfortunately, does not appear to have left any account of the results. The earliest mention of this cavern I have been able to find is a very brief one in Bellamy's "Natural History of South Devon," published in 1839 (p. 14). During the Plymouth Meeting in 1841, Mr. George Bartlett, a native of Brixham, who assisted Mr. Lyte, described to this Section the objects of interest the Ash-Hole had yielded (see *Report Brit. Assoc.* 1851, *Trans. Sections*, p. 61). So far as was then known the cave was thirty yards long and six yards broad. Below a recent accumulation, four feet deep, of loam and earth, with land and marine shells, bones of the domestic fowl and of man, pottery, and various implements, lay a true cave-earth, abounding in the remains of elephant. Prof. Owen, who identified, from this lower bed, relics of badger, polecat, stoat, water-vole, rabbit, and reindeer, remarks, that for the first good evidence of the reindeer in this island he had been indebted to Mr. Bartlett, who stated that the remains were found in this cavern (see "Brit. Foss. Mam." 1846, pp. 109-110, 113-114, 116, 204, 212, 479-480). I have made numerous visits to the spot, which, when Mr. Lyte began his diggings, must have been a shaft-like fissure, accessible from the top only. A lateral opening, however, has been quarried into it; there is a narrow tunnel extending westward, in which the deposit is covered with a thick sheet of stalagmite, and where one is tempted to believe that a few weeks' labour might be well invested.

Brixham Cavern.—Early in 1858 an unsuspected cavern was broken into by quarrymen at the north-western angle of Windmill Hill at Brixham, at a point seventy-five feet above the surface of the street, almost vertically below, and 100 feet above mean tide. On being found to contain bones, a lease in it was secured for the Geological Society of London, who appointed a committee of their members to undertake its exploration; funds were voted by the Royal Society, and supplemented by private subscriptions; the conduct of the investigation was intrusted to Mr. Prestwich and myself; and the work, under my superintendence, as the only resident member of the committee, was begun in July, 1858, and completed at midsummer, 1859.

The cavern, comprised within a space of 135 feet from north to south, and 100 from east to west, consisted of a series of tunnel galleries from six to eight feet in greatest width, and ten to fourteen feet in height, with two small chambers and five external entrances.

The deposits, in descending order, were:—

1st, or uppermost. A floor of stalagmite, from a few inches to a foot thick, and continuous over very considerable areas, but not throughout the entire cavern.

2nd. A mass of small angular fragments of limestone, cemented into a firm concrete with carbonate of lime, commenced at the principal entrance, which it completely filled, and whence it extended thirty-four feet only. It was termed the *first bed.*

3rd. A layer of blackish matter, about twelve long, and nowhere more than a foot thick, occurred immediately beneath the first bed, and was designated the *second bed.*

4th. A red, tenacious, clayey loam, containing a large number of angular and subangular fragments of limestone, varying from very small bits to blocks a ton in weight, made up the *third bed.* Pebbles of trap, quartz, and limestone were somewhat prevalent, whilst nodules of brown hematite of iron and blocks of stalagmite were occasionally met with in it. The usual depth of the bed was from two to four feet, but this was exceeded by four or five feet in two localities.

5th. The third bed lay immediately on an accumulation of pebbles of quartz, greenstone, grit, and limestone, mixed with small fragments of shale. The depth of this, known as the *fourth* or *gravel bed*, was undetermined; for, excepting a few feet only, the limestone bottom was nowhere reached. There is abundant evidence that this bed, as well as a stalagmitic floor which had covered it, had been partially broken up and dislodged before the introduction of the third bed.

Organic remains were found in the stalagmitic floor and in each of the beds beneath it, with the exception of the second only; but as ninety-five per cent of the whole series occurred in the third, this was not unfrequently termed the *bone bed.*

The mammals represented in the stalagmite were bear, reindeer, *Rhinoceros tichorhinus*, mammoth, and cave lion.

The first bed yielded bear and fox only.

In the third bed were found relics of mammoth, *Rhinoceros tichorhinus*, horse, *Bos primigenius*, *B. longifrons*, red deer, reindeer, roebuck, cave lion, cave hyæna, cave bear, grizzly bear, brown bear, fox, hare, rabbit, *Lagomys spelæus*, water-vole, shrew, polecat, and weasel.

The only remains met with in the fourth bed were those of bear, horse, ox, and mammoth.

The human industrial remains exhumed in the cavern were flint implements and a hammer-stone, and occurred in the third and fourth beds only. The pieces of flint met with were thirty-six in number. Of these fifteen are held to show evidence of having been artificially worked, in nine the workmanship is rude or doubtful, four have been mislaid, and the remainder are believed not to have been worked at all (see *Phil. Trans.*, vol. 163, 1873, pp. 561, 562). Of the undoubted tools, eleven were found in the third and four in the fourth bed. Two of those yielded by the third bed, found forty feet apart, in two distinct but adjacent galleries, and one a month before the other, proved to be parts of one and same *nodule*-tool; and I have little or no doubt that it had been washed out of the fourth bed and redeposited in the third.

The hammer-stone was a quartzite pebble, found in the upper portion of the fourth bed, and bore distinct marks of the use to which it was applied.

Speaking of the discovery of the tools just mentioned, Mr. Prestwich said in 1859:—"It was not until I had myself witnessed the conditions under which flint implements had been found at Brixham, that I became fully impressed with the validity of the doubts thrown upon the previously prevailing opinions with respect to such remains in caves" (*Phil. Trans.*, 1860, p. 280); and according to Sir C. Lyell, writing in 1863:—"A sudden change of opinion was brought about in England respecting the probable co-existence, at a former period, of man and many extinct mammalia, in consequence of the results obtained from the careful exploration of a cave at Brixham. . . . The new views very generally adopted by English geologists had no small influence on the subsequent progress of opinion in France" ("Antiquity of Man," pp. 96, 97).

Bench Cavern.—Early in 1861 information was brought me that an ossiferous cave had just been discovered at Brixham, and, on visiting the spot, I found that, of the limestone quarries worked from time to time in the northern slope of Furzeham Hill, one known as Bench Quarry, about half a mile due north of Windmill Hill Cavern, and almost overhanging Torbay, had been abandoned in 1839, and that work had been recently resumed in it. It appeared that in 1839 the workmen had laid bare the greater part of a vertical dyke, composed of red clayey loam, and angular pieces of limestone, forming a coherent wall-like mass, 27 feet high, 12 feet long, 2 feet in greatest thickness, and at its base 123 feet above sea-level. In the face of it lay several fine relics of the ordinary cave mammals, including an entire left lower jaw of *Hyæna spelæa* replete with teeth, but which had nevertheless failed to arrest the attention of the incurious workmen who exposed it, or of any one else.

Soon after the resumption of the work in 1861, the remnant of the outer wall of the fissure was removed, and caused the fall of an incoherent part of the dyke, which it had previously supported. Amongst the *débris* the workmen collected some hundreds of specimens of skulls, jaws, teeth, vertebra, portions of antlers, and bones, but no indications of man. Mr. Wolston, the proprietor, sent some of the choicest specimens to the British Museum, and submitted the remainder to Mr. Ayshford Sanford, F.G.S., from whom I learn that the principal portion of them are relics of the cave hyæna, from the unborn whelp to very aged animals. With them, however, were remains of bear, reindeer, ox, hare, *Arvicolo ratticeps*, *A. agrestis*, wolf, fox, and part of a single maxillary with teeth not distinguishable from those of *Canis isatis*. To this list I may add rhinoceros, of which Mr. Wolston showed me at least one bone.

From the foregoing undesirably, but unavoidably, brief descriptions, it will be seen that the Devonshire caverns, to which attention has been now directed, belong to two classes,—those of Oreston, the Ash-Hole, and Bench being *Fissure Caves;* whilst those of Yealm Bridge, Windmill Hill at Brixham, Kent's Hole, and Ansty's Cove are *Tunnel Caves.*

Windmill Hill and Kent's Hole Caverns have alone been satisfactorily explored; and besides them none have yielded evidence of the contemporaneity of man with the extinct cave mammals.

Oreston is distinguished as the only known British cavern which has yielded remains of *Rhinoceros leptorhinus* (*Quart. Journ.* Geol. Soc., xxxvi. p., 456).

Yealm Bridge Cavern, if we may accept Mr. Bellamy's identification in 1835, was the first in this country in which relics of glutton were found (*South Devon Monthly Museum*, vi., pp. 218–223; see also "Nat. Hist. S. Devon," 1839, p. 89). The same species was found in the caves of Somerset and Glamorgan in 1865 (*Pleist. Mam.*, *Pal. Soc.*, pp. xxi. xxii.), in Kent's Hole in 1869 (*Rep. Brit. Assoc.*, 1869, p. 207), and near Plas Heaton, in North Wales, in 1870 (*Quart. Journ.* Geol. Soc., xxvii., p. 407).

Kent's Hole is the only known British cave which has afforded remains of beaver, (*Rep. Brit. Assoc.*, 1869, p. 208), and up to the present year the only one in which the remains of *Machairodus latidens* had been met with. Indeed Mr. MacEnery's statement, that he found in 1826 five canines and one incisor of this species in the famous Torquay Cavern was held by many palæontologists to be so very remarkable as, at least, to approach the incredible, until the Committee now engaged in the exploration exhumed, in 1872, an incisor of the same species, and thereby confirmed the announcement made by their distinguished predecessor nearly half a century before (*Rep. Brit. Assoc.*, 1872, p. 46). In April last (1877) the Rev. J. M. Mello was able to inform the Geological Society of London that Derbyshire had shared with Devon the honour of having been a home of *Machairodus latidens*, he having found its canine tooth in Robin Hood Cave in that county, and that there, as in Kent's Hole it was commingled with remains of the cave hyæna and his contemporaries (*Abs. Proc.* Geol. Soc., No. 334, pp. 3, 4).

The Ash Hole, as we have already seen, afforded the first good evidence of a British reindeer.

In looking at the published reports on the two famous Torbay caverns it will be found that they have certain points of resemblance as well as some of dissimilarity:—

1st. The lowest known bed in each is composed of materials which, whilst they differ in the two cases, agree in being such as may have been furnished by the districts adjacent to the cavern-hills respectively, but not by the hills themselves, and must have been deposited prior to the existing local geographical conditions. In each, this bed contained flint implements and relics of bear, but in neither of them those of hyæna. In short, the *fourth bed* of Windmill Hill Cavern, Brixham, and the *breccia* of Kent's Hole, Torquay, are coeval, and belong to what I have called the *Ursine* period of the latter.

2nd. The beds just mentioned were in each cavern sealed with a sheet of stalagmite, which was partially broken up, and considerable portions of the subjacent beds were dislodged before the introduction of the beds next deposited.

3rd. The great bone bed, both at Brixham and Torquay, consisted of red clayey loam, with a large percentage of angular fragments of limestone; and contained *flake* implements of flint and chert, inosculating with remains of mammoth, the tichorhine rhinoceros, and hyæna. In fine, the *cave-earth* of Kent's Hole and the *third bed* of Brixham Cavern correspond in their materials, in their osseous contents, and in their flint tools. They both belong to what I have named the *Hyænine* period of the Torquay Cave.

But, as already stated, there are points in which the two caverns differ:—

1st. Whilst Kent's Hole was the home of man, as well as of the contemporary hyæna during the absences of the human occupant, there is no reason to suppose that either man or any of the lower animals ever did more than make occasional visits to Brixham Cave. The latter contained no flint chips, no bone tools, no utilised *Pecten*-shells, no bits of charcoal, and no coprolites of hyæna, all of which occurred in the cave-earth of Kent's Hole.

2nd. In the Torquay Cave relics of hyæna were much more abundant in the cave-earth than those of any other species. Taking the teeth alone, of which vast numbers were found, those of the hyæna amounted to about 30 per cent. of the entire series, notwithstanding the fact that, compared with most of the cave-mammals, his jaws, when furnished completely, possess but few teeth. At Brixham, on the other hand, his relics of all kinds amounted to no more than 8·5 per cent. of all the osseous remains, whilst those of the bear rose to 53 per cent.

3rd. The entrances of Brixham Cavern were completely filled up and its history suspended not later than the end of the Palæolithic era. Nothing occurred within it from the days when Devonshire was occupied by the cave and grizzly bears, reindeer, rhinoceros, cave lion, mammoth, and man, whose best tools were unpolished flints, until the quarrymen broke into it early in A.D. 1858. Kent's Cavern, on the contrary seems to have never been closed, never unvisited by man, from the earliest Palæolithic times to our own, with the possible exception of the Neolithic era, of which it cannot be said to have yielded any certain evidence.

Though my "History of Cavern Exploration in Devonshire" is now completed, so far as the time at my disposal will allow, and so far as the materials are at present ripe for the historian, I venture to ask your further indulgence for a few brief moments whilst passing from the region of fact to that of inference.

That the Kent's Hole men of the Hyænine period—to say nothing at present of their predecessors of the Breccia—belonged to the Pleistocene times of the biologist, is seen in the fact that they were contemporary with mammals peculiar to and characteristic of those times. This contemporaneity proves them to have belonged to the *Palæolithic* era of Britain and Western Europe generally, as defined by the archæologist; and this is fully confirmed by their unpolished tools of flint and chert. That they were prior to the deposition of even the oldest part of the peat bogs of Denmark, with their successive layers of beech, pedunculated oak, sessile oak, and Scotch fir, we learn from the facts that even the lowest zone of the bogs has yielded no bones of mammals but those of recent species, and no tools but those of *Neolithic* type; whilst even the granular stalagmite, the uppermost of the Hyænine beds in Kent's Hole, has afforded relics of mammoth, *Rhinoceros tichorhinus*, cave bear, and cave hyæna.

That the men of the Cave Breccia, or Ursine period, to whom we now turn, were of still higher antiquity, is obvious from the geological position of their industrial remains. That the two races of Troglodytes were separated by a wide interval of time we learn from the sheet of crystalline stalagmite, sometimes 12 feet thick, laid down after the deposition of the breccia had ceased, and before the introduction of the cave-earth had begun, as well as from the entire change in the materials composing the two deposits. But, perhaps, the fact which most emphatically indicates the chronological value of this interval is the difference in the faunas. In the cave-earth, as already stated, the remains of the hyæna greatly exceed in number those of any other mammal; and it may be added that he is also disclosed by almost every relic of his contemporaries—their jaws have, through his agency, lost their condyles and lower borders; their bones are fractured after a fashion known by experiment to be his; and the splinters into which they are broken are deeply scored with his teeth-marks. His presence is also attested by the abundance of his droppings in every branch of the cavern. In short, Kent's Hole was one of his *homes;* he dragged thither, piecemeal, such animals as he found dead near it; and the well-known habits of his representatives of our day have led us to expect all this from him. When, however, we turn to the breccia, a very different spectacle awaits us. We meet with no trace whatever of his presence, not a single relic of his skeleton, not a bone on which he has operated, not a coprolite to mark as much as a visit. Can it be doubted that had he then occupied our country he would have taken up his abode in our cavern? Need we hesitate to regard this entire absence of all traces of so decided a cave-dweller as a proof that he had not yet made his advent in Britain? Are we not compelled to believe that man formed part of the Devonshire fauna long before the hyæna did? Is there any method of escaping the conclusion that between the era of the Breccia and that of the Cave-earth it was possible for the hyæna to reach Britain?—in other words, that the last continental state of our country occurred during that interval? I confess that, in the present state of the evidence, I see no escape; and that the conclusion thus forced on me compels me to believe also that the earliest men of Kent's Hole were *interglacial*, if not *preglacial*.

The following table will serve to show at one view the co-ordinations and theoretical conclusions to which the facts of Kent's Cavern have led me, as stated briefly in the foregoing remarks. The table, it will be seen, consists of two divisions, separated with double vertical lines. The first, or left hand, division contains three columns, and relates exclusively to Kent's Cavern, as is indicated by the words heading it. The second, or right hand, division is of a more general character, and shows the recognised classification of well-known facts throughout Western Europe. The horizontal lines are intended to convey the idea of more or less well-defined chronological horizons, and their occasional continuity through two or more columns denotes contemporaneity. Thus, to take an example from the two columns headed "Archæological" and "Danish-Bog," in the second division: the horizontal line passing continuously through both, under the words "Iron" and "Beech," is intended to suggest that the "Iron Age" of Western Europe and the "Beech" zone of the Danish Bogs take us back about equally far into antiquity; whilst the position of the line under the word "Bronze" indicates that the "Bronze age" (still of Western Europe) takes us back from the ancient margin of the Beech era, through the whole of that of the Pedunculated Oak, and about half-way through the era of the Sessile Oak; and so on in all other cases.

KENT'S CAVERN.			PERIODS.				
Deposits.	Bones.	Implements.	Archæological.	Danish-Bog.	Biological.	Geographical.	Climatal.
Black Mould.	Ovine.	Iron,	Iron.	Beech.	Recent.	Insular.	Post-Glacial.
		Bronze,	Bronze.	Pedunculated Oak.			
		and (?)		Sessile Oak.			
		Neolithic.	Neolithic.	Scotch Fir.			
Granular Stalagmite.	Hyænine.	Palæolithic Flakes.	Palæolithic.		Pleistocene.	Continental.	Glacial and (?) Inter-Glacial.
Black Band.							
Cave-earth.							
Crystalline Stalagmite.	Ursine.	Palæolithic Nodules.				Insular.	
Breccia.						Continental.	Pre-Glacial.

SECTION D.

BIOLOGY.

OPENING ADDRESS BY THE PRESIDENT, J. GWYN JEFFREYS, LL.D., F.R.S., TREAS. G. AND L.SS.

BEING merely an amateur naturalist, and not having had any strictly scientific education, I consider it a great honour to be invited to preside over this important Section of the Association. I cannot pretend to give such an address as may be expected from the president; but I will offer some remarks on a subject in which I take considerable interest and have done some work, viz., the deep-sea mollusca.

The historical part of the subject has been fully treated by Dr. G. C. Wallich in his "North-Atlantic Sea-bed," 1862; by Prof. Prestwich in his Presidential Address to the Geological Society of London in 1871; and by Prof. Sir Wyville Thomson in his "Depths of the Sea," 1873.

By the term "deep-sea" I do not mean the zone which the late Prof. Edward Forbes called the eighth, and which he supposed to be the lowest and the limit of habitability, in his elaborate and excellent "Report on the Ægean Invertebrata," published by the Association in 1844. That zone comprised the depths lying between 105 and 230 fathoms. Nor would I refer to it the "deep-sea" zone which I defined in the Introduction to my work on "British Conchology," 1862; this applied to the British seas only, and extended to the "line of soundings," being about 100 fathoms. Since that time the exploring expeditions in H.M.SS. *Lightning*, *Porcupine*, *Challenger*, and *Valorous*, as well as in the Norwegian frigate *Vöringen*, have shown that mollusca inhabit the greatest depths that have been examined, and that life is not less abundant and varied in the abysses of the ocean than it is in the shallowest water. Instead of 300 fathoms or 1,800 feet, which Forbes assumes to be the extreme boundary of submarine life, we must now take 3,000 fathoms, or 18,000 feet, and even much lower depths. It may be well to distinguish two zones of depth exceeding that which I have termed "the line of soundings"; and I would propose the name "abyssal" for depths between 100 and 1,000 fathoms, and "benthal" (from the Homeric word *βένθος*, signifying the depths of the sea) for depths of one thousand fathoms and more.

The first knowledge that I had of the mollusca from the lowest or "benthal" zone I owe to Dr. Wallich, who kindly gave me a few small shells which he got in a sounding of 1,622 fathoms in N. Lat. 55° 36′, W. Long. 54° 33′, off the coast of Labrador, during his cruise in H.M.S. *Bulldog* in 1860. These consisted of undescribed species of *Aclis*, *Homalogyra*, and *Pleurotoma*, *Pleurotoma tenuicostata* of M. Sars, and fragments of *Saxicava rugosa*, Linné, and of other shells which are unknown to me. Among these was a dead but perfect specimen of *Crenella faba*, Fabricius, which is a common inhabitant of the laminarian zone in Arctic seas, and may have been voided by a fish or sea-bird. This would account for the occasional occurrence at great depths of other shallow-water shells and fragments.

I had the good fortune to take part in the two *Porcupine* expeditions of 1869 and 1870, and in the *Valorous* cruise of 1875; and the mollusca of the *Lightning* (1868), *Challenger* (1873-76), and *Vöringen* (1876) expeditions have been submitted to my inspection. I am consequently enabled to form some idea of the bathymetrical distribution of the mollusca thus obtained, with the aid of my dredging experience for upwards of forty years.

Perhaps the best way of communicating this idea to others will be by giving the subjoined list of the species of deep-sea mollusca dredged by me in the *Valorous*, all of which are found at depths exceeding 1,000 fathoms. The range of depth there and elsewhere in the North Atlantic and Mediterranean will be noted, as well as some geological and other observations. Four only of such deep dredgings were made during the cruise, viz., in 1,100, 1,750, 1,450, and 1,785 fathoms. The first two were in Davis Strait, and the other two between Cape Farewell and W. Long. 26° on the return voyage.

Names of species.	Range of depth in fathoms	Observations.
BRACHIOPODA.		
Terebratula tenera, *Jeffreys*	1450	
Atretia gnomon, *J.*	1100-1750	
Discina Atlantica, *King*	690-2400	Coralline-Crag fossil.
CONCHIFERA.		
Pecten fragilis, *J.*	1000-1785	
Amussium lucidum, *J.*	156-1450	
Lima ovata, *S. V. Wood*	1450	Coralline-Crag and Monte-Mario fossil.
L. subovata, *J.*	49-1450	
L. gibba, *J.*	1450-1785	
Idas argenteus, *J.*	994-1450	
Dacrydium vitreum, *Möller*	30-2435	Sicilian fossil.
Nucula reticulata, *J.*	420-1470	
Leda acuminata, *J.*	20-1750	Sicilian fossil, as *L. Messanensis*, Seguenza.
L. pusio, *Philippi*, var.	257-1750	Sicilian fossil.
L. pustulosa, *J.*	202-1470	Sicilian fossil.
L. expansa, *J.*	690-1750	
L. lata, *J.*	165-1785	
L. sericea, *J.*	740-1450	
Glomus nitens, *J.*	557-1750	
Limopsis tenella, *J.*	1450	
L. cristata, *J.*	292-1095	
Arca pectunculoïdes, *Scacchi.*	20-1100	Coralline-Crag and Sicilian fossil.
Malletia excisa, *Ph.*	1443-1750	Sicilian fossil.
M. cuneata, *J.*	718-1800	
Montacuta Dawsoni, *J.*	3-1750	Fragments only at greatest depth.
Kellia symmetros, *J.*	488-1750	
Axinus cycladius, *S. V. Wood*	30-1750	Coralline-Crag fossil.
A. eumyarius, *M. Sars*	114-1456	
A. Croulinensis, *J.*	20-1785	
A. incrassatus, *J.*	40-1750	
Diplodonta Torelli, *J.*	30-1450	Fragment only at greatest depth.
Isocardia cor, *L.*	40-1785	Fry only at greatest depth.
Tellina calcaria, *Chemnitz*	1-1750	Sicilian fossil. Fragments only at greatest depth.
Poromya rotundata, *J.*	1450	
Pecchiolia abyssicola, *M. Sars*	110-1450	Fragments only at greatest depth.
P. gibbosa, *J.*	1450	Fragment only.
P. tornata, *J.*	1785	Fragment only.
Neæra striata, *J.*	435-1450	
N. exigua, *J.*	1450	
N. notabilis, *J.*	1450	
N. circinnata, *J.*	994-1450	
N. papyria, *J.*	1450	
N. angularis, *J.*	290-1785	Fragment only at greatest depth.
SOLENOCONCHIA.		
Dentalium candidum, *J.*	410-2435	
D. capillosum, *J.*	220-1785	
D. ensiculus, *J.*	740-1785	
D. subterfissum, *J.*	1000-1476	Fragment only from *Valorous.*
D. vagina, *J.*	1450-1785	
Siphodentalium vitreum, *M. Sars*	150-1750	
S. affine, *M. Sars*	100-1450	
S. Lofotense, *M. Sars*	20-1750	
Cadulus tumidosus, *J.*	110-1450	
C. Olivi, *Sc.*	539-1450	Sicilian fossil.
C. cylindratus, *J.*	1215-1476	
GASTROPODA.		
Propilidium ancyloïdes, *Forbes*	60-1450	Sicilian fossil, as *Rostrisepta parva*, Seg.
Puncturella profundi, *J.*	740-1750	
Scissurella crispata, *Fleming.*	7-1095	Sicilian fossil.
S. tenuis, *J.*	1450	
Cyclostrema basistriatum, *J.*	50-1095	Sicilian fossil.
Acirsa prælonga, *J.*	994-1450	
Eulima stenostoma, *J.*	50-1456	
Natica affinis, *Gmelin*	5-1100	Fragments only at greatest depth.
N. sphæroides, *J.*	1750	A young shell.
Seguenzia formosa, *J.*	325-1785	Sicilian fossil, as *S. monocingulata*, Seg.
S. carinata, *J.*	690-1095	
Cerithium procerum	400-1450	*C. Danielseni*, Friele.
Trophon Fabricii, *Beck*	35-1450	Fragment only at greatest depth.
Fusus attenuatus, *J.*	690-1215	
F. Sabini, *Gray*	100-1450	Fragments only at greatest depth.
Pleurotoma tenuicostata, *M. Sars*	40-1622	
P. exarata, *Möll*	5-1230	
Cylichna alba, *Brown*	7-1400	Sicilian fossil.
Utriculus lacteus, *J.*	1443-1450	Fragment only at greatest depth.
U. substriatus, *J.*	1750	
Actæon exilis, *J.*	49-1450	Sicilian fossil.
Scaphander puncto-striatus, *Mighels and Adams*	26-1450	Sicilian fossil. Fragment only at greatest depth.

Besides undeterminable fragments of other and probably new species.

The species named in the above list are 75 in number. Of these no less than 46 have been described by me for the first time in the *Annals and Magazine of Natural History*, for 1876 and 1877. Several of them were also procured in the *Porcupine*, *Challenger*, and *Vöringen* expeditions. A great many more deep-sea species remain to be worked out and described by me from the *Porcupine* expeditions of 1869 and 1870.

I have not included the pteropods in the list, although their shells occur at the greatest depths—because they are oceanic, and inhabit only the surface or superficial zone, their shells falling to the bottom after death and when evacuated by predaceous animals.

The mollusca of very deep water, or the benthal zone, are certainly peculiar, and constitute part of a distinct fauna, notwithstanding that some of them frequent shallower water. It is very difficult to say how far they may be affected by bathymetrical conditions. An important contribution to this part of the subject was made by Mr. Buchanan at a recent meeting of the Royal Society of Edinburgh, in which he stated, as the preliminary result of his analysis of the sea-water collected in the *Challenger* expedition, that as regards the percentage of oxygen present at different depths, it diminishes from the surface to a depth of 300 fathoms, and increases from this point to lower depths.[1] See also my account of the behaviour of *Trochus occidentalis*, when dredged from the deep-sea zone on our northern coasts, which is explained by Mr. Buchanan's statement.[2]

They are not always of a small size. In the *Porcupine* expedition of 1869, the dredge brought up, at the depth of 1,207 fathoms, in the Bay of Biscay, a living specimen of *Fusus attenuatus*, which measures two inches and a quarter in length; and another dredging at the depth of 2,435 fathoms (nearly three miles) in the same part of the Bay, yielded a living specimen of *Dentalium candidum* about an inch and a half long. In the *Challenger* expedition was trawled, at the depth of 1,600 fathoms, in the South Atlantic (S. Lat. 46° 16′, E. Long. 48° 27′), a living specimen of a magnificent shell belonging to *Cymbium* or an allied genus, which has a length of six inches and three-quarters and a breadth of four inches! And during my cruise in the *Valorous*, I dredged, at the depth of 1,100 fathoms, in Davis Strait, a living specimen of *Dentalium candidum* an inch and three-quarters long. These treasures of the deep are so apt to entrance the imagination of a naturalist, that I have often dreamt of walking on the sea-bed and picking up unknown and wonderful shells; and in my waking hours I have envied the faculty of the argonaut in Morris's "Life and Death of Jason,"

> "Euphemus, who had power to go
> Dryshod across the plain no man doth sow."

I hope it is pardonable to avail one's self of a little poetical licence to make the quotation applicable to the bottom as well as to the surface of the sea.

The distribution of the deep-sea mollusca is unquestionably caused by submarine currents, with the direction and extent of which, however, we are unacquainted. As far as I have had an opportunity of judging from the mollusca of the North and South Atlantic, I am inclined to think that the Arctic and Antarctic currents do not extend beyond the Equator. The South-Atlantic species procured by the *Challenger* party in deep water appear to be different from those of the North-Atlantic in similar depths, according to our present notion of species. It is unnecessary for me to renew my objection to the phrase "representative species," as Sir Wyville Thompson has satisfactorily disposed of the matter in page 14 of his "Depths of the Sea."

It will be seen, on referring to the list of deep-water mollusca procured in the *Valorous* cruise, that several of the species are also Sicilian fossils. They occur in the Pliocene formation of the south of Italy. Professor Seguenza has just published a very complete and valuable catalogue entitled "Elenco dei Cirripedi e dei Molluschi della zona superiore dell' antico plioceno,' which are arranged in two divisions, "Depositi littorali" and "Depositi submarini." But some further distinction would seem to be necessary in order to separate the strata, inasmuch as certain species which are assuredly littoral are included in the submarine division. For instance, *Actæon pusillus*, Forb. (which lives at depths varying from 40 to 1,456 fathoms), and *Cylichna ovata*, J. (66–862 fathoms), are entered in both divisions; while peculiarly shallow-water species, such as *Patella vulgata*, *Tectura virginea*, and six now also living species of *Chiton*, appear only in the submarine or deep-water division. Many of the species in Seguenza's Catalogue (besides those noticed in the *Valorous* list of deep-water mollusca), which had been previously considered extinct, were discovered by me in the *Porcupine* expeditions to be still living; and I have no doubt that the rest of the so-called extinct species, from the upper zone of the older Pliocene in Sicily, will sooner or later be detected in future deep-sea explorations. In fact our examination of the abyssal fauna has been hitherto extremely slight and cursory, taking into account the enormous extent of area, the difficulties caused by unfavourable weather, and the inadequacy of the instruments used in the investigation. Our good neighbours, the Norwegians, have not relaxed in their work; and while this Address is being delivered their second year's expedition to the Arctic seas will almost have been completed. May every success attend them!

There has been lately a good deal of controversy as to the supposed "continuity of the chalk"; and the affirmative of the proposition has been most ably argued by my colleague and friend, Sir Wyville Thomson, in his "Depths of the Sea."

Prof. E. Forbes, in his "Report on Ægean Invertebrata" (1844), was, I believe, the first to state the proposition. He says, at p. 178, that the strata in his lowest region, or 230 fathoms, would, if filled up, "present throughout an uniform mineral character closely resembling that of chalk, and will be found charged with characteristic organic remains and abounding in foraminifera. We shall, in fact, have an antitype of the chalk."

Sir Wyville Thomson supports his view by the weighty authority of Dr. Carpenter, Prof. Huxley, and Prof. Prestwich; and although the late Sir Charles Lyell entered a vigorous protest against the hypothesis, and went so far as to designate it a "popular error," I will refrain from expressing any opinion of my own, but will content myself with stating a few facts in elucidation of the question.

The comparison of the deep-sea ooze with the geological formation known as chalk depends on two points, viz., the mineral composition and the organisms belonging to each.

1. *Mineral Composition.*—The late Prof. David Forbes, whose knowledge as a mineralogist and chemist was universally recognized, furnished me, on my return from the *Porcupine* expedition of 1869, with a complete analysis of a sample of Atlantic mud procured at a depth of 1,443 fathoms. He proved that it differed from ordinary chalk in containing scarcely more than 50 per cent. of carbonate of lime, whereas chalk consisted all but entirely of carbonate of lime. Indeed Sir Wyville Thompson admits that "a more careful investigation shows that there are very important differences between them."

2. *Organisms.*—I must here confine myself chiefly to the mollusca, which Sir C. Lyell regarded as "the highest or most specialized organization" on which geological reasoning and classification are founded.

Misled by the apparent resemblance of Mediterranean and Atlantic ooze to the ancient chalk, geologists have been accustomed to consider the chalk fauna as having lived in deep water. Let us see how this is with respect to the mollusca. I have lately, with the assistance of Mr. Henry Woodward and Mr. Etheridge, examined the cretaceous mollusca in the British Museum and the Museum of Economic Geology; and Mr. Etheridge has most obligingly prepared and furnished me with a tabular list of the genera and number of species in each genus in the upper cretaceous group (exclusive of the gault and greensand), which list I will, with his permission, here insert:—

Genera.	No. of species in Chalk-marl.	No. of species in Lower Chalk.	No. of species in Upper Chalk.
BRACHIOPODA.			
Argiope	1	1	...
Crania	1	2	2
Kingena	1	1	1
Magas	2	1	1
Rhynchonella	13	7	3
Terebratella	2	1	2
Terebratula	11	10	4
Terebratulina	2	2	2
Telebrirostra	1	...	...
Thecidea	...	1	1
Trigonosemus	1	1	1
Total	35	27	17

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[1] NATURE, June 14, 1877.

[2] "British Conchology," vol. iii. pp. 335, 336.

Genera.	No. of species in Chalk marl.	No. of species in Lower Chalk.	No. of species in Upper Chalk.
LAMELLIBRANCHIATA. (Conchifera.)			
Avicula	1	1	...
Exogyra	3	2	3
Gervillia	?	...	...
Inoceramus	6	8	9
Lima	3	9	7
Ostrea	3	5	11
Pecten	5	10	12
Pinna	...	...	2
Plicatula	2	1	1
Spondylus	4	2	4
Arca	2	2	1
Astarte	...	1	1
Chama	...	...	1
Cypricardia	...	...	1
Diceras	...	...	1
Isocardia	...	...	1
Leda	...	1	...
Modiola	...	1	1
Opis	1	1	...
Pholadomya	1	1	3
Teredo	1	1	2
Trigonia	3	...	...
Unicardium	...	1	1
Venus	1	1	...
	36	48	62
Radiolites	...	?	1
(Solenoconchia.)			
Dentalium	1	1	...
GASTROPODA.			
Actæon	1	1	...
Aporrhais	2	3	...
Avellana	1	1	...
Calyptræa	...	...	1
Cerithium	1	1	...
Columbellina	2	...	...
Dimorphosoma	2	...	...
Dolium	...	1	...
Emarginula	1	2	...
Fusus	...	1	2
Gibbula	1	...	...
Hipponyx	...	...	1
Natica	...	1	...
Patella	...	...	1
Pleurotomaria	3	2	3
Pterocera	1	...	...
Rostellaria	1	...	...
Scalaria	...	1	1
Solarium	1	3	1
Trochus	1	1	2
Turbo	...	3	...
Turritella	...	1	1
	18	22	13
CEPHALOPODA.			
Ammonites	29	31	9
Ancyloceras	1	...	...
Aptychus	...	...	6
Baculites	1	2	1
Belemnitella	1	2	3
Belemnites	3	2	...
Crioceras	...	...	1
Hamites	...	3	1
Helicoceras	...	...	1
Nautilus	10	10	2
Scaphites	1	2	1
Turrilites	11	11	...
Annisoceras	1	...	...
	58	63	25

A glance at the above list, and, much more, an inspection of the chalk mollusca in a good collection, ought to convince any conchologist that all these genera were comparatively shallow-water forms. I should infer that the depth might have been from low-water mark to 40 or 50 fathoms. None of the genera are deep-water. *Chama*, *Ostrea*, *Pinna*, *Calyptræa*, *Hipponyx*, and, most assuredly, *Patella* cannot be placed in the latter category; and the old proverb, "noscitur ex sociis," will apply to mollusks as well as to men. *Teredo* may have been littoral or have come from floating wood. Not a single species of *Leda*, *Pecchiolia* (or *Verticordia*), *Neæra*, not one of the Solenoconchia, nor of the *Bulla* family occurs in the upper or white chalk, although they now inhabit the deep-sea ooze and especially characterize the modern deposit.

But *Nautilus* and *Spirula* are believed by some to be deep-water forms. This must be a mistake. Although the animal of that common species *Nautilus pompilius* has rarely been met with, the shells are often found on beaches in the Indian Ocean and South Pacific; and I am not aware of any instance of a deep-water mollusk being cast ashore. It is not likely. Rumphius (the "Plinius Indicus"), in his "Amboinsche Rariteitkamer," or Cabinet of the Curiosities of Amboyna, 1705, has given an interesting account of the habits of the pearly nautilus, a translation of which I will copy from the admirable monograph of Professor Owen:—"When the nautilus floats on the water, he puts out his head and all his tentacles, and spreads them upon the water, with the poop of the shell above water; but at the bottom he creeps in the reverse position, with his boat above him, and with his head and tentacles upon the ground, making a tolerably quick progress. He keeps himself chiefly upon the ground, creeping also sometimes into the nets of the fishermen; but after a storm, as the weather becomes calm, they are seen in troops, floating on the water, being driven up by the agitation of the waves. This sailing, however, is not of long continuance, for having taken in all their tentacles, they upset their boat, and so return to the bottom."

As to the *Spirula*, the old Dutch naturalist remarked that it attaches itself to the rocks, and is thrown up on the beach when the north wind blows. Péron found the first living specimen in Australia; Mr. Percy Earl obtained one on the coast of New Zealand; the late Sir Edward Belcher another in the Indian Archipelago; Mr. Bennett got one off Timor; and an imperfect specimen was procured in the *Challenger* Expedition. I was favoured, in January, 1875, by Mr. J. Tyerman, of Tregeny, sending for my inspection a perfect specimen of *Spirula australis* and one of *Argonauta gondola* in spirit of wine, with a memorandum that "the *Spirula* and *Argonauta* were taken by a friend while dredging or, rather, skimming for pteropods in the Persian Gulf." Mr. Tyerman added that other live specimens of the *Spirula* were captured at the same time. Sir Lewis Pelly informs me that the Persian Gulf is nowhere deeper than between 40 and 50 fathoms. *Spirula* has apparently the same habit as species of *Loligo* and allied genera, in occasionally frequenting the surface of the sea. The shells of *S. australis* are thrown up in considerable numbers on every beach in the North Atlantic, having been wafted northwards by winds and the equatorial current or so-called "Gulf Stream."

Assuming, therefore, that the usual habitat of mollusca in past epochs did not differ from that of recent mollusca of the same kind, I think we may safely conclude that the shells of the cretaceous system, or, more strictly, the upper chalk, belonged to shallow and not deep-water mollusca.

Mr. Woodward tells me that the chalk crustacea are also shallow-water forms.

The white chalk is in many places principally composed of *Globigerinæ*, *Orbulinæ*, and coccoliths or coccospheres, all of which inhabit at present the surface of the sea. According to Dr. Wallich, *Globigerina* is found in all latitudes and at all depths, ranging from 50 to 3,000 fathoms.[1] Mr. Parker and Prof. Rupert Jones (first-rate authorities on the foraminifera) admit that *Orbulina* and *Globigerina* are "occasionally found in shallow water."[2]

I cannot identify a single species of the cretaceous mollusca as now living or recent. All of them are evidently tropical forms. One of the cretaceous species, indeed, *Terebratula striata*, Wahlenberg, has been supposed by some palæontologists to be identical with *T. caput-serpentis*, the latter of which has a range of bathymetrical distribution from low-water mark to

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[1] "North-Atlantic Sea-bed," p. 137.

[2] *Quart. Journ.* Geol. Soc., vol. xvi. p. 279.

808 fathoms; its geographical extension is equally great, and it has also not a slight amount of variation in shape and sculpture. But I am not disposed to unite the two species. In *T. striata* the ribs are much narrower than in the typical *T. caput-serpentis* and are finely beaded or tuberous, especially towards the beaks, and they are not so close together as in the variety *septentrionalis*. This question of identity depends, however, on the capability of hereditary persistence which some species possess; and although a certain degree of modification may be caused by an alteration of conditions in the course of incalculable ages, our knowledge is not sufficient to enable us to do more than vaguely speculate, and surely not to take for granted the transmutation of species. We have no proof of anything of the kind. Devolution, or succession, appears to be the law of nature; evolution (in its modern interpretation) may be regarded as the product of human imagination. I am not a believer in the fixity of species, nor in their periodical extinction and replacement by other species. The notorious imperfection of the geological record ought to warn us against such hasty theorization. We cannot conceive the extent of this imperfection. Not merely are our means of geological information restricted to those outer layers of the earth which are within our sight, but nearly three-fourths of its surface are inaccessible to us, so long as they are covered by the sea. Were this not the case, we might have some chance of discovering a few of the missing links which would connect the former with the existing fauna and flora. It is impossible even to guess what strata underlie the bottom of the ocean, or when the latter attained its present position relatively to that of the land. The materials of the sea-bed have been used over and over again in the formation of the earth's crust; "Omnia mutantur, nihil interit;"[1] and the future history of our globe will, to the end of time, repeat the past. What does Shakespeare say, as a geologist, to such cosmical changes?

> "O heaven! that one might read the book of fate,
> To see the revolution of the times
> Make mountains level, and the continent
> (Weary of solid firmness) melt itself
> Into the sea! and, other times, to see
> The beachy girdle of the ocean
> Too wide for Neptune's hips."

There is also the difficult problem of submarine light, evidenced by the facts of deep-sea animals having conspicuous and well-formed eyes, and of the shells of deep-sea mollusca being sometimes coloured, which is yet unsolved.

Much more remains to be done; and probably many generations, nay, centuries, must elapse before the very interesting subject which I have now ventured to submit to your consideration will be mastered or thoroughly understood in all its varied aspects. Let us then confess our ignorance, and conclude in the sublime words of the Psalmist:—"Thy way is in the sea, and thy path in the great waters, and thy footsteps are not known."[2]

THE BRITISH MEDICAL ASSOCIATION.

DURING the past week the British Medical Association held its forty-fifth annual meeting in Manchester. The Committee of Management for the reception of the Association deserves hearty congratulations on the success which has followed their hospitable effort. For although they were under no disadvantages of position or room, but rather the contrary, they had spared no pains whatever to secure the comfort of their guests, and they may fairly be said to have equalled or outdone their opportunity.

The class-room and lecture theatres of the Owens College and Medical School, were placed at the disposal of the Committee, and gave the Association most convenient means of holding its general business and sectional meetings. The large museum at the Medical School, the dissecting room, the physiological laboratory, the chemical laboratories and the engineering drawing room were set aside for the purposes of the Annual Museum. The museum—very extensive this year—included besides pathological and surgical specimens, plates, casts, &c., an unusually large number of histological specimens, chiefly of morbid tissues. In addition there was the usual display of surgical and scientific instruments, the latter being reinforced by the collection of physiological apparatus belong to the Medical School. The general meetings except the first were held in Prof. Roscoe's lecture theatre, and there also were delivered the special addresses in medicine, Surgery, Obstetrics, and Physiology. The first general meeting, and the address of the President of the Association took place in the Concert Hall, none of the college rooms being large enough for the purpose. A temporary covered way joined the Medical School to the College, and on the ground between the two buildings was erected a tent or series of tents in which were exhibited a large number of sanitary appliances under the auspices of the Manchester and Salford Sanitary Association.

The meeting of the Association was inaugurated on Tuesday morning by the Bishop of Manchester who preached a sermon in the Cathedral; and in the afternoon of the same day the first general meeting was held for the election of the president for the year and for hearing the Report of Council. The retiring president Dr. De Bartolomé, of Sheffield, alluding to the events of his official year, spoke with much spirit of the manner in which the public services of the medical men engaged in the rescue of entombed miners after the Pont-y-pridd colliery accident had been ignored by the Government and the nation; and he announced that the Council, having regard to the fact that there was no provision for the recognition of heroic or meritorious services when performed by medical men as such, had determined to confer upon the medical men concerned in the accident a medal and a testimonial scroll, and had recommended that the medal should be perpetuated as the Medal of the British Medical Association, to be awarded for like acts in the future. The latter suggestion was afterwards adopted at the second general meeting.

The president, Dr. M. A. Eason Wilkinson, Senior Physician to the Manchester Royal Infirmary, having been elected, delivered an address on Hospital Defects and their Remedies a subject which is greatly engaging the attention of local medical men. He gave a history of the Manchester Royal Infirmary and spoke with satisfaction of the union of the School of Medicine and the Owens College.

In the evening there was a reception by the President of the Association and the Senate and Council of the Owens College, held at the College.

On Wednesday a general meeting of the Association assembled to hear the special address in Medicine, by Dr. William Roberts, F.R.S., on the subject of Spontaneous Generation and the doctrine of Contagium Vivum. Dr. Roberts' treatment of the subject may be considered to fall into three divisions—physiological, pathological, and theoretical.

In the first, after alluding to the anology which may possibly be real, between contagious fever and the action, say, of yeast in fermentation, he proceeded to consider two propositions. The first proposition is: That organic matter has no inherent power of generating bacteria, and no inherent power of passing into decomposition. To substantiate this he exhibited specimens of decomposable organic fluids which, having been sterilized, had remained in his possession undecomposed for many months or even years. Sterilization had been effected three ways:—

1. By prolonged boiling, the exclusion of germs being afterwards secured by plugs of cotton-wool.

2. By filtration through unglazed earthenware previously heated to redness, into flasks sterilized by the heat of boiling water.

3. By transferring the organic decomposable fluid, such as blood, urine, pus, etc., directly from the interior of the body to well sterilized flasks and subsequently defending them from germs by plugs of cotton-wool.

The second proposition is:—That bacteria are the actual agents of decomposition. This Dr. Roberts considers to be proved by the following considerations:—

a. That which originates decomposition comes from the air; since removal of the plugs in any of the above cases is infallibly followed by decomposition.

b. That which originates decomposition consists of solid particles floating in the air; since filtration of the air (as above) is able to prevent decomposition: and air which is optically pure (Tyndall) has no fecundating power.

c. That which originates decomposition has not the nature of a soluble ferment; since decomposable fluids *in which putrefaction has already set in* yield filtrates through earthenware, which do not decompose, while pepsin, diastase, &c., readily pass through the same medium.

But it is nevertheless true that certain liquids, as neutralized hay, infusions, and milk, often produce bacteria even after they have been boiled for two or three hours, and when there is no possibility of subsequent infection. And it is equally true that bacteria are invariably killed by exposure to a temperature of about 140° F. or more. Are not these facts strong evidence of abiogenesis? No: and for the following reasons:—

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[1] Ovid, Met. xv. 165.
[2] Ps. lxxvii. 19.

1. Although bacteria infallibly die at the above-named temperature their spores may not; and this is more than probable since Dallinger and Drysdale have demonstrated that while certain septic monads are destroyed on heating to 140° F., their spores survived a heat of 300° F.

2. Cohn has examined the organisms which arise under the conditions named, viz., in boiled hay-infusions, and he has demonstrated that they are never a new creation as might have been expected, but invariably the well-known bacterian *Bacillus subtilis*. Is it possible to believe, in the face of the whole theory of evolution, that abiogenesis is able at one stroke, and within seventy hours, to produce such a specialised organism as this?

3. Saprophytes are devoid of chlorophyll and hence cannot assimilate carbonic anhydride; they get their carbon exclusively from more complex carbon compounds. Hence, at least, it must be granted that saprophytes cannot have been the primordial forms of life; and the probability of the spontaneous generation of such organisms, even granting spontaneous generation as an existing process, falls in proportion.

In the second division of his address Dr. Roberts discussed the pathology of three infective diseases the cause of which has been traced in each case to infective organisms, viz., septicæmia, relapsing fever, and splenic fever. Without entering into details, it may be allowed to notice that Dr. Roberts, alluding to the subject of the antiseptic treatment of wounds, spoke of the need of a broader view of its principle, the essence of which is not to protect the wound from septic organisms, but *to defend the patient against the septic poison* generated under the influence of those organisms, an end which may be obtained either by the method of Prof. Lister of rigid exclusion of the septic organisms or by preventing the absorption of the pyrogen product, as, *e.g.*, by allowing free exit for the discharges by the open method of treatment.

In conclusion, Dr. Roberts pointed to the fact that there exists a remarkable morphological identity between the organisms of certain infective diseases and other quite harmless saprophytes. Thus *Bacillus anthracis* of splenic fever only differs from *Bacillus subtilis* in the fact that its rods are motionless: while the spirilla of relapsing fever are identical in form and botanical characters with *Spirochœte plicatilis* of Ehrenberg. May not these coincidences, he suggests, point to a natural explanation of the origin of contagia? May not the harmful organisms be merely variations or sports from the harmless saprophytes resembling them, just as the bitter almond is a sport from the sweet, and the nectarine from the peach? May not typhoid fever for example, be explained as due to a variation from some common saprophyte of our stagnant pools or sewers, which under certain conditions of its own surroundings or certain conditions within the human body, acquires a parasitic habit?

On Thursday the members of the Association assembled to hear the special address in Surgery by Mr. T. Spencer Wells, F.R.C.S. After giving a retrospect of the progress of surgery, and noticing the important advances made during quite recent times, the speaker said:—

"A certain section of the community, well-meaning it may be, but led astray by thoughtless enthusiasts or self-interested itinerant lecturers, vehemently asserts that if we are to perfect ourselves in these or in other modes of saving human life or lessening human suffering, we must only do so by practice upon the human subject; we must not, as a surgeon or a physiologist, take the life of a dog or a cat, a rabbit or a sheep, a pigeon or a frog, for any scientific purpose, or with the object of benefiting the human race. Anybody may slaughter oxen and sheep by thousands for human food in any way he pleases, oysters may be eaten alive, the pheasant or the partridge, the fox or the deer may be expressly reared to supply the sportsman with exercise or the amusement of killing—in a word, the lower animals may be devoted to the use of man for any purpose that is not scientific. But if a surgeon experimentally sacrifices half a dozen dogs or rabbits in the hope of improving some operation which may prevent the loss of human life or lessen human suffering, he is branded as inhuman, and barely escapes the supervision of the police. Possibly, some of these benevolent individuals will voluntarily offer up themselves to our Committee on Transfusion, in the hope of perfecting the practice. Until they do so, they will perhaps be a little less clamorous if a few sheep or rabbits are used in the cause of humanity."

Referring afterwards to the rewards of public service in the medical profession and the need of medical statesmanship, Mr. Spencer Wells sought to show that it would be both just and conducive to the highest public welfare that eminent members of the medical profession should be occasionally admitted to a seat in the House of Lords.

The Section of Physiology was opened with an address by the president, Prof. Arthur Gamgee, F.R.S., of Manchester. After giving a *résumé* of some of the important physiological work of the last year, including an account of the latest contributions by Engelmann and Hermann to the contact theory of the muscular current, Prof. Gamgee referred to the wishes of some physiologists and medical men to destroy the very intimate connection between medical and physiological science which at present exists in England, and deprecated any such attempt, pointing out the benefits which had resulted to both sciences by their mutual reaction. He strongly insisted, however, on the need of a sound preliminary scientific training for medical students if physiology is not by a natural process to split off from medicine owing to the sheer incapacity of the average medical student to comprehend her teachings. Prof. Gamgee concluded his address by referring to the vivisection agitation, and to the presence of Prof. Ludwig in Manchester, in the following terms:—

"We have passed, or rather we are passing, through a period of great anxiety to physiology. A popular clamour, unfortunately too well known to all of you, has imperilled the studies which we all have so greatly at heart. An Act of Parliament is now in force which, if interpreted in a spirit of hostility to science, might put a stop to these studies. But I trust that the spirit of the time, the spirit of justice too, which we think characterizes our countrymen, will render such hostility impossible, and relying upon the justice and enlightenment of the minister of the Crown to whom the enormous responsibility of carrying out this Act has been entrusted, we venture to predict that the interests of science will not ultimately suffer.

"I cannot close this address without expressing the gratification and pride with which I see amongst us the eminent man who to-day honours us by his presence. In Carl Ludwig we see one of the three or four men who, more than all others, have helped to build up the present edifice of physiology—a man to whom those of our science will refer in ages yet to come as having, perhaps more than anyone else, introduced methods of precision into physiology, and, by numerous conquests in nearly all its departments, proved their utility. We welcome him amongst us, and beg to assure him that the influence of his teaching extends not only to every university of Germany, but even to us. All of us have more or less directly learned from him, and all of us are, I trust, inspired in some measure by his intense devotion to science. All would, I hope, emulate to the extent of their powers, the example of the great head of the Leipzig physiological school, who, in unselfishly contributing to the success of his pupils, for the furtherance of the science which he loves, has seen the fullest realization of his proudest hopes."

In the evening the annual dinner of the Association was held in the Large Hall of the Assize Courts.

Prof. Kronecker, of Berlin, exhibited a current interrupter to secure equal intensity of opening and closing shock. The current is made by the point of a swinging bar which is kept swinging in a vertical plane by means of an electro-magnet. The point dips into mercury in order to make the current, and leaves the mercury to break it. The surface of the mercury is continually washed free from oxidized metal by a stream of water.

Prof. M'Kendrick, of Glasgow, read a paper *On the Physiological Action of the Chinoline and Pyridine Series of Compounds*, containing the results of an extended research carried on with the co-operation, first of Prof. Dewar, of Cambridge, and afterwards of Dr. Ramsay, of Glasgow. In justice to Prof. M'Kendrick and his colleagues we will not attempt to give even an outline of the peculiar effects of these series of bodies, especially as the whole of the valuable memoir will shortly be published. But we may call attention to the following inferences of more general physiological interest:—

1. There is no appreciable difference between the physiological action of the bases obtained from chincona and those derived from tar.

2. All the substances examined are remarkable for not possessing any specific paralytic action on the heart likely to cause syncope, but they destroy life, in lethal dose, either by exhaustive convulsions or by gradual paralysis of the respiratory centres, thus causing asphyxia. There is further no immediate action on the sympathetic system of nerves, but there is

probably a secondary action since after large doses the vaso-motor centre becomes involved.

3. In ascending the chinoline series, the physiological action changes in character, the lower members seeming to affect the sensory encephalic centres and the reflex centres of the cord, destroying voluntary and reflex movement; the higher members seeming to affect chiefly the motor centres, causing violent convulsions, and afterwards paralysis.

4. Speaking of these series of bodies, the mere knowledge of the constituent elements of a body is no guide to its physiological action.

5. Speaking of these series of bodies, the base, and not the acid with which it may happen to be united, determines within slight limits, the physiological character of the compound.

6. The union of methyl, ethyl, amyl, and allyl with chinoline does not entirely change its characteristic mode of action, but their presence causes a tendency to spasm and convulsion. Also in the case of the pyridine and picoline substitution compounds, increase of molecular complexity and weight does not indeed entirely change the mode of action of the simpler compound, but is always attended by a tendency to spasm and convulsion.

7. When the bases of the pyridine series are doubled by condensation, producing polymers such as dipyridine, &c., they not only become more active physiologically, but the mode of action of the condensation product differs from that of the simpler base.

Saturday was entirely taken up by excursions into the surrounding counties, Lancashire, Cheshire, and Derbyshire.

Throughout the week facilities were given to members of the Association to visit the various hospitals, libraries, manufactories, and buildings of interest in the town and neighbourhood.

OUR BOOK SHELF

Jainism; or, the Early Faith of Asoka. By E. Thomas, F.R.S. (London: Trübner and Co., 1877.)

THIS is a book which will be of great interest to orientalists and students of the science of religion and is likely to occasion a good deal of controversy. It embodies two articles published by the author in the *Journal* of the Royal Asiatic Society, the first of which endeavours to show that the Greek monograms on Bactrian coins represent dates, the hundreds being omitted in imitation of the Hindu *loka kala*, or as when we write '77 for 1877. The dates, Mr. Thomas thinks, refer to the Seleucid era (B.C. 312), and we are therefore able to place the Indo-Scythian dynasty of Kanishka, whose monuments at Mathurâ have recently been discovered, from B.C. 2 to A.D. 87. The second article challenges the usual opinion that Jainism is a late corrupt form of Buddhism and seeks to prove that Buddhism itself was originally a Jainist sect and that Asoka, the Constantine of India, was a Jainist before he was a Buddhist. His grandfather, Chandra Gupta or Sandracottus, is claimed by the Jainists, and their claim is supported by the testimony of Megasthenes; according to Abúl Fazl, Asoka himself introduced Jainism into Kashmir, and the gradual passage of his belief from Jainism to Buddhism may be detected in his rock and pillar edicts. The Bhabra edict, late in his reign, first contains positive Buddhism, and his earlier Jainist title of *Devánampiya* or "beloved of the gods," is dropped as incompatible with a creed which denied the existence of any God at all. The Maháwanso has allowed a reference to "the twenty-four supreme Buddhos"—the number of the Jainist saints—to remain in its text, and the symbols of the Buddhas are borrowed from their Jainist prototypes. The existence of Jainism at the beginning of the Christian era is proved by the recent discoveries at Mathurâ, where the figures are nude as among the Jainists, not clothed as among the Buddhists, and the Kanishka coins lately found at Peshâwar are further evidence of Saivism and the worship of many deities, Indian, Persian, Greek, and even Roman, but not of atheistic Buddhism. It may be added that Mr. Thomas believes that in these Kanishka coins we have evidence of the soldiers of Crassus having been settled in the extreme north-west of India.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Rainbow Reflected from Water

ON Monday last, August 6, at 7 P.M., I was standing at the end of Eastbourne Pier looking towards the sea. Behind me the sun was low on the horizon but shining brightly, overhead and out to sea rain was falling from somewhat broken masses of clouds. In front a brilliant rainbow formed a complete semicircle, the two ends apparently resting on the sea, and outside the principal bow the secondary arc was shining with considerable intensity. The sea was calm, but its surface was by no means glassy, being ruffled over with minute wavelets. Reflected from the surface of the sea, and extending in a broken curved line from the extremities of the rainbow nearly up to the pier, was a complete reproduction of the colours in the sky; the reflection, bearing in mind the ruffled surface of the water, being perfect. Not only were the colours of the primary bow reproduced, but a faint reflection of the secondary bow could be detected, whilst the dark space between the two bows, and the luminous haze which always extends for some distance from the concave edge of the primary bow, were distinctly reproduced.

The intensity of the reflected image was, as near as I could judge, one-fourth that of the actual bow. Near the horizon when the real and reflected arcs seemed to touch, there was a decided displacement of the colours, as if the diameter of the reflected bow was somewhat less than that of the original. In other words, the reflected red was not accurately in line with the red in the real bow, but appeared to line with the yellow, whilst the yellow of the reflected arc seemed a continuation of the green in the actual rainbow.

I regret that I had no polarising apparatus with me.

August 8 WILLIAM CROOKES

Science Lectures in London

IT has occurred to me, while reading some lectures given in Glasgow and Manchester, that were such lectures given in London during the winter months, they would confer a great boon upon a large number of people.

Perhaps, Sir, if you were to propose such a thing through the medium of your valuable paper, some might be found to bring about such a result, as I believe no difficulty would be found in forming a society such as the Glasgow Science Lecture Association.

I hope some abler pen will be found to take up the subject.

Herne Hill, August 7 L. JEANS

Strange Dream Phenomenon

I HAVE just experienced almost as remarkable a coincidence as those adduced by Sir W. Hamilton to prove the activity of consciousness even in sleep.

I had not been to rest for forty-one hours, and was overcome by sleep while in the act of writing a short diary I am in the habit of keeping. During the time I was asleep, I dreamed of some house property in Brighton, a dream purely fictitious and very remote from anything I had previously thought of. Awakening in a short time (scarcely a minute), I found myself still writing; and on further examination I discovered that I had, following the current of my thoughts, written as much of my dream as time had allowed. J. VINCENT ELSDEN

Edelweiss

AS an old Alpine man, let me say that Edelweiss is not localised in any part of the Alps; I have found plenty of it at far less altitudes than the natives sometimes try and make awe-stricken tourists believe.

Your paragraph is otherwise correct, more so than one in a contemporary which said that Italian herdsmen were exterminating the flower; *they*, at all events, are not likely to show such Alpine prowess. MARSHALL HALL

Scientific Club, 7, Savile Row, W., August 13

OUR ASTRONOMICAL COLUMN

THE COLOURS OF α URSÆ MAJORIS AND ARCTURUS.—Mr. T. W. Backhouse, writing with reference to the periodical change of colour in α Ursæ Majoris, indicated by the observations of Herr Weber, of Peckoloh, of which mention was made recently in this column, remarks that after watching it for several months he concludes the variation must be very slight. Mr. Backhouse observed with a small binocular, power $2\frac{1}{2}$, with which he estimated the depth of colour as compared with β Ursæ Majoris and β Ursæ Minoris, which are respectively bluish and deep orange, not the actual colour of the star. Representing the difference between the two stars of comparison by a scale from 0 to 1, "the depth of colour of α Ursæ Majoris only varied from $\frac{4}{7}$ to $\frac{3}{4}$ on this scale, so that the whole range of its variations was less than $\frac{1}{5}$ of the difference between those two stars, probably not greater than can be accounted for by errors of observation. The greatest depth of colour, assuming β Urs. Maj. and β Urs. Min. not to vary, was observed on January 6, March 18, and April 11, which are not long after Weber's times of minimum." Mr. Backhouse adds that if any period is indicated by his observations, it is more likely to be one of three months than any other.

Herr Weber observed with a Steinheil achromatic of $2\frac{2}{3}$ inches aperture and $3\frac{1}{2}$ feet focal length, power, 90. For details the reader may consult *Astron. Nach.*, Nos. 1663 and 1773.

Writing on the subject of periodical variation in the colour of a fixed star, we recall some observations by Prof. Julius Schmidt, now director of the observatory at Athens, published upwards of twenty years since, from which he concluded that a very marked change of colour had taken place in the light of Arcturus at least in the year 1852, as compared with ancient observations, and indeed with quite recent ones. On March 21, 1852, while waiting for a meridian observation of Arcturus at the Observatory at Bonn, Prof. Schmidt remarked to his surprise, that this star, which for eleven years previously he had considered one of the most decidedly red stars in the heavens, was, to the naked eye, only yellowish white, or as he says, to speak more correctly, "matt graugelb." During the transit there was no ruddiness either in a dark or illuminated field, and on going out into the open air and comparing Arcturus with other stars of the first magnitude, he found it light yellow, even Capella showing more colour. A comparison with Mars, which he had often previously made since the year 1841, was no longer possible, as with reference to the colour of the planet, "Arcturus appeared as a white star." Thormann, on the same night, considered the star "a dirty yellow, like the colour of slightly tarnished brass." The star was often observed at Bonn in this year, and though Argelander was at first sceptical as to any variation of colour, at the end of March he was convinced that Arcturus "exhibited no deeper yellow tinge than Capella, and that red was entirely absent." On May 11 Schmidt considered there was hardly an appreciable difference in colour from that of Capella. He considered that the circumstance of the star having lost for a certain time in the year 1852 every trace of redness was no illusion, but was quite confirmed. Arcturus was one of the six *ὑπόκιῤῥοι* or fiery red stars of Ptolemy, in which list Sirius, now without a trace of ruddiness, was also included.

The difficulty and uncertainty attending observations on the colours of the stars are considerable for reasons which are sufficiently obvious. There is, perhaps, no more striking instance of the little value attaching to isolated observations or observations not undertaken for the special object, than that afforded by the binary star γ Leonis, of which Struve says: "Est stella duplex in hemisphærio boreali pulcherrima et splendore et coloribus," and the colours he assigned in the "Mensuræ Micrometricæ," golden for the larger star, and greenish-red for the smaller one, have been repeated with trifling variation of terms by subsequent observers. Yet on the date of discovery of the duplicity of the star by Sir W. Herschel, February 11, 1782, we find he assigns for the colours "L. white, S. white, inclining a little to pale red," differences which, but for the reason just mentioned, might well countenance the idea of a total change in the colours of both components. It may be added that in γ Delphini, we have a very similar case.

THE SYSTEM OF 40 o^2 ERIDANI.—M. Camille Flammarion communicates some measures of 40 Eridani and its companions made early in the present year. For the epoch 1877·12, the following are the results of his observations:—

Stars AD	Position	148°·0	Distance	37″·2
AE	,,	339·2	,,	109·9
AB	,,	104·7	,,	81·5
BC	,,	130·0	,,	4″ ±

The secular proper motion of the star A is, according to

Struve, in R.A.	... − 216″·8 ...	in Decl.	...	− 342″·3
Mädler, ,,	... − 218·8 ...	,,	...	− 347·0

And as M. Flammarion remarks on allowing for the proper motion of the principal star, Prof. Winnecke's measures at the epoch 1864·84 compared with his own, show that the stars D and E do not form part of the physical system of 40 Eridani. His measures of A B agree with Struve's in proving that B, while partaking of the great proper motion of A, is yet approaching it slowly. C continues its rapid retrograde change of position with respect to B, the distance remaining nearly the same, and if the stars be regularly measured with powerful instruments, we must soon have sufficient data for an approximation to the elements of the orbit. When some one of the southern observatories possesses a heliometer, and we do not know of any instrumental addition to an establishment in a more southern latitude from which results of greater interest and importance in this branch of astronomy can be expected, the determination of the parallax of 40 Eridani, and it may be added of ϵ Indi ought to be amongst the first objects to which it is devoted.

THE THIRD RADCLIFFE CATALOGUE OF STARS.—In the late Annual Report of the Rev. R. Main, the Radcliffe Observer, to the Board of Trustees, it is stated that considerable progress has been made with the compilation of the third catalogue founded upon observations made at this important astronomical establishment, from 1862 to 1870, and additional assistance is proposed for its speedy completion, though no definite time is assignable for publication. The number of stars contained in the new catalogue is 4,389, or nearly twice the number included in the "second Radcliffe Catalogue," which was the first issued by the present director. The same Report mentions that the planet Mercury had been meridionally observed thirty-nine times during the year ending with June, 1877, a large number considering the difficulty attending the observations; but it is not the first time that the Radcliffe observer has had occasion to report successfully in this direction; some few years since we believe as many as forty-five observations were secured with the transit-circle in the course of the twelvemonth.

NEW MINOR PLANET.—A telegram from M. Stephan announces the discovery of a small planet by M. Borrelly, at Marseilles, on the 11th inst. At 8h. 35m. its R.A. was 21h. 19m. 50s., and N.P.D. 105° 59′; tenth magnitude.

ON BLACK SOAP FILMS

IT is a matter of general knowledge that a very thin plate of a transparent substance exhibits colours, the tints of which depend upon the angle of incidence of the light by which it is viewed, and also upon its thickness and refractive index, and that these "colours of thin plates" were classified by Newton and divided by him into *orders*. If, however, such a plate is of less than a certain thickness, it appears black whatever the angle of incidence of the light may be, and its colour, therefore, gives us no indication of its thickness, except that it must be less than a particular value.

With a well-made soap-solution it is by no means difficult to obtain a film thin enough to appear black, and special interest attaches to observations made on it when in this state of extreme tenuity, as the thinner it becomes the more nearly must its thickness be comparable with the distance at which the forces exerted upon one another by the molecules of which it is composed cease to be sensible. Now under whatever conditions a film which is part black may have been formed, and whatever its shape may be, a very rapid and remarkable change of thickness invariably (as far as the writer's experience goes) occurs at the boundary which divides the black from the coloured portion of the film.

It is indeed true that contiguous portions of films are often of different thicknesses. Thus, when a bubble is first blown, streams of various colours circulate rapidly over all its surface, but as a rule, these miniature convulsions cease in a short time, and the colours appear in regular order from top to bottom, each tint shading off gradually into those which precede and follow it. Sometimes, too, and especially when a film becomes very thin, specks of a slightly different colour from the rest are observed moving upon its surface in paths which are often very regular. Thus in a cylindrical film which had been formed several hours and the top of which had become black, while the rest exhibited the blue of the second order, flakes of a deeper blue than the main body of the film were seen to move rapidly for many minutes in such a way as to indicate that a regular circulation was kept up in the film. Two horizontal streams moved in opposite directions round the base of the cylinder; at their meeting-point they united, and the direction of the current became vertical, but on approaching the boundary of the black, the stream again bifurcated, and separate currents flowed in opposite directions round the upper edge of the blue, which again uniting on the other side of the cylinder, flowed down to the base and resumed their original directions. Slight differences in the temperature of different parts of the film would sufficiently explain these currents, which are only mentioned here because the coloured flakes by which they were rendered conspicuous proved the existence of inequalities in the thickness of the film. These and the other variations in thickness referred to above, are, however, slight compared with those which are often observed at the edge of the black, as to the naked eye whole orders of colours (which can, however, in general be seen by means of a microscope) may appear to be missing between the black and the portion of the film which is in contact with it. Thus, in one case which has been noted, the latter must have been fourteen times, and very probably was more than forty times, thicker than the black itself.

But even when the colour next the black is the white of the first order, we do not observe, as we should if the change in thickness were very gradual, an undefined boundary between the two, shifting with every variation of the angle of incidence of the light by which it is viewed, but rather a definite line of demarcation ruled, as it were, across the film, the position of which is independent of the direction of the incident light, and at which the change of thickness, if much less than that observed in the extreme case above cited, is nevertheless very considerable.

Both, then, by its comparative magnitude and by the constancy with which it recurs, this phenomenon seems to suggest some special connection between the formation of the black portion of the film and the molecular forces which are at play in the liquid of which it is composed.

At the edge of the black itself, however, optical methods fail, as above explained, to give us any further help in investigating the form of the film, but some information has been obtained on this point in the course of a series of experiments on the electrical resistance of soap films, upon which Prof. Reinold and the author of this article are engaged, which they have recently made the subject of a communication to the Royal Society. To determine the thickness of a uniform soap film by measuring its electrical resistance and (having previously determined the specific resistance of the liquid of which it is composed) applying Ohm's law that the resistance of a homogeneous conductor varies inversely as its section may at first appear a simple problem; but at the outset the difficulty arises that the molecules of a liquid which lie very near the surface are necessarily in a different state from those in the interior. The latter are subjected to the action of other molecules which are on the average distributed symmetrically around them, while the greater number of molecules which can exert any influence on the former are situated on one side only. Hence the surface of a liquid differs in its physical properties from the interior, and there is no guarantee that the electrical resistance of the liquid in mass is the same as that of a very thin film skimmed off its surface. But although Ohm's law cannot be applied to the case of a very thin film with any certainty that the numbers obtained by its aid will be correct, and the electrical method fails to give thoroughly reliable information as to the thickness of a black film, yet it is possible by experimenting on a film as it gradually thins and the black portion increases in size, to learn from its electrical resistance whether the thickness of the black portion is constant, or whether, as is ordinarily the case with the rest of the films, it is thinner above than below, and by repeating the investigation with different films to determine whether the thickness of a black film depends upon the varying circumstances of its formation. To investigate this question fully will require more time than the authors of the paper above referred to have, as yet, been able to devote to it, but they have obtained very consistent and definite results in the experiments they have already made. The liquid used was M. Plateau's "*liquide glycerique*," with three parts of potassium nitrate added to every 100 parts of water to improve the conductivity.

The apparatus employed consisted essentially of a glass cylinder about 16 c.m. high fitted with an ebonite cover, through the centre of which passed a brass tube which could be raised or depressed at pleasure, and was retained in its position by friction.

To the lower end of the tube was attached a circular brass plate, to which was soldered a ring of stout platinum foil 33·51 mm. in diameter.

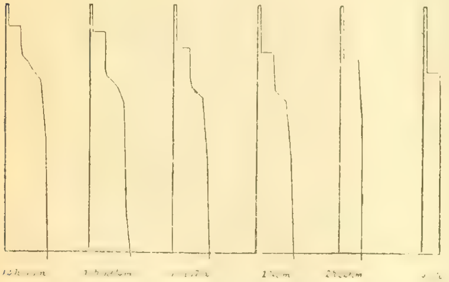
A platinum crucible, the mouth of which was of the same diameter as the ring was placed at the bottom of the glass vessel in a little dish containing mercury. When the apparatus was used, a little of the liquid was poured into the glass vessel and into the platinum crucible, in order to prevent the film thinning by evaporation; a piece of india-rubber tubing provided with a pinch-cock was attached to the upper end of the brass tube, and a plane film having been formed on the platinum ring was blown out, through the tube, into a bubble which, when large enough, adhered to the rim of the crucible. The quantity of air inside the bubble and the position of the various parts of the apparatus were then adjusted so as to make the bubble as accurately cylindrical as possible.

A current could be passed through the film by attaching wires to two binding screws, one of which was affixed to the brass tube, while the other, attached to the ebonite cover, was in conducting communication with the crucible by means of a platinum wire, which dipped into the mercury with which it was surrounded.

This piece of apparatus was included in the arm of a Wheatstone's bridge, by means of which and a delicate reflecting galvanometer the resistance of the film was measured. As the latter continually altered and generally slowly increased, the known resistance on the bridge was made up to a certain amount, and the moment when the resistance of the cylinder became equal to it noted. The part of the total resistance thus measured, which was due to the coloured portions of the films was calculated on the assumption that Ohm's law held good, a process which made it necessary to determine the shapes of the films, which increased its thickness from above to below.

This was done by measuring with a cathetometer the breadths of the bands of colour displayed by the film when viewed by light incident at 45°, and noting the time at which each observation was taken. This operation was performed at least twice, and thus the rate of motion of the bands of colour was determined, from which data their positions and the form of the film could be calculated for the moments at which the electrical observations were made.

The result of these measurements was curious and will perhaps be best understood by means of the figure. The curves there given show the thicknesses of all parts of a particular film at the hours named. The upper parts represent the black portions of the films, the lower correspond to the coloured portions, and the sudden change in thickness between the two is well marked. The curves also illustrate very well the phenomena generally observed after the black began to form, namely, that the portion of the film in contact with it became thicker, though never thicker than any of the lower parts of the film, which, on the other hand, continued to become thinner, so that at last the whole assumed one uniform tint, which changed but slowly, and sometimes in such a manner as to indicate an increase in thickness. This latter phenomenon is due to absorption of moisture from the air, but it seems not unlikely that the thickening observed at the lower edge of the black may have been in part due to the fact that the sudden decrease in thickness caused by the formation of the black portion of the film must necessitate the rapid removal of a comparatively large quantity of liquid from its lower edge. The curves given in the figure, together with all the others drawn from the data acquired, prove that the coloured parts of the films observed did not increase regularly in thickness from top to bottom, but that rapid changes occurred somewhat similar to, though far less marked than, that observed at the lower edge of the black. Some films thicker than those from which the figure was drawn showed two such shoulders as those curves exhibit below the black, and it was remarked that a rapid change of slope in the surface of the film generally began at points where its thickness was such as to cause it to show the yellow of the second order when seen through the telescope of the cathetometer. The thicknesses of the films are necessarily represented on a much larger scale than their lengths, as the latter would cover many yards of paper if magnified to the same degree as the former. Hence the figures give only a very exaggerated representation of the forms of the films. For instance, in the case represented in the last figure, the difference in thickness between the black and the rest of the film, which was the yellow of the second order, was certainly not greater than 320-millionths of a millimetre. If, now, this increase in thickness took place in one-hundredth of a millimetre (a distance which would be inappreciable on the scale of the figures), the colours of the first order and those of the second inside the yellow would probably not have been visible if crowded together in so small a space, yet the transition from the black to the yellow would, in such a case, be made by an easy slope of 3 in 100, instead of by the abrupt change represented. We are thus, in all probability, dealing throughout with very small curvatures, and it is only the extraordinary sensitiveness of the tests of the thickness of a film at our command which enables us to detect the slight changes of slope which occur.



Having thus determined the shape of the coloured portion of a film it is easy to calculate its resistance, and by subtracting this calculated result from the observed total resistance of the film to deduce that of the black part alone, and thence to calculate the resistance of a ring of the black of the diameter of the crucible, and 1 mm. broad. If the number so obtained varies in different experiments, the inference is that either the thickness of the black portions of the films varies, or that the assumption that Ohm's law was applicable to the rest of the film was incorrect; if it is constant we draw the double conclusion that the thickness of a black film is constant, and that the coloured films obey Ohm's law. Such a method affords, however, only an unsatisfactory test of the latter of these points, as the resistance of the coloured portion of a film is in general so small a fraction of that of the black part, that a considerable apparent divergence from Ohm's law, and consequent error in its calculated value, would not produce much error in the resistance of the black film.

With regard to the first point, however, viz., the variations to which the thickness of a black film is subject, the experiments furnish some more trustworthy information. Five cylinders were observed on five different days, and in all thirty-six determinations of the resistances of black films were made. The breadth of the black ring varied on different occasions between 1·4 m.m. and 11·87 m.m., and the total resistances measured lay between 3·7 and 22 megohms. The highest and lowest values of the resistance of a ring of the black 1 mm. broad obtained from individual experiments, differ from one another by about eleven per cent. of the mean value, but the means of each day's results agree to within about five per cent., and the means of four out of the five days' results agree to within two-and-a-half per cent. We thus learn that the thickness of the black films formed in the same manner and with the same apparatus, varied very little on the different occasions on which they were observed. By treating the experiments in this way, we mingle together indiscriminately, all the observations obtained from any one cylinder, whatever the breadth of the black film may have been. If, however, it diminished in thickness from below to above, we should expect that observations taken when the black part of a film was large and therefore, on the average, thinner, would, on the whole, give higher values for the resistances than those taken when it was small.

Accordingly, the results were grouped according to the breadth of the black film, and it was found that the mean value of the resistance of a ring 1 mm. broad, deduced from experiments made when the breadth of the black was between

0 and 2	mm. was	1·761 megohms.
2 ,, 4	,,	1·764 ,,
4 ,, 6	,,	1·734 ,,
6 ,, 8	,,	1·760 ,,
8 ,, 10	,,	1·756 ,,
10 ,, 12	,,	1·760 ,,

These numbers prove that the thickness of the black ring is independent of its breadth.

It was, however, thought possible that the thickness of the black film might be affected by that of the coloured portion of the film which appeared to be in contact with it, and accordingly the results were once more grouped with reference to that colour only.

The mean value of the resistances when the colour next to the black was the

Blue of the second order, was	1·826
Green ,, ,, ...	1·748
Yellow ,, ,, ...	1·719
Orange ,, ,, ...	1·756
Red ,, ,, ...	1·716
Green of the third order ,, ...	1·738

The first of these numbers is considerably larger than the rest, but it is deduced in great part from a high set of measurements which were obtained during the observations on the first cylinder. Equally high values were, however, obtained later on the same day when the colour next to the black was the orange of the second order, and on the only other occasion on which an observation was taken with the blue of the second order in contact with the black, the number obtained was 1·760. Hence the high value, 1·826, does not seem to have any special significance, and we conclude that the thickness of the black is independent of the portion of the film which appears to the naked eye to be in immediate contact with it.

The principal error with which these experiments are probably affected is due to the fact that the lower boundary of the black part of a film was not always strictly horizontal. When this was noticed to be the case, the breadth of the black ring was measured in several parts, and a mean value was deduced, but it was difficult for the observer to determine whether or no the further edge of the black was below that nearer to him. In spite, however, of this and the other possible errors of experiment, the numbers obtained certainly prove that the thickness of a black film is uniform, and is very approximately constant under such variations of circumstance as those to which the films observed were exposed. One other point may be worthy of notice. The mean value of the resistance of a black ring 1 mm. broad deduced from all the experiments was 1,750,000 ohms, and by applying Ohm's law to this we obtain for the thickness of the black film twelve-millionths of a millimetre. This value, which must be received with the cautions given above, is only one-third of that at which a film of the solution used would begin to appear black, and would make the thickness of the film one-forty-ninth part of the wave-length of D.

As Prof. Reinold and the writer are at present engaged in investigating the question of the magnitude of the radius of molecular attraction, a consideration of the various speculations as to molecular magnitudes, to which these experiments lead must be for the present postponed.

A. W. RÜCKER

RAINFALL IN SOUTH INDIA

THE probability of another failure of rain and consequent famine in South India gives to any facts connected with rainfall in that country so great an interest that I will not delay longer the publication of a result obtained by me several months ago in a paper which would have been presented to the Royal Society last session, but for the desire to complete it with some details expected by me from India.

Whether the amount of rain follows the decennial law or not, all the known causes of variation will pass, it may be believed, through all their phases within ten years; so that the yearly mean rainfall deduced from ten years' observations should give a considerable approximation to the mean from any series however long. Any deviations from this result would be expected to be small and irregular. The fact is otherwise, if the observations at Trevandrum, on the west coast, and at Madras on the east coast, may be taken (as has been done) for approximate representations of the variations for the country around.

The Trevandrum series of observations includes the years 1838 to 1876. Taking the *sums* of rainfall for each ten years (1838–47, 1839-48, and so on), it appears that the amount was a maximum in the ten years 1843–52, and equal to 761 inches, from which time it has gradually diminished (with occasional slight increases) till now, 1867–76, when it is only 562 inches; or the *yearly* mean rainfall during the last ten years was nearly 20 inches less than a quarter of a century ago.

When we examine the Madras observations we find a quite similar result. The Madras series includes the period 1813 to 1876: the ten-yearly sum of rain was a maximum in 1818–27, and equal to 555 inches; it diminished to a minimum in 1828–37, and equal to 382 inches; increased, as at Trevandrum, to a maximum in 1843-52, and equal to 583 inches; diminishing thence to 396 inches in 1860-69. The agreement with Trevandrum is complete till this period; but the sum increased at Madras to 510 inches in 1865–74, diminished thence till now, whereas at Trevandrum the diminution has continued to the present time.

If we compare the diminutions at the two stations while they agreed, we find—

	Madras.	Trevandrum.
1843–52	583 inches.	761 inches.
1860–69	396 ,,	583 ,,
Diminution ...	187 ,,	178 ,,

Or at both stations a diminution of the yearly mean rainfall of about 18 inches. It will be of importance to ascertain whether this diminution has been experienced at more northern stations.

The yearly rainfall on the west coast of India, near Cape Comorin, was about 26 inches in 1844; it increased to about 70 inches at Trevandrum, thence to about 120 inches at Cananore, diminishing at Bombay and farther north, the maximum fall occurring somewhere between Cochin and Bombay. This variation does not depend wholly at least on the nearness of the stations to the range of the Ghats. It appears that the great atmospheric current sweeping over the Indian Ocean charged with vapour during the monsoons has a central current of maximum vapour depth or of velocity. If we can imagine that this central current shifts on the whole northwards from year to year for some time, the stations to the south will receive less and less rain, while those to the north will receive more; the total precipitation might thus remain the same. If we could suppose such a movement of the vapour masses to obey a law like that of the sunspots (and something resembling this has been found by me in the investigations for the isobaric lines and wind-currents in the British Isles), then we should find in such a case the same physical cause producing a maximum of rainfall at a northern station, at the same time as a minimum at a southern station. This is merely a suggestion to show the caution which may be necessary before we accept without qualification any fixed rule as to the similar actions of the same physical cause at Madras, Calcutta, and Bombay.

It may be asked to what extent the Trevandrum series of observations confirms the results given in NATURE (vol. xvi. p. 252) for Madras and Bombay, relatively to the ten-yearly period. I may point out that the Trevandrum series comprises only three and a half cycles, too few for any very satisfactory conclusion. It is easy, however, to inquire to what extent the Madras and Trevandrum series agree during the time we have observations from both (1838-1876). Should they give the same result for these thirty-eight years, whatever that result may be, we may fairly conclude as to the probability of Trevandrum giving generally the same result as Madras.

An examination of the yearly rainfalls at the two stations during this period shows in a very marked way a cycle of about five years, or a double oscillation in the decennial period; and this for seven successive oscillations. I have in consequence sought the equations of sines giving the most probable representation of the mean oscillations (single and double) for the ten and a half year period at the two stations from the thirty-eight years' observations; these are as follow, (y) being the yearly rainfall in inches :—

Madras ... $y = 5{\cdot}4 \sin (\theta + 50^\circ) + 4{\cdot}6 \sin (2\theta + 252^\circ)$,
Trevandrum $y = 5{\cdot}6 \sin (\theta - 17^\circ) + 8{\cdot}4 \sin (2\theta + 259^\circ)$.

While the series are too short for any certain conclusions yet the general agreement of the equations for the two places is quite distinct.[1]

What I desire especially to point out is the large *double* oscillation in the ten and a half year cycle; at Trevandrum (where there are *two* monsoons) the mean range amounts to 16·8 inches; it is only 9·2 inches at Madras; but the epochs of maximum and minimum are very nearly the same at both stations as shown by the angles 252° and 259°. Also these angles give the epochs of *minimum* rainfall both in the years of minimum and of maximum sun-spots.

It is not my intention to seek an explanation of this result, especially (though well shown seven times in succession) since it requires more extensive investigation, like that for ten and a half years, before any considerable weight can be given to it; I have thought it desirable, however, to bring this oscillation to the notice of investigators. The object of men of science is not to prove or disprove a case but to ascertain the truth. We are only groping at present in the darkness, yet it is not merely possible but probable that results of great practical importance may be derived from a conjoint study of terrestrial and solar physics, though they may not be the precise results which were expected or, it may be, desired. I shall endeavour at another time to give reasons for believing this.

Putting aside all that has hitherto been considered glorious to a nation in advancing science for its own sake; if we remember the vast human suffering that may be alleviated hereafter by encouraging now the study of solar actions as observed on the sun's surface, and as felt on our globe by the trembling magnet, the heaving air, and, it may be, the falling rain, there will I think be few, who know a tithe of what science has already done for humanity, who will not join in the demand for the small sum required from the nation's resources for so great an end.

JOHN ALLAN BROUN

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[1] The preceding equations cannot be compared with those for Madras and Bombay (NATURE, vol. xvi. p. 252), since here $\theta = 0$, for 1838·5, so that the term for the single oscillation does not agree with that found previously from the longer series at Madras.

NOTES

THE Lavoisier medal of the French Society for the Encouragement of Industry has been awarded for the present year to Mr. Walter Weldon, for the great progress realized by his manufacture of bleaching powder. This medal is seldom awarded—among the few recipients being Henri St. Claire Deville, Henri Giffard, Boussingault, Ferdinand de Lesseps, and Sir Charles Wheatstone. The presentation was made by M. Dumas, who delivered an eloquent address.

AT the next session of the French Association for the Advancement of Science, the committee appointed on meteorology will propose to memorialize the Cabinet to create a Meteorological Institute, in which will be centralized all the meteorological institutions of France, thus severing the connection between them and the Paris Observatory.

THE number of stations of the French Agricultural Meteorological Service was 1,149 on July 1. All the departments except five, the poorest and the least educated, are possessed of stations. The service was inaugurated last August. Not more than 3,600 French communes are said to have telegraphic stations, so that the third part of the communes in a position to enjoy daily telegrams have availed themselves of the opportunity in less than one year. The French Agricultural and Marine Meteorological Service has a very limited credit, not more than 30,000 francs.

IN the royal castle at Nürnberg the well-known Himalayan traveller Hermann v. Schlagintweit-Sakünlünski is exhibiting the large collection made by himself and his two brothers Adolf and Robert during their travels in India and High Asia.

THE proposal to found a popular Astronomical Institute at Hamburg, advocated by the optician Dr. Hugo Schröder, has been received with general favour. Indeed the project has so far exceeded the original proposal as to include a zoological garden, an aquarium, and, in fact, a great institute for the spread of scientific knowledge. Popular lectures will be given at the institute, which will be furnished with a refractor costing 188,000 marks, a dozen of the best microscopes, a model of the world, planetaria, &c. The first cost is estimated at 600,000 marks.

A PROPOSITION has been made by influential members of the Municipal Council of Paris to establish a regular course of lectures on astronomy at the Montsouris Observatory. There has been no public lectureships on astronomy at Paris since Arago died, except at the Sorbonne and Collége de France, but none of these lectures are for beginners.

Two living specimens of the Colorado beetle were found in Liverpool docks last week; one was taken on Wednesday on board the Spanish *S.S. Carolina*, which arrived about a week previously from New York, bringing sixty head of large cattle from Texas; and it was supposed to have come in the fodder for the cattle, which consisted of hay and maize. This was stored on deck above the after hatchway close to the saloon, and the beetle was taken crawling up the wall inside the saloon near the ceiling. The weather having been warm the windows had been open, and it could easily find admittance from the adjoining fodder, which was all but exhausted. Some delay took place in getting the ship into dock, and the cattle disembarked in consequence of a misunderstanding as to the regulations regarding the importation of cattle, so that about a week elapsed before the ship got into dock; and by the time she did get in fresh fodder had immediately to be got to supply the cattle. This relieves any anxiety which might otherwise have been felt as to other specimens having been distributed on shore with any surplus fodder. The history of the other specimen was not so clearly traced. It was said to have fallen from a steamer on to a sail of some craft, and to have been picked up by a bargeman, who gave it to a gentleman who took it for identification to Mr. Moore of the Free Library and Museum. But as far as can be learned it appears to have been taken, if not in the same dock at least in the same region of docks as the other one, so may have possibly also come from the same source. Credit is due to Mr. Moore for the promptitude with which he at once telegraphed to the Privy Council the occurrence of both of these specimens. It is only by such prompt co-operation that the Privy Council can expect success in their efforts to exclude this dreaded pest.

DETAILS are to hand of the volcanic eruption in Ecuador, on June 26, referred to in last week's NATURE. The eruption is supposed to have originated in Cotopaxi, and the enormous quantity of ashes ejected has spread desolation over a wide area. A calculation has been made of the quantity of ashes which had fallen in thirty hours, and it was estimated that on each square kilometer of space 313 kilogrammes of ashes had fallen. A mineralogical analysis of the ashes was made with the following result:—Volcanic ashes composed of exceedingly fine particles of lodestone, vitreous feldspar, hornblende, and an amorphous substance. The eruption was accompanied by an enormous and destructive flood of mud and water which swept down the rivers Cutuchi, San Felipe, and Yanayaco.

WE have received the Tenth Report of the Peabody Museum of American Archæology and Ethnology, at Harvard University From this we notice that the scope of the Annual Reports of the Museum has been enlarged, and that it is proposed to print special papers on American archæological and ethnological matters, so far as the funds will permit. The Museum authorities are anxious to receive exchanges for its reports, over 200 copies of which are now sent to foreign societies, journals, &c, of kindred character, but thus far very few returns have been received. We regret to see that of these few only two or three come from England. As the formation of a working library is one of the objects of the Museum, we would earnestly urge upon all workers in this department to lend their aid by sending copies of any papers they may publish, and upon all who receive copies of the valuable Museum Reports to make what return they can.

UNDER the conjoint auspices of the Smithsonian Institution and the Peabody Museum of Cambridge (U.S.) Mr. Clark Mills was recently sent to Florida for the purpose of securing casts of a number of Indians held as prisoners of war in the old Spanish fort at St. Augustine. We learn that Mr. Mills has been completely successful in this object, having secured excellent likenesses in plaster of sixty-two Indians, representing some ten or twelve different tribes. These, it is understood, will be used in the preparation of a series of dressed lay figures of Indians in the National Museum.

THE Nez Percés Indians, who are now giving a good deal of trouble to the United States Government, belong to the family of the Sahaptins, whose habitat is on both sides of the middle and lower Columbia River and its tributaries. Their reservation in North-western Idaho has given for several years past a census of 1,400, but a large number have refused to live on the reservation or conform to treaties made. Their name, it appears, is a misnomer, the habit of piercing the nose to receive a white shell, having existed only among two other and less important branches of the same family. According to the *Nation*, the Nez Percés, while fierce and haughty, are honest, just, and often charitable. Though their manner is often cold and reserved, they are eminently civil, and on occasion become social and even gay. Quick of temper and prone to resent any apparent fraud or injustice even more than a wanton injury, they are easily appeased, and in general more nearly approach the "noble savage" of romance than any of the aborigines known to the present generation of Americans. They are well and strongly built, of a dark copper colour. The compression of the forehead produced in infancy, almost entirely disappears in adults. The portion of the tribe settled along the Clearwater and Lapwai rivers and their tributaries, and the Kamia, has of late been fairly successful in self-maintenance by farming and stock raising, The "non-treaties" roam on the Clearwater and its branches, and on the Snake about its forks, the Wallowa valley, however, being their favourite pasture ground. They organize great hunting expeditions once or twice every year, and as the buffalo is not now found west of the Rocky Mountains, are forced to cross the Bitter Root range to the plains between the Yellowstone and Missouri rivers, and so come in collision with several tribes of their hereditary foes. Regular fishing encampments are also formed between June and September on their home rivers; and the salmon, with the jerked buffalo meat, and a number of esculent roots, are laid up for their long winters. Stock is their main pride and wealth, however, and the herds of the independent bands are of a highly patriarchal character. Individuals not unfrequently own from one to three thousand horses. The labour connected with these partly devolves on a class of slaves, who are prisoners of war, and their descendants. The Nez Percés have not only been passively peaceable and friendly ever since the Louisiana purchase of 1803, but they have been steadfast allies of the U. S. Government in troublous times; and the conduct of the General Government towards them appears to betray a want of common sense and honesty, to say nothing of gratitude for tried and valuable fidelity.

THE scientific press of Paris lost last week one of its most useful and respected members, M. Montucci, the scientific critic of *Galignani's Messenger*, who died after a short illness, at the age of seventy. Dr. Henry Montucci, a German by birth, had become a Frenchman by naturalisation. He wrote a number of books in the French language on scientific subjects, his last work being a "Theory of Progress." He has written a number of papers on mathematics, which have been printed in the *Comptes Rendus* of the French Academy of Sciences. He was sent in 1868 by the French Government to England and Scotland, to report on the public instruction in these two countries.

THE next eclipse of the moon is attracting the notice of physicists and aëronauts in France. It is proposed to make a balloon ascent during the night of August 23-24, in order to ascertain whether, independently of clouds, the eclipsed moon has a bloody tinge.

THE new Hôtel Dieu, Paris, is now in working order, and on Saturday, August 11, it was inaugurated by the President of the French Republic. It covers a space of 20,000 square yards, including gardens. Everything has been organised in splendid style, and the most improved methods have been adopted by the architect. The chemical laboratories, on an extended scale, have been organised by the exertions of Dr. Liouville, as we reported eighteen months ago in our "Notes," and they are now fitted up. Probably the principal feature of the new building is the ventilation. This is accomplished by means of two steam-engines of 40 horse-power each, which pump the air to an altitude of 130 feet from the ground. The air is circulated through the rooms of patients after having been filtered, and warmed or cooled, as is deemed advisable, before being admitted. The circulation is to be kept up at a rate of 100 cubic metres per hour per head. The air is filtered again and burned after being used, to destroy infectious germs, when it is sent out.

THE Mayor and Corporation of Leamington have invited the Sanitary Institute to hold its Congress in their town, and the invitation has been accepted. The Congress will meet early in October.

PROF. MARSH, in continuation of his investigation of the fossil remains of the Rocky Mountains, announces a new genus and species of toothed bird, which he calls *Baptornis advenus*, basing it upon a tarso-metatarsal bone. He also describes a new fossil lizard, by far exceeding in magnitude any land animal hitherto discovered, which must have been fully fifty to sixty feet in length. It was probably a herbivorous reptile. It comes from a bed on the eastern flank of the Rocky Mountains.

THE additions to the Zoological Society's Gardens include two Rufous Tinamous (*Rhynchotus rufescens*) from South America, presented by Capt. Fairfax, R.N.; a Spotted Cavy (*Coelogenys paca*) and a Coati (*Nasua nasica*) from South America, presented by Mr. J. Trotter; a Gannet (*Sula bassana*), British, presented by Mr. S. N. Sharpe; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Miss Potter.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

LONDON.—The Rev. Philip Magnus, in his address at University College School on the occasion of the distribution of prizes, spoke in high terms of the progress which that school has made since he himself was a pupil in it. He referred especially to the extension of science-teaching to the small boys of the school, the addition of a physical lecture-room and chemical laboratory, and to the fact that the school was one of the first to break down the old grammar-school system of teaching, and to see the importance of scientific instruction as well as instruction in English and other modern languages.

UNIVERSITY COLLEGE, BRISTOL.—The Council have selected as Principal of the College, Mr. Alfred Marshall, M.A., Fellow of St. John's College, Cambridge, who will also hold the Professorship of Political Economy. Several reappointments have been made in the College staff; the Lecturership in Mathematics and Applied Mechanics is still vacant. The second session will begin on October 9, and it is understood that there will be an inaugural meeting of some importance. The College Calendar will shortly be issued.

TÜBINGEN.—On the 10th instant the University of Tübingen celebrated, in true German style, the 400th anniversary of its foundation. Among the other honours conferred was that of Doctor in the Medical Faculty, upon Prof. Tyndall.

JENA.—The University of Jena has conferred upon Sir C. Wyville Thomson the degree of Doctor of Philosophy, "*Honoris causa*."

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 6.—M. Peligot in the chair.—Conditions for the principal normals of a curve to be principal normals of a second curve, by M. Serret.—New considerations on the localisation of cerebral centres regulating the co-ordinated movements of articulate and written language, by M. Bouilland. A case described in which a young man lost the power of writing, though there was no indication of a morbid state either in the hand or other parts of the right arm, and which would commonly be called one of writer's cramp, M. Bouilland attributes it to a like cause to that in aphasia, viz., lesion of a cerebral centre regulative of the movements involved. And if the co-ordinating centre for oral language is (as is thought) in the third circumvolution of the left anterior lobe of the brain, it is considered likely that either in this circumvolution or in the nearest part of another, resides the regulative power for the co-operating movements of written language.—Experimental researches made with gases produced by explosion of dynamite on various characters of meteorites and bolides (continued), by M. Daubrée. The angularity of the fragments in many cases indicates that they have been subject to strongly-heated and compressed gases only during a very short time, probably less than a second, having been separated near the end of the bolide's course. Small quantities of gas may give great effects, and M. Daubrée shows that the encounter of a bolide with air in the higher regions of the atmosphere is a fact of the same order with some of his experiments.—Tertiary strata of Vicentin (concluded), by MM. Hebert and Munier-Chalmas.—M. Thenard presented (from M. Videau, of Blanzy) a remarkable specimen of crystallised glass, obtained in a Siemens furnace, acting eight months and a half.—Spherical refraction; exposition of the laws and formulæ of Gauss, starting with the principle of the equivalence of physical forces, by M. Giraud Teulon.—On the formula $2^{2^n} + 1$, by M. Pepin.—Observations on a memoir of M. Haton de la Gonpilliere, entitled "Direct and Inverse Developpoids of Various Orders," by Abbé Aoust.—Observations of the planets 170, 171, and 172, at the observatory of Marseilles; discovery of the planet 173 by M. Borrelly, by M. Stephan.—Elements and ephemerides of the planet 148 Gallia, by M. Bossert.—Reply to some of the objections formulated by M. Cosson against the project of formation of a Saharan sea, by M. Roudaire.—Comparative influence of leafy woods and resinous woods on rain and the hygrometric state of the air, by M. Fautrat. If vapours dissolved in the air were apparent like fogs, we should find forests enveloped by a large moist screen, and for pine forests the envelope would be greater than for others. The forest, too, receives more rain than the neighbouring land, and the fact is more pronounced in the case of pine forests than in others. Pines retain in their branches more than half of the water poured on them, while leafy trees let 58 per cent. go to the ground.—On the catechines, by M. Gautier.—On the ovary during pregnancy, by M. De Linety.—On the quantity of hæmoglobin in red blood corpuscles, by M. Malassez. With a new colorimeter he estimated the quantity of hæmoglobin in a cubic millimetre of blood; then, dividing this by the number of corpuscles in the same volume, he arrived at the average quantity of hæmoglobin per corpuscles. In Parisians in the prime of life the number got was 27·7 to 31·9 $\mu\mu$ gr. (or millionths of a millionth of a gramme). In health the "richness in hæmoglobin" varies little in an individual; but in disease it is otherwise. In anæmic persons, the quantity varied between 10 and 25 $\mu\mu$ gr. Birds which have fewer corpuscles than mammalia, have more hæmoglobin in each. The same holds for fish, reptiles, and batrachians, but in these, the increase of hæmoglobin does not as in birds, compensate the diminution in number of corpuscles. Variations in volume (of corpuscles) may explain variations in richness in hæmoglobin among animal species little apart; but for those far apart one must take account of the presence of a nucleus, and suppose also modifications of the globular substance.—Experiments demonstrating that chloroform has not any action either on septicity or on the vibrionians of putrefied blood, by M. Feltz. Chloroform, then, cannot serve for separating in septic putrefied blood the diastasic from the organic ferments.—On a new larval form of Cestoides (second note), by M. Villot.—M. Perez made some observations on M. Fol's opinions regarding fecundation of the egg in the star-fish and sea-urchin.

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THURSDAY, AUGUST 23, 1877

MAURITIUS OBSERVATORY

Reports for 1874 *and* 1875 *of the Observatory, Mauritius* (*Appendices to Minutes of Council No.* 9 *of* 1875, *and No.* 24 *of* 1876). *Mauritius Meteorological Results*, 1874 *and* 1875.

THESE Reports give an interesting account of the progress of the Royal Alfred Observatory at Mauritius. The first division is devoted to the "Buildings, Grounds, and Water Supply;" this is a more important division than may be supposed at first sight. It appears in the Report for 1874, that in an attempt to arrange the grounds extensive pits were formed which were converted, through heavy rains, into noxious pools; while in next year's Report it is mentioned, under the head "Staff," that all had acted with "great zeal and perseverance notwithstanding occasional attacks of fever from which no one here has been exempt." The cause is indicated to be in a neighbouring marsh left undrained since 1872 or 1873.

In a similar report by the director of an observatory in South India to the Government, the construction of a road from the Observatory near the sea to another on the summit of the Ghats is mentioned as having been placed under his superintendence. The surveyor, however, who had to see the trace cut out, was unable to induce the coolies to accompany him through a certain part of the country, as a tiger had there carried away different members of a hill tribe. The director requested the Government to offer a suitable reward for the death of the man-eater, but in vain; the only reward promised being that usually given, the estimated value of the skin. The worth of the thing was what it would bring. A cooly, unlike a soldier, costs nothing to make him a useful machine; if a few coolies are snapped up by a hungry tiger others can always be had equally capable at the same daily rate of hire; the finances of the State do not suffer. If the director of the Observatory, the surveyor, or any other salaried officer, should be eaten, that would be a positive gain to the Treasury, since the pension due for previous services would be at once cancelled. No one can estimate the financial loss if a healthy officer should escape in such a case!

No Government, of course, would *reason* in this way, and we feel sure that of Mauritius will do what they can to make the scientific work they have so well begun as little dangerous as possible.

There remains, however, an annual source of discomfort and mischief in countries like the Mauritius where the rainfall is heavy—the "leaking" of the roofs. This is a more serious matter, financially speaking, than the health of the staff, since the whole objects of the observatory may be defeated by the action of humidity on the different instruments, including the object-glasses of the photo-heliograph, the equatorial, and other telescopes. In the Report for 1875 it is remarked that "the roof of the main building, which leaked considerably, has been repaired on two occasions. It still leaks, however, though not nearly as much as it did in February last, when some of the rooms were flooded, and books and papers were more or less damaged," &c. The roof of the Magnetic Observatory is also noted as "now almost water-tight." We hope the importance of this matter will be thoroughly appreciated by the Government, since the preservation of the instruments and the whole value of the results to be obtained from them depends upon it.

The magnetical instruments have been placed twelve feet under ground. We are afraid that this is not a good arrangement. Dr. Lamont tried a similar plan at Munich during five years, and the damp rotted the wooden supports of the roof, &c. He was at last obliged to place all the instruments above ground. We have also tried a similar method for a short period; but the humidity in such positions, and especially where the soil is frequently saturated with water, is destructive of all satisfactory results. The great object sought is constancy of temperature, and this can be gained to a great extent by placing the instruments within an *inner* room with thick ceiling and inner walls. Such a room need be entered rarely, and with self-registering apparatus the parts requiring manipulation may be placed in an external chamber communicating with the instrument room by a small opening in the wall.

Dr. Meldrum, the able and zealous director of the observatory, has both magnetical and meteorological self-registering apparatus, and in connection with the latter he receives meteorological observations from various stations. Astronomical work is limited chiefly to certain occasional phenomena (the transit of Venus was observed). There is a time-ball; a tide-gauge is expected; sun-spot pictures are taken with the photo-heliograph; sea observations from ship-logs are studied; storm-warnings are given; a magnetic survey has been begun, and special researches are undertaken.

Among the latter are useful practical studies connected with cyclones, which merit the greatest encouragement. Dr. Meldrum has noticed in his Report for 1875 the difference of his and M. Faye's views as to cyclonic movements. The latter insists that the wind moves in circles round the centre, while the former upholds spirally inward flowing currents (see NATURE, vol. xii. p. 458). This difference involves a most important question. According to the usual rule, as M. Faye says, the centre of the cyclone is at right angles to the direction of the wind; according to the other the wind is blowing towards the centre. That is an exaggeration of Dr. Meldrum's view; but in the Report for 1875 he says (Art. 90) that if a ship runs before the wind to north-west, believing the centre of the cyclone to be to north-east, the latter may really be to north or to north by west; that is to say, may make an angle of 45° or even of only 34° with the wind direction. We cannot accept Mr. Meldrum's theory of spiral cyclonic movements with the associated ascending currents, in which we have never seen any reason to believe; but we think there is considerable evidence for affirming that the angle made by the direction of the surface wind and that of the cyclone centre is generally less than 90°; and we do not think that M. Faye, in his effort to apply mechanical principles to the movements of the aërial masses, has had all the conditions of the problem before him, a fact which will appear more evident when the results we have obtained relatively to the movements of the atmosphere and the directions of the lines of equal barometric pressure are taken into consideration.

We have need, however, of more exact observations in cyclones at different distances from the centres, as we think it not improbable that the angle which the wind makes with the direction of the centre may vary with the distance from it as well as with the wind velocity.

Dr. Meldrum has also found periods for the frequency of cyclones and for the amount of rainfall agreeing with the decennial period of sunspots. It will be difficult, we think, to obtain quite satisfactory results for the cyclones, as the amount of evidence which will prove the existence of one will vary with the individual judging. A gale with a certain amount of veering or backing experienced by some ships may belong to a cyclone or it may not, there is no precise measure in many cases where there is not a sufficiently wide distribution of ships. No measure, also, is taken of the dimensions of the cylone or velocity of the wind, which it would be desirable to include in such an investigation. Some theorists insist that all winds are cyclonic. In any case we are inclined to believe that if such a decennial period exist it will be more accurately determined by measurements of the wind velocity for several years at fixed stations in different parts of the world. The question of such a period for the rainfall will, we have no doubt, receive ultimately a distinct answer from the observations at such stations, many series of which Dr. Meldrum has already collected and discussed with results in favour of the existence of such a period.

Meteorological results for 1874 and 1875 have also been published, and these include a number of important tables relating especially to the climatology of the Island.

JOHN ALLAN BROUN

OUR BOOK SHELF

A New London Flora; or, Handbook to the Botanical Localities of the Metropolitan Districts. By E. Ch. de Crespigny, M.D. (London: Hardwicke and Bogue, 1877.)

THERE are some local floras which have more than a local value, from the interspersion of critical notes on the species and sub-species by competent authorities. Of this character are Leighton's "Flora of Shropshire," and Bromfield's of the Isle of Wight. Others, of more modest pretensions, aim only at supplying information of interest to collectors or to those engaged in investigating the facts connected with the geographical distribution of plants; and these possess the advantage that their moderate size enables them to be used as pocket-companions. To this latter category belongs the little volume we have now before us, which strikes us as being a very good volume of its kind. The greater part is occupied by a list of species (alphabetical, so as to avoid the necessity of an index) of Phanerogams and Cryptogams, with the general distribution or special habitats attached. The nomenclature is that of the "London Catalogue of British Plants of 1874," unencumbered by any disquisitions as to specific or varietal distinctions, or the limits of natural orders. Of the 1,665 Phanerogams and Vascular Cryptogams included in the "London Catalogue," no fewer than 1,250 are found within the limits of the metropolitan flora. These limits, as understood in Dr. de Crespigny's volume, are, however, somewhat vague. They are stated to include an "average thirty-mile radius," but the radius appears to extend considerably further in some directions than in others. Thus, while we find a reference to the well-known localities for *Hymenophyllum tunbridgense* near Tunbridge Wells, and *Osmunda regalis* near Haslemere, there is none to that of *Anemone Pulsatilla* near Hitchen. These irregularities are, however, no doubt partly due to the direction of the author's individual researches, which seem to have been carried out with great zeal and accuracy, and to have extended over many years. The rest of the volume is occupied by a list of seventy-five localities, the scarcest and most interesting species of the locality being included in each list, distinguishing those which are authenticated by the author himself—by far the larger number. We can confidently recommend this volume to those interested in the flora of the metropolitan district.

Ethnography and Philology of the Hidatsa Indians. By Washington Matthews. (Washington: Government Printing-office, 1877).

THE United States Geological and Geographical Survey deserves the highest credit for publishing a work which pedantic red-tapeism might have thought did not belong to its province, and Mr. Matthews deserves equal credit for the care, thoroughness and scientific precision with which he has compiled it. We hope that so good an example will find many imitators. The Hidatsa (Hidacha), or Minnetari Indians, are a branch of the Dakota family, and now form one of the three tribes whose scanty relics inhabit the permanent village at Fort Berthold. The two other tribes are the Mandans and the Arickaris, and the linguistic relations of the community form one of the most interesting and important facts ever presented to the notice of the philologist. "This trio of savage clans," says Mr. Matthews, "although now living in the same village, and having been next-door neighbours to one another for more than a hundred years on terms of peace and intimacy, and to a great extent intermarried, speak, nevertheless, totally distinct languages, which show no perceptible inclination to coalesce. The Mandan and Hidatsa languages are somewhat alike, and probably of a very distant common origin; but no resemblance has yet been detected between either of these and the Arickaree. Almost every member of each tribe understands the languages of the other tribes, yet he speaks his own most fluently; so it is not an uncommon thing to hear a dialogue carried on in two languages, one person, for instance, questioning in Mandan, and the other answering back in Grosventre (Hidatsa), and *vice versâ*. Many of them understand the Dakota, and use it as a means of intercommunication, and all understand the sign-language." It should be added, as another curious philological fact that reduplication in verbs, which is a prominent feature of the Dakota, occurs in only one instance in the closely-allied Hidatsa. As in many other savage idioms, slight differences exist between the language of the women and of the men, the former tending to substitute *r* for *d*, and the latter preferring *l* and *n*. But the ethnologist as well as the philologist will find plenty of materials for study and reflection. Polygamy is practised, and a man usually marries his brother's widow, unless she object to the arrangement. Elopement sometimes takes place, divorce very rarely. "As with other western tribes, it is improper for a man to hold a direct conversation with his mother-in-law; but this custom seems to be falling into disuse." Males sometimes have four names, all containing the same noun, but a different adjective, and the names are afterwards solemnly changed once or even oftener. Coloured beads and pendants are made of pounded glass procured from the Europeans; the process of making them is very elaborate, and the antiquity of the art may be gathered from the fact that triangular pendants were used, "not as ornaments only, but as evidences of betrothal, as long ago as the oldest men can remember." Morally, the Hidatsa seem among the best of the Indians; they are described as industrious, honest, and peaceable, with fine physiques, light complexions, and great powers of endurance.

Across Central America. By J. W. Boddam Whetham. (London : Hurst and Blackett, 1877.)

THIS is a thoroughly readable and exceedingly instructive narrative, by a capable observer, of a journey through a country not often visited by travellers, and of which English readers probably know little or nothing. Mr. Whetham gives an interesting account of some of the wonderful ruins which exist in Central America, and we can commend his work to our readers as possessing both novelty and interest.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Contractile Filaments of the Teasel

THE observations of my son Francis on the contractile filaments protruded from the glands of Dipsacus,[1] offer so new and remarkable a fact in the physiology of plants, that any confirmation of them is valuable. I hope therefore that you will publish the appended letter from Prof. Cohn, of Breslau, whom every one will allow to be one of the highest authorities in Europe on such a subject. Prof. Cohn's remarks were not intended for publication, but he has kindly allowed me to lay them before your readers.

Extract from Prof. Cohn's Letter :—

"Immediately after the receipt of your very kind letter of July 26 I went to fetch Dipsacus, several species of which grow in our Botanic Garden; and proceeding after your recommendations, I put transverse sections of the cup-like bases of young leaves, or the epidermis of these parts carefully removed from the green parenchyma, into distilled water. I thus had the pleasure of witnessing with my own eyes this most curious discovery. First I ascertained the anatomical structure of the pear-like glands which are rather elegant and remarkable. From the basal cell rises the stalk-cell, in the second story there are two cells, in the third four, and in the uppermost series eight cuneiform cells converging to the centre. But you may conceive how much I was surprised by seeing the filiform protuberances issuing from the apex of the glands; it was quite a perplexing spectacle. The filaments are, in their refrangibility, very like the pseudopodia of some Rhizopods (*e.g.*, Arcella or Difflugia). I followed their changes for some time, and remarked quite definitely, as I find described in the paper of Mr. Francis Darwin how the protuberances slowly lengthen out, crook themselves hooklike or winding, and get knobbed either at the summit or midway; I saw the knobs or beads glide down the thread, and at last be sucked into a globular mass adhering to the gland. I saw the protuberances always rise between the septa of two or more adjoining cells, but nearly as frequently between the *lateral* septa as on the apical centre. Generally there were many protuberances on the same gland, pressed forward out of different spots; sometimes I saw two diverging branches proceed from the same point like a pair of compasses, each behaving independently in its changes. But the most curious appearance in these protuberances was a constant waving undulation along their extension, sometimes slower and perceptible with difficulty, sometimes vigorous and quicker, but never ceasing; more delicate filaments appeared to me very like Vibrio, or the vibratory flagella of some Infusoria. Not finding a special description of the waving movements of the filaments in your son's paper, I asked some of my pupils if they saw anything remarkable in the filaments, without indicating what, but they all took the same impression as myself. The only facts I have not yet been able to witness of your son's discoveries are Figs. 6, 14, 15, and the moniliform contraction; nor have I yet found time to apply chemical reagents, of which your son has made such good use.

"Of course I am not able, after two days' inspection, to form a definite judgment about the true nature of the filiform protuberances. Putting aside the hypothesis of a parasitic Rhizopod, there are two probabilities which still balance in my mind, as clearly stated by your son. (1) The protuberances are secretions of some colloidal matter, absorbing water, but insoluble in it; the movements are physical (not vital ones), the elongation of the filaments depending upon the imbibition, their contraction on the withdrawal of water by different reagents. There are such substances, *e.g.*, *myeline*, which shows rather similar changes in water. Please also to repeat the experiments I performed at the meeting of the British Association last year. Into a cylindrical glass containing soluble silicate of alkali (Wasserglas), diluted with half its amount of water, put a small piece of crystallised chloride of iron; from the fragment there rises a hollow reddish tube growing upwards and moving very quickly, like an Enteromorpha. But if you put into the diluted silicate some *protochloride* of iron (the latter is usually in the form of a powder, but may easily be brought by gentle pressure of the fingers into crumb-like masses), then from the lumps there arise innumerable filaments, very delicate and transparent, very like the glass threads of Hyalonema, which rise in fascicules vertically till they reach the surface of the fluid.

"But I cannot deny that the general impression produced by Dipsacus does not contradict the hypothesis that the changes of the filaments are the vital phenomena of protoplasmic pseudopodia.

"A French biologist (whose name I cannot just now remember) has proved many years ago (I think in an early number of the *Bull. de la Soc. Bot. de France*) that the water in the cups of Dipsacus is not a simple collection of rain in a gutter, but a secretion of the leaf bases. If this be truly the case, it is quite probable that the glands may have a special adaptation for this purpose. Indeed, I should not hesitate to agree with the vital theory, if there were any analogy known in plants. But further study of the phenomenon and the repetition of the chemical reactions which your son has already indicated, will, I hope, in a short time enable me to form a more decided judgment in this perplexing dilemma.

"In the meantime I am happy to congratulate Mr. Francis Darwin and yourself on account of the extraordinary discovery he has made, and the truly scientific paper in which he has elaborated it, and which has added a series of quite unexpected facts to the physiology of plants."

In a subsequent letter, Prof. Cohn describes what appear to him as thinned points or pores in the cell wall of the glands from which the filaments seem to be protruded. He also mentions the very curious fact which he has discovered, that by adding iodine to the detached epidermis of the leaf cups of Dipsacus the whole fluid contents of the epidermis cells turn blue like diluted starch paste, although no starch grains are met with in any epidermis cell except in the stomata.[1] He adds that the basal cell of the gland becomes blue, while the rest of it and the excreted globules are stained yellow.

I may add that I have heard from Prof. Hoffmann, of Giessen, that he formerly observed contractile filament of a somewhat similar nature on the annulus of *Agaricus muscarius*. He has described them in the *Botanische Zeitung*, 1853, and figured them, *ibid.*, 1859, tab. xi. Fig. 17.

CHARLES DARWIN

Down, Beckenham, August 15

Relations between Sun and Earth

PROF. BALFOUR STEWART in the last of his exceedingly interesting articles in NATURE (vol. xvi. p. 45) on the suspected relations between the sun and the earth, winds up with an appeal (which I should like to see promptly responded to by the Government here as well as at home) in favour of the establishment of some institution to keep a daily watch upon the luminary that is found to exercise such a marvellous control over terrestrial magnetism and meteorology. He also mentions incidentally the discovery by Dr. Hunter that the famines in Southern India have a period of recurrence which is nearly the same as that of sun-spot frequency. This is no doubt an exceedingly plausible hypothesis inasmuch as five out of the six years of drought mentioned by Dr. Hunter as preceding the years of famine during the present century fall within the group of minimum sun-spot years, the sixth (1854) being also a year of relatively few sun-spots (19·2 according to Wolf).

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[1] Abstract published in *Proc.* Roy. Soc., 1877, No. 179; published in full in *Quarterly Journal of Microscopical Science*, July, 1877.

[1] Prof. Cohn adds that the blue coloration of the epidermis by iodine occurs in the leaves of Ornithogalum.

Dr. Hunter's avowed object, however, in writing his pamphlet was to prove that a cycle of drought sufficient to cause famine existed throughout the *whole* of Southern India, and with this end in view he has been content to show that a cycle of rainfall corresponding with the period of solar maculation existed merely for one single station, viz., Madras.

Having found a decided correspondence between the rainfall of Madras and the eleven-year period of sun-spots, he thence argues somewhat hastily that the same conditions apply throughout the whole of Southern India. This hasty generalisation from the results of one station situated in a vast continent, the rainfall of which varies completely both in amount and the season in which it falls, according to locality, has been strongly contested by Mr. Blanford, the Government meteorologist, who on making a careful comparison of the rainfalls of seven stations, three of which—Madras, Bangalore, and Mysore—are in Southern India, the others being Bombay, Nagpore, Jubbulpore, and Calcutta, finds that with the exception of Nagpore in Central India, which shows some slight approach to the same cyclical variation which is so distinctly marked in the Madras registers, the rest of the stations form complete exceptions to the rule adduced for Madras, in many of them the hypothetical order of relation being reversed. Mr. Blanford, however, shows that underlying the above irregularities a certain cyclical variation exists on the average at all the stations, the amount nevertheless being so insignificant (not more than 9 per cent. of the total falls) that it could not possibly be considered of sufficient magnitude to become a direct factor in the production of famine. It thus appears that the cycle of rainfall which is considered to be the most important element in causing periodic famines, has only been proved satisfactorily for the town of Madras. It may perhaps hold for the Carnatic and Northern Siccars—the country immediately surrounding Madras, though, owing perhaps to the want of rainfall registers in these districts, evidence with regard to this point is still wanting.

Though Dr. Hunter has thus been only partially successful, I would not attempt to detract in any way from the value of his able pamphlet, so far as it goes, an indirect effect of which has been to stimulate meteorological inquiry and research in the same direction throughout India. The meteorology of this country, from its peculiar and tropical position, is in such complete unison with any changes that may arise from oscillations in the amount of solar radiation and their effects upon the velocity and direction of the vapour-bearing winds, that a careful study of it cannot fail to discover meteorological periodicities in close connection with corresponding periods of solar disturbance. In connection with the previous remarks, and as showing what a close connection exists between solar and terrestrial meteorology, I may observe that Mr. Hill, the meteorologist for the North-West Provinces, and myself, have coincidently discovered the existence of a remarkable cycle in the *winter* rainfall of Northern India, between the latitudes of 20° and 30°, corresponding *inversely* with the period of solar spots, *i.e.*, the maximum winter rainfall coincides with the minimum period of sun-spots, and *vice versâ*.

As a failure of the winter rains in the Northern Provinces in 1860-61 (*years of maximum sun-spot*) has been the cause of a severe famine, this theory, if completely established, would not be without its value in the economical administration of the North-West Provinces and the Punjab. I have not at present examined the rainfalls of all the stations in the Upper Provinces, but Mr. Hill, having readier access to them than myself, has probably done so to a larger extent, and tells me that the results of his investigations are similar to my own in bearing out the preceding hypothesis. A theory is not wanting to account for this tendency to vary inversely with the sun-spots, if we, according to opinion held by Drs. Hahn and Köppen, Prof. Piazzi Smyth, and Mr. Pogson, the Government astronomer at Madras, assume that the sun's heat is greater in years of minimum sun-spot. For in these years the anti-trade current, the descent of which upon the Himalaya and Northern India in the winter is generally understood to be the vehicle of the rain at that season, would be owing to the increased evaporation over the Southern Indian Ocean, reinforced with a larger supply of vapour than usual, while in years of maximum sun-spot the supply would be smaller. At all events, whatever be the real cause, the facts as far as we have gone, are exceedingly favourable to the existence of such a cycle. Calcutta, though lying close to the tropics, and therefore coming in for a small share of winter rainfall, still shows the preceding relation to a wonderful extent, and as its register of rainfall extends farther back than most of the other North Indian rainfalls, furnishes a more trustworthy result than many other stations whose rainfalls registered only for short periods scarcely afford more than a slight balance of probability in favour of the assumption. The following table is arranged in a double series of years occupying the same position in the spot-cycle, and gives the average rainfall for each double series for the months of November, December, January, February, March, and April, from 1837 to 1876 inclusive. I have indicated the groups containing the years of maximum and minimum sun-spot. The maximum rainfall will be seen to occur in the latter, and the minimum in the former group.

Calcutta Rainfall during the months of November, December, January, February, March, and April.

	Years.						Average rainfall of group in inches.
11th	1876	1865	1854	1843		Group containing years of minimum sun-spot.	8·49
1st	1877	1866	1855	1844			6·44
2nd		1867	1856	1845			
3rd		1868	1857	1846		... ...	5·93
4th		1869	1858	1847			
5th		1870	1859	1848	1837	Group containing years of maximum sun-spot.	4·44
6th		1871	1860	1849	1838		
7th		1872	1861	1850	1839	... ...	5·03
8th		1873	1862	1851	1840		
9th		1874	1863	1852	1841	... ...	6·15
10th		1875	1864	1853	1842		
Eleventh series repeated ...	...	..	...	...	...		8·49

Further analysis only tends to render the connection still more evident, but I have no time to add anything further. In conclusion I need only remark that Jerusalem, which is situated somewhere about the same latitude as Lahore, and receives its total annual supply during the winter months alone, fully bears out the hypothesis as far as records show from 1846 to 1859.

Bankipore, Patna E. D. ARCHIBALD

Reproduction by Conjugation

IN Prof. Allen Thomson's Inaugural Address to the British Association, I find the following sentence, referring to the simplest form of sexual reproduction among cryptogams, known as conjugation:—"In more ordinary cases, as in *Spirogyra*, where the embryo is formed in one of the two cells, it seems to be indifferent in which of them it is formed." If my own experience may be taken as trustworthy and adequate, there is one fact in connection with this phenomenon which would seem to show that it may not be altogether indifferent, and that the differentiation of male and female elements may be carried back even one step further than is stated by this distinguished biologist. When two filaments—which we may call A and B—are conjugating, then, as far as my observation has gone, the direction of conjugation is uniformly the same, *i.e.*, either the contents of every cell in A pass over into the adjacent cell of B, or the reverse; we never find the contents of some of the cells of A passing over into B, and the contents of some of the cells of B passing over into A. If this is so, and if we call the filament in which the zygospores are ultimately produced A, then it is clear that we may fairly call A the female and B the male filament; and it would appear certain that there must be some hitherto undetected difference between them. My own observations in this respect relate almost exclusively to *Spirogyra*, and I shall be very glad to know if they are confirmed, or otherwise, by those of more experienced algologists.

ALFRED W. BENNETT

The Greenland Foehn

HOFFMEYER'S facts respecting spells of warm weather in the Arctic winter, as reported in NATURE, vol. xvi. p. 294, are very interesting, but his explanation of them seems demonstrably insufficient. He thinks they are a phenomenon of the same kind with the *Foehn* of the Alps, which latter he explains by saying that a wind which at its origin is saturated with moisture will, when it is forced over a mountain chain, be raised 1° Cent. for every 200 metres of height of the mountains. The heat thus gained is liberated in the condensation of the vapour. I believe this is satisfactory as regards the *Foehn*.

But this will not account for a rise of the temperature of Southern Greenland from its mean December temperature, which, according to Dové's map, is below freezing, to 14° C. A rise of 14° C. would require, according to the above law, a mountain chain 2,800 metres, or about 8,000 feet in height, and there is none in Greenland approaching this.

I used to think that great rises of temperature in the Arctic winter were due to the wind tearing up the frozen surface of the sea, and liberating the heat of the water below; but this will not account for an increase of temperature above freezing. I have no explanation to offer. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, August 13

Does Sunshine Extinguish Fire?

IT is a popular belief that a fire will not burn if exposed to the sun, and, from all I have observed, it seems well founded. Can any of your readers favour me with an explanation of the phenomenon, if true; or is it a mere superstition?

Schwarzwald, August 11 CHARLES WATSON

OUR ASTRONOMICAL COLUMN

THE OPPOSITION OF MARS, 1877.—The present opposition of the planet Mars offers conditions so nearly analogous to those of the opposition in 1862, that the many fine drawings made in that year, a number of which are contained in vol. xxxii. of the "Memoirs" of the Royal Astronomical Society, become available for comparison with such as may be made during the actual opposition. The same hemisphere of the planet is presented to the earth, and our depression from the martial equator is sensibly the same; thus, at opposition in 1862 the angle of position of the visible (southern) pole was 145°·3 and the earth's depression −22°·7, while at the opposition of 1877 the figures are respectively 160°·3 and −22° 5. The least distance of Mars from the earth in 1862 was 0·406, while in 1877 it is 0·377.

Notwithstanding Secchi observing in 1858 found the features upon the disc of Mars irreconcilable with those delineated in Mädler's drawings made under similar circumstances in 1830, it was sufficiently evident at the opposition of 1862 that these differences are to be attributed to the temporary conditions occasioned by clouds of varying density, form, and extent, in the atmosphere of the planet itself, heightened perhaps in some degree by the state of our own atmosphere at the times of the observations. A striking instance in support of this conclusion was afforded by Mr. Lockyer's observations on September 25, 1862. At 10h. 44m., when his drawing No. 14 was completed, the well-known spot a of Mädler was *quite invisible*, while when No. 15 was made shortly afterwards, this spot was "among the most prominent features upon the planet's disc."

There would appear now to be little doubt that the green and red portions of the disc do really represent seas and continents, and are not due to the effect of contrast, another explanation which has been suggested. During the actual favourable appearance of the planet, we may expect that measures will be made which will admit of a closer determination of the position of the axis of rotation than any yet obtained. The results at present upon record are (1) Sir W. Herschel's, which assigns for the longitude of the ascending node of the equator of Mars upon his orbit, 79° 27′ for 1872, and for the obliquity of his ecliptic 28° 42′. The reduction by Oudemann's of Herschel's measures, make these figures 79° 18′ and 20° 53′ respectively; (2) Schroeter's, as given by M. Terby, which places the south pole in 172° 54′·7, with latitude 60° 33′·2, whence we find for 1798, longitude of ascending node of equator on orbit, 84° 54′, obliquity of ecliptic 27° 57′ for 1798; and (3) Oudemann's reduction of Bessel's measures with the Königsberg heliometer, made September 28, 1830, January 21, 1835, and February 11, 1837, giving for the place of the ascending node 80° 50′, and for the obliquity 27° 17′ for 1834. With the last values which have been generally adopted, we have for the ascending node of the equator of Mars upon the earth's equator (N), and its inclination thereto (I):—

$$N = 47^\circ\ 42' + 0^\circ{\cdot}50\ (t - 1850)$$
$$I = 39\ 52 - 0{\cdot}25\ (t - 1850).$$

The following table showing the angle of position of the visible pole of Mars, and the elevation of the earth above the plane of his equator, at the oppositions between 1850 and 1880, has been calculated from the above elements, and may be of interest to some readers; the least distance of Mars from the earth is added:—

Date of opposition.	Position of visible pole.	Elevation of earth.	Least distance of Mars.
	° ′	° ′	
1852, Jan. 24	23 16	9 8 N.	0·660
1854, Feb. 26	343 46	22 6 N.	0·675
1856, April 2	35 2	23 30 N.	0·625
1858, May 15	40 45	12 4 N.	0·514
1860, July 17	192 34	10 47 S.	0·391
1862, Oct. 5	145 17	22 42 S.	0·406
1864, Nov. 30	142 37	6 29 S.	0·534
1867, Jan. 10	343 54	10 24 N.	0·636
1869, Feb. 13	7 37	21 40 N.	0·677
1871, March 19	28 35	24 53 N.	0·636
1873, Feb. 13	41 2	17 59 N.	0·563
1875, June 19	209 7	0 51 S.	0·433
1877, Sept. 5	160 14	22 31 S.	0·377
1879, Nov. 12	138 48	13 54 S.	0·482

A glance at this table exhibits a well-known condition that when Mars is nearest to the earth and when we have consequently the best opportunities of studying the features upon his disc, his southern hemisphere is always directed to the earth, and hence we are likely to be better acquainted with that hemisphere than with the northern one, which is turned towards the earth only at the greater distances of Mars.

THE SATELLITES OF SATURN.—A series of observations of all the eight satellites of Saturn by Prof. Asaph Hall, dated from Washington in December, 1876, has at last made its appearance in No. 2,145 of the *Astronomische Nachrichten*.

SATELLITES OF MARS.—A telegram from the Smithsonian Institution to M. Leverrier, received August 19, notifies the extraordinary discovery of two satellites of Mars by Prof. Asaph Hall, of the U.S. Naval Observatory at Washington. The telegram runs thus: "Two satellites of Mars by Hall at Washington, first elongation west, August 18, eleven hours, Washington distance, eighty seconds, period thirty hours, distance of second, fifty seconds."

THE BRITISH ASSOCIATION

PLYMOUTH, *Tuesday*

THE Plymouth Meeting of the British Association has not realised, as far as numbers are concerned, the success which we anticipated last week, and which was indicated by the business done on the days immediately preceding the opening of the meeting.

The attendance of the regular members from a distance has been very good indeed, and can compare favourably with meetings that have gone before it, but the visit of the British Association, while opening wide the gates of hospitality of the people of Plymouth, does not seem to have awakened the scientific interest of the community sufficiently to cause many to enlist in its ranks. It is seldom that so small a number of local members have been added to the list of members and associates at the annual meeting of the British Association. Yet Plymouth and its surroundings have a well-known scientific reputation, and the falling off in numbers of the present meeting must rather be looked for to the fact that one of the two sister-towns to Plymouth has, as a body, held aloof from the Association, than to any want of scientific interest pervading the great port of the west of England.

At the opening meeting on the evening of Wednesday last, there was a fair attendance, but the noble Guildhall was not filled. It is estimated that about 1,300 persons were present, including the members of the General Committee, who, as usual, were accommodated with seats on the platform. The business commenced by the Mayor of Plymouth introducing to the meeting the President, Prof. Allen Thomson. It was, we believe, intended as a graceful compliment to its host, the town of Plymouth, that the British Association requested the mayor to take the place of the retiring president, Prof. Andrews, who is absent through illness. The meeting went off very well and the presidential address was, notwithstanding its difficult and abstruse nature, listened to with profound attention, and commanded the respect which must be paid to anything coming from so high an authority upon embryology as Prof. Allen Thomson.

On the next morning the sectional addresses were delivered, some of which appeared in our last issue and others we give in the present number. In Section A the address by Prof. Carey Foster, F.R.S., was warmly received and elicited great applause when he spoke of the radiometer and of the great value of the researches of Mr. Crookes, notwithstanding the recent utterances of an influential mover in scientific circles which might have a tendency to depreciate the value of those researches. Prof. Haughton, of Dublin, read two papers, the first upon a method of calculating the absolute duration of geological periods, and the second, a reply to Prof. Newcomb upon the co-efficient of acceleration of the moon's mean motion as illustrated by the original account of the solar eclipse of Agathocles. Upon both papers an interesting discussion took place.

At the same section Prof. Osborne Reynolds read a paper (which we publish to-day) upon the rate of progression of groups of waves and the rate at which energy is transmitted by waves. The paper was illustrated by a very beautiful model in which the progression of wave-groups was made visible to the audience by means of a long series of light pendulums connected elastically with one another.

Friday was a great day in Section A as far as attendance was concerned. It had been announced the day before that Mr. Preece would read a paper on the telephone and show it at work, and that Sir William Thomson would make a communication to the section on the possibility of life on a meteoric stone falling on the earth. In anticipation of these two papers the room of Section A was crowded from an early hour, and although there were several long mathematical papers on the list, the non-mathematical visitors waited in the most patient manner for the papers they had come to hear. It so happened, however, that the papers that appeared so dry to those waiting for something else were of the very highest interest and value to mathematicians and astronomers; especially that by Prof. J. C. Adams on his discovery of original papers by Newton which proved that that great philosopher had solved some of the most important lunar problems, the solution of which has been till now attributed to a much later date, and proving most conclusively that Newton had never fallen into the error which for years had been attributed to him.

Sir Wm. Thomson's paper on the possibility of a meteorite becoming the vehicle of animal life to this earth from another planet, or heavenly body, was evidently listened to with much enjoyment. Sir William sees no difficulty in the assumption that animals or germs might without injury be conveyed to this earth by meteorites if protected in the crevices of the meteoric mass, and much amusement was caused by his saying that though the outside shell of a meteoric stone might be incandescent from the friction caused by its flight through the terrestrial atmosphere, yet within a crevice of that stone might be concealed a Colorado beetle, which, falling on the earth, might become the father of a large and prosperous family. The amusement caused by this quaint idea was increased to roars of laughter when Prof. Haughton, with his well-known wit, ridiculed the idea of the transmission of living animals by meteorites, and said that if Sir William Thomson had spoken of a Colorado beetle arriving by a meteoric stone becoming the mother of a large number of baby Colorado beetles he might have felt some sort of alarm, but he didn't care how many papa beetles came, so long as they left the mamma Colorado beetles at home.

Mr. Preece followed with his paper on the telephone. The room was crowded to excess and the paper was of the highest possible interest, and not only illustrated by diagrams and the instruments themselves, but the latter were connected by wires with the post-office at Plymouth, about a quarter of a mile off and with that at Exeter some fifty miles away. By means of Bell's articulating telephone the human voice was distinctly conveyed and conversations were carried on between the two stations. Owing to induction from the parallel wires between Plymouth and Exeter, there was a confused roar as from hail pattering on a window pane, and no words could be heard, but in his lecture in the Guildhall on the following evening, the traffic was stopped for ten minutes, and a conversation was carried on by the human voice between Plymouth and Exeter. The Guildhall on the occasion of Mr. Preece's lecture, was crammed almost to suffocation, and the discourse lasted two hours and a half; the lecture was upon telegraphy, but the telephone was undoubtedly the chief attraction.

The instrument was again described in Section G this afternoon by the inventor, Dr. Graham Bell, who arrived from Liverpool yesterday; he was received with enthusiastic applause, and a most interesting series of experiments were shown in illustration of his paper.

At the General Committee Meeting yesterday, a letter from the city of York was read by the secretary, inviting the British Association to celebrate their jubilee or fiftieth anniversary in that city in consideration of the fact that the first meeting of the Association was held at York in 1831. The invitation was unanimously accepted.

After some quiet discussion the resolution that the Association should visit Nottingham in 1879, and Swansea in 1880, was carried unanimously; whereupon Prof. Haughton rose, and, amidst great laughter, expressed his regret at the proceedings terminating so peacefully, for, as an Irishman, he never liked to see a good fight stopped. Mr. Spottiswoode is to be president at the Dublin meeting.

Some wholesome resolutions have been approved of by the Council on the regulations as to the admission of papers to be read in the various sections. With regard to the discontinuance of Section F the Council ask the General Committee to report more fully on the reasons which have induced them to recommend this step.

PLYMOUTH, *Wednesday*

[*By Telegraph*].

The following grants were passed at the meeting of the General Committee held at the Royal Hotel to-day. The names of the members who would be entitled to call on the general treasurer for the respective grants are prefixed :—

Mathematics, and Physics.	£
Cayley, Prof.—Continuation ot Borckhardt's Tables ...	100
Foster, Prof. Carey.—Observation of Atmospheric Electricity at Madeira	15

	£
Glaisher, Mr. J.—Luminous Meteors	10
Joule, Dr.—Determination of the Mechanical Equivalent of Heat (renewed)	65
Thomson, Sir W.—Measurement of the Lunar Disturbance of Gravity (renewed)	50
Chemistry.	
Brown, Prof. Crum.—Quantitative Estimation of Atmospheric Ozone	10
Roberts, Mr. Chandler.—Chemical Composition and Structure of some of the less-known Alkaloids	25
Geology.	
Evans, Mr. J.—Kent's Cavern Exploration	50
Evans, Mr. J.—Record of the Progress of Geology	100
Godwin Austen, Mr.—Kentish Boring Exploration	100
Harkness, Prof.—North-West Highlands Fossils	10
Haughton, Rev. Dr.—Fermanagh Caves Exploration	30
Herschel, Prof. A.—Thermal Conductivity of Rocks	10
Hull, Prof.—Circulation of Underground Waters	15
Lubbock, Sir J., Bart.—Victoria Cave, Settle, Exploration	100
Biology.	
Dew Smith, Mr.—Table at the Zoological Station, Naples	75
Fox, Col. Lane.—Exploration of Ancient Earthworks	25
McKendrick, Dr.—Investigation of Pulse Phenomena by Thomson's Siphon Recorder	10
Rolleston, Prof.—Examination of two Caves and Tumuli near Tenby	25
Stainton, Mr.—Record of Zoological Literature	100
Thomson, Dr. Allen.—Transmission of Electrical Impulses through Nerve Structure	30
Statistics and Economic Science.	
Farr, Dr.—Anthropometric Committee (renewed)	66
Mechanics.	
Froude, Mr. W.—Instruments for Measuring the Speed of Ships (renewed)	50
Thomson, Sir W.—Datum Level of the Ordnance Survey	10
	£1,081

SECTION A.—MATHEMATICAL AND PHYSICAL.

On the Rate of Progression of Groups of Waves and the Rate at which Energy is Transmitted by Waves, by Prof. Osborne Reynolds, F.R.S.—When several waves forming a discontinuous group travel over the surface of deep water, the rate of progression of the group is always much less than the rate at which the individual waves which compose the group are propagated. As the waves approach the front of the group they gradually dwindle down and die out, while fresh waves are continually arising in the rear of the others. This, which is a well-known phenomenon, presents itself to our notice in various ways.

When a stone is thrown on to the surface of a pond, the series of rings which it causes gradually expands so as finally to embrace the entire surface of the water; but if careful notice be taken it is seen that the waves travel outwards at a considerably greater rate than that at which the disturbance spreads.

Or, when viewing a rough sea, if we endeavour to follow with the eye any wave which is larger than its neighbours, we find, after following it in its course for a short distance, that it has lost its extra size, while on looking back we see that this has been acquired by the succeeding wave.

But perhaps the most striking manifestation of the phenomenon is in the waves which spring from the bows of a rapid boat, and attend it on its course. A wave from either bow extends backwards in a slanting direction for some distance and then disappears; but immediately behind it has come into existence another wave parallel to the first, beyond which it extends for some distance when it also dies out, but not before it is followed by a third which extends still farther, and so on, each wave overlapping the others rather more than its predecessor. Although not obvious, very little consideration serves to show that the stepped form of these columns of waves is a result of the continual dying out of the waves in front of the group, and the formation of fresh waves behind. For as each wave cuts slantwise through the column formed by the group, one end is on the advancing side or front of the group, and this is continually dying while the other is in the rear and is always growing.

So far as I am aware, no general explanation of these phenomena has as yet been given It has been shown, and I believe first by Prof. Stokes, that if two series of parallel waves of equal magnitude, but differing slightly in length, move simultaneously in the same direction over the same water so as to form a series of groups of waves separated by bands of interference, that these groups will advance with half the velocity of the individual waves. This is doubtless an example of the same phenomenon, and shows that the theory of wave motion is capable of explaining the phenomena; but it appears to leave something to be desired,—for instance, why should the bands of interference only progress with half the velocity of propagation in a deep sea, whereas in sound the corresponding bands of interference which constitute the beats move at the same velocity as the waves.

My object in this paper is to point out a fact in connection with wave transmission which appears to have hitherto passed unnoticed at all events in connection with the phenomena described above, of which it affords a clear and complete explanation. One of the several functions performed by waves progressing through a medium is the transmission of energy. Thus the energy which we receive from the sun is brought to us in the waves of light and heat; so in the case of sound the work done by the arm of the drummer is transmitted to our ears by the waves of sound. It is possible however to have waves which travel through a medium without conveying energy; such are the waves caused by the wind on a field of corn. This kind of wave may be well understood by suspending a series of small balls by threads, so that the balls all hang in a row, and the threads are all of the same length. If we then run the finger along, so as to set the balls oscillating in succession, the motion will be such as to give the idea of a series of waves propagated from one end to the other; but in reality there is no propagation, each pendulum swings independently of its neighbours, there is no communication of energy, the waves being merely the result of the general arrangement of the motion.

In this case there is no communication of energy, neither is there any propagation of disturbance. Any one ball may be set swinging without in the least disturbing the others; and what is indicated here is a general law that wherever a disturbance is transmitted through a medium by waves there must always be communication of energy. The rate at which energy is transmitted in different media, or by different systems of waves, is very different. This may be illustrated at once by experiment. If the balls just described are all connected by an elastic thread, then they can no longer swing independently. If one be set in motion then, by virtue of the connecting thread, it will communicate its motion to its neighbours until they swing with it, so that now waves would be propagated through the balls. The rate at which a ball would impart its motion, *i.e.* its energy, to its neighbours would clearly depend on the tension of the connecting thread. If this was very slight compared with the weight of the balls it would stretch, and the ball might accomplish several swings before it had set its neighbours in full motion, so that of the initial energy of disturbance a very small portion is communicated at each swing. But if the tension of the thread be great compared with the weight of the balls, one ball cannot be disturbed without causing a similar disturbance in its neighbours, and then the whole energy will be communicated. This is simply illustrated by laying a rope or chain on the ground, and fastening down one end; if then the loose end be shaken up and down the wriggle caused will travel to the other end, leaving the rope perfectly straight and quiet on the ground behind it, so that in this case it is at once seen that the wave carries forward with it the whole energy of disturbance.

The straight cord and the pendulous balls represent media in which the waves are at the opposite limits—in one case none of the energy of disturbance is transmitted, and in the other case the whole is transmitted. Between these two limits we may have waves of infinite variety, in which any degree of energy from all to nothing is transmitted. Now the waves of sound belong to the class of the cord in which all the energy is transmitted; but what I want particularly to make clear is that the waves on water are between the limits they are analogous to the waves in the balls suspended when connected by an elastic string And I have so to show that according to the accepted theory of wave motion the waves on deep water only carry forward half the energy of disturbance.

In regular trochoidal waves the particles move in vertical circles with a constant velocity and are always subject to the same pressure. Of the energy of disturbance half goes to give motion to the particles and half to raise them from their initial position to the mean height which they occupy during the passage of the wave.

Now the mean horizontal positions of the particles remain unaltered by the waves, hence, since their velocities are constant, none of their energy of motion is transmitted; nor since the pressure on each particle is constant can any energy be transmitted by pressure. The only energy therefore which remains to be transmitted is the energy due to elevation, and that this is transmitted is obvious since the particles are moving forward when above their mean position, and backward when below it. This energy constitutes half the energy of disturbance, and this is therefore the amount transmitted.

For a definite mathematical proof that—

In waves on deep water the rate at which the energy is carried forward is ½ the energy of disturbance per unit of length × by the rate of propagation.

Let h_0 be the initial height occupied by a particle supposed to be of unit weight, h_1 the height of the centre of the circle in which it moves as the wave passes, r the radius of the orbit, and θ the angle the radius vector makes with the horizontal diameter, then the height of the particle above its initial position is $h_1 - h_0 + r \sin \theta$, adding to this the height due to its velocity we have the whole energy of disturbance—

$$= 2(h_1 - h_0) + r \sin \theta.$$

The velocity of the particle is—

$$= \sqrt{2g(h_1 - h_0)},$$

and the horizontal component of this is—

$$= \sqrt{2g(h_1 - h_0)} . \sin \theta.$$

Therefore the rate at which energy is being transmitted by the particle—

$$= \left\{2(h_1 - h_0) + r \sin \theta\right\} \sqrt{2g(h_1 - h_0)} . \sin \theta.$$

and the mean of this—

$$= \frac{1}{2\pi} \int_0^{2\pi} \left\{2(h_1 - h_0) + r \sin \theta\right\} \sqrt{2g(h_1 - h_0)} \sin \theta \, d\theta$$

$$= \tfrac{1}{2} r \sqrt{2g(h_1 - h_0)},$$

and if λ be the length of the wave and $n\lambda$ the rate of propagation—

$$h_1 - h_0 = \frac{\pi r^2}{\lambda} \text{ and } \frac{2g}{\lambda} = 4\pi r^2,$$

∴ the mean rate at which energy is transmitted by this particle

$$= n\lambda(h_1 - h_0),$$

or the rate of propagation multiplied by half the energy of disturbance. Q.E.D.

It now remains to come back to the speed of the groups of waves and to show that *if the rate at which energy is transmitted is equal to the rate of propagation multiplied by half the energy of disturbance, then the velocity of a group of waves will be ½ that of the individual waves.*

Let P_1, P_2, P_3, P_4 be points similarly situated in a series of waves which gradually diminish in size and energy of disturbance from P_3 to P_1, in which direction they are moving. Let E be the energy of disturbance between P_1 and P_2 at time t, $E + a$ the energy between P_2 and P_3, $E + 2a$ between P_3 and P_4, and so on.

Then at the time $t + n$ after the wave has moved through one wave-length it follows that the energy between P_1 and P_2 will be—

$$= \frac{E + E + a}{2} = E + \frac{a}{2},$$

and between P_2 and P_3 will

$$= \frac{E + a + E + 2a}{2} = E + \frac{3a}{2},$$

and again after another interval, n, the energies between P_1 and P_2, P_2 and P_3 will be respectively—

$$= \frac{E + \frac{a}{2} + E + \frac{3a}{2}}{2} = E + a,$$

$$= \frac{E + \frac{3a}{2} + E + \frac{5a}{2}}{2} = E + 2a.$$

So that after the waves have advanced through two wave-lengths the distribution of the energy will have advanced one, or the speed of the groups is ½ that of the waves. Q.E.D.

Of course this reasoning applies equally to the waves on the suspended balls, when connected by an elastic string, as to water; and in this case the conclusions may be verified for, as on water, the groups of waves travel at a slower rate than the waves. This experiment tends to throw light on the manner in which the result is brought about. When a ball is disturbed, the disturbance is partly communicated to the adjacent ball by the connecting string, and part retained in the form of pendulous oscillation; that part which is propagated forward is constantly reduced in imparting oscillations to the successive balls and soon dies out, while the motion retained by the swinging pendulum constantly gives rise to succeeding waves until it is all absorbed. If the tightness of the cord be adjusted to the length of the suspending threads, waves may be made to travel along in a manner closely resembling the way in which they travel on water, the speed of the group being ½ the speed of the individual waves.

Although the progression of a group has hitherto been spoken of as if the form of the group was unaltered, this is by no means the case as a rule.

In the mathematical investigation it was assumed that the motion of the particles is circular; this, however, cannot be the case when the succeeding waves differ in size by a sensible quantity, and hence in this case the form of the group cannot be permanent. And it may be further shown that as a small group proceeds, the number of waves which compose it will continually increase, until the gradation becomes indefinitely small; and this is exactly what is observed, whether on water or on the strings.

So far as we have considered deep water, when the water is shallow compared with the length of the waves, the results are modified, but in this case the results as observed are strictly in accordance with the theory.

According to this, as waves enter shallow water the motion of the particles becomes elliptical, the eccentricity depending on the shallowness of the water; and it may be shown that under these circumstances the rate at which energy is transmitted is increased, until when the elliptic paths approach to straight lines the whole energy is transmitted, and consequently it follows that the rates of the speed of the groups to the speed of the waves will increase as the water becomes shallower, until they are sensibly the same. In which case only the groups of waves are permanent, and Mr. Scott Russell's solitary wave is possible. Besides the explanation thus given of these various phenomena, it appears that we have here a means of making some important verifications of the assumptions on which the wave theory is based; for the relative speed of the groups and the waves which compose them affords a criterion as to whether or not the particles move in circles.

SECTION D.—BIOLOGY.

Department of Anthropology.

ADDRESS BY FRANCIS GALTON, F.R.S.

PERMIT me to say a few words of personal explanation to account for the form of the address I am about to offer. It has been the custom of my predecessors to give an account of recent proceedings in anthropology, and to touch on many branches of that wide subject. But I am at this moment unprepared to follow their example with the completeness I should desire and you have a right to expect, owing to the suddenness with which I have been called upon to occupy this chair. I had indeed the honour of being nominated to the post last spring, but circumstances arising which made it highly probable that I should be prevented from attending this meeting, I was compelled to ask to be superseded. New arrangements were then made by the Council, and I thought no more about the matter. However, at the last moment, the accomplished ethnologist who otherwise would have presided over you was himself debarred by illness from attending, and the original plan had to be reverted to.

Under these circumstances I thought it best to depart somewhat from the usual form of addresses, and to confine myself to certain topics with which I happen to have been recently engaged, even at the risk of incurring the charge of submitting to you a memoir rather than an address.

I propose to speak of the study of those groups of men who are sufficiently similar in their mental characters or in their physiognomy, or in both, to admit of classification; and I especially desire to show that many methods exist of pursuing the inquiry in a strictly scientific manner, although it has hitherto been too often conducted with extreme laxity.

The types of character of which I speak are such as those described by Theophrastus, La Bruyére, and others, or such as may be read of in ordinary literature and are universally recognised as being exceedingly true to nature. There are no worthier professors of this branch of anthropology than the writers of the higher works of fiction, who are ever on the watch to discriminate varieties of character, and who have the art of describing them. It would, I think, be a valuable service to anthropology if some person well versed in literature were to compile a volume of extracts from novels and plays that should illustrate the prevalent types of human character and temperament. What, however, I especially wish to point out is, that it has of late years become possible to pursue an inquiry into certain fundamental qualities of the mind by the aid of exact measurements. Most of you are aware of the recent progress of what has been termed psycho-physics, or the science of subjecting mental processes to physical measurements and to physical laws. I do not now propose to speak of the laws that have been deduced, such as that which is known by the name of Fechner, and its numerous offshoots, including the law of fatigue, but I will briefly allude to a few instances of measurement of mental processes, merely to recall them to your memory. They will show what I desire to lay stress upon, that the very foundations of the differences between the mental qualities of man and man admit of being gauged by a scale of inches and a clock.

Take, for example, the rate at which a sensation or a volition travels along the nerves, which has been the subject of numerous beautiful experiments. We now know that it is far from instantaneous, having indeed no higher velocity than that of a railway express train. This slowness of pace, speaking relatively to the requirements that the nerves have to fulfil, is quite sufficient to account for the fact that very small animals are quicker than very large ones in evading rapid blows, and for the other fact that the eye and the ear are situated in almost all animals in the head, in order that as little time as possible should be lost on the road, in transmitting their impressions to the brain. Now the velocity of the complete process of to and fro nerve transmission in persons of different temperaments has not been yet ascertained with the desired precision. Such difference as there may be is obviously a fundamental characteristic and one that well deserves careful examination. I may take this opportunity of suggesting a simple inquiry that would throw much light on the degree in which its velocity varies in different persons, and how far it is correlated with temperament and external physical characteristics. Before I describe the inquiry I suggest, and towards which I have already collected a few data, it is necessary that I should explain the meaning of a term in common use among astronomers, namely, "personal equation." It is a well known fact that different observers make different estimates of the exact moment of the occurrence of any event. There is a common astronomical observation, in which the moment has to be recorded at which a star that is travelling athwart the field of view of a fixed telescope, crosses the fine vertical wire by which that field of view is intersected. In making this observation it is found that some observers are over sanguine and anticipate the event, while others are sluggish and allow the event to pass by before they succeed in noting it. This is by no means the effect of inexperience or maladroitness, but it is a persistent characteristic of each individual, however practised in the art of making observations or however attentive he may be. The difference between the time of a man's noting the event and that of its actual occurrence is called his personal equation. It remains curiously constant in every case for successive years, it is carefully ascertained for every assistant in every observatory, it is published along with his observations, and is applied to them just as a correction would be applied to measurements made by a foot-rule, that was known to be too long or too short by some definite amount. Therefore the magnitude of a man's personal equation indicates a very fundamental peculiarity of his constitution; and the inquiry I would suggest, is to make a comparison of the age, height, weight, colour of hair and eyes, and temperament (so far as it may admit of definition) in each observer in the various observatories at home and abroad, with the amount of his personal equation. We should thus learn how far the more obvious physical characteristics may be correlated with certain mental ones, and we should perhaps obtain a more precise scale of temperaments than we have at present.

Another subject of exact measurement is the time occupied in forming an elementary judgment. If a simple signal be suddenly shown, and if the observer presses a stop as quickly as he can when he sees it, some little time will certainly be lost, owing to delay in nerve transmission and to the sluggishness of the mechanical apparatus. In making experiments on the rate of judgment, the amount of this interval is first ascertained. Then the observer prepares himself for the exhibition of a signal that may be either black or white, but he is left ignorant which of the two it will be. He is to press a stop with his right hand in the first event, and another stop with his left hand in the second one. The trial is then made, and a much longer interval is found to have elapsed between the exhibition of the alternative signal, and the record of it, than had elapsed when a simple signal was used. There has been hesitation and delay: in short, the simplest act of judgment is found to consume a definite time. It is obvious that here, again, we have means of ascertaining differences in the rapidity of forming elementary judgments and of classifying individuals accordingly.

It would be easy to pursue the subject of the measurement of mental qualities to considerable length, by describing other kinds of experiment, for they are numerous and varied. Among these is the plan of Prof. Jevons, of suddenly exhibiting an unknown number of beans in a box, and requiring an estimate of their number to be immediately called out. A comparison of the estimate with the fact, in a large number of trials, brought out a very interesting scale of the accuracy of such estimates, which would of course vary in different individuals, and might be used as a means of classification. I can imagine few greater services to anthropology than the collection of the various experiments that have been imagined to reduce the faculties of the mind to exact measurement. They have engaged the attention of the highest philosophers, but have never, so far as I am aware, been brought compendiously together, and have certainly not been introduced, as they deserve, to general notice.

Wherever we are able to perceive differences by inter-comparison, we may reasonably hope that we may at some future time succeed in submitting those differences to measurement. The history of science is the history of such triumphs. I will ask your attention to a very notable instance of this, namely, that of the establishment of the scale of the thermometer. You are aware that the possibility of making a standard thermometric scale wholly depends upon that of determining two fixed points of temperature, the interval between them being graduated into a scale of equal parts. These points are, I need hardly say, the temperatures of freezing and of boiling water respectively. On this basis we are able to record temperature with minute accuracy, and the power of doing so has been one of the most important aids to physics and chemistry as well as to other branches of investigation. We have been so accustomed, from our childhood, to hear of degrees of temperature, and our scientific knowledge is so largely based upon exact thermometric measurement, that we cannot easily realise the state of science when the thermometer, as we now use it, was unknown. Yet such was the condition of affairs so recently as two hundred years ago, or thereabouts. The invention of the thermometer, in its present complete form, was largely due to Boyle, and I find in his "Memoirs" (London, 1772, vol. vi. p. 403) a letter that cannot fail to interest us, since it well expresses the need of exact measurement that was then felt in a particular case, where it was soon eminently well supplied, and therefore encourages hope that our present needs as anthropologists may hereafter, in some way or other, be equally well satisfied. The letter is from Dr. John Beale, a great friend and correspondent of Boyle, and is dated February, 1663. He says in it:—

"I see by several of my own thermometers that the glass-men are by you so well instructed to make the stems in equal proportions, that if we could name some degrees, ... we might by the proportions of the glass make our discourses intelligible in mentioning what degrees of cold our greatest frosts do produce. . . . . If we can discourse of heat and cold in their several degrees, so as we may signify the same intelligibly, ... it is more than our forefathers have taught us to do hitherto."

The principal experiments by which the mental faculties may be measured require, unfortunately for us, rather costly and delicate apparatus, and until physiological laboratories are more numerous than at present, we can hardly expect that they will be pursued by many persons.

Let us now suppose that, by one or more of the methods I have described or alluded to, we have succeeded in obtaining a group of persons resembling one another in some mental quality, and that we desire to determine the external physical characteristics and features most commonly associated with it. I have nothing new to say as regards the usual anthropometric measurements, but I wish to speak of the great convenience of photographs in conveying those subtle but clearly visible peculiarities of outline which almost elude measurement. It is strange that no use is made of photography to obtain careful studies of the head and features. No single view can possibly exhibit the whole of a solid, but we require for that purpose views to be taken from three points at right angles to one another. Just as the architect requires to know the elevation, side view, and plan of a house, so the anthropologist ought to have the full face, profile, and view of the head from above of the individual whose features he is studying.

It might be a great convenience, when numerous portraits have to be rapidly and inexpensively taken for the purpose of anthropological studies, to arrange a solid framework supporting three mirrors, that shall afford the views of which I have been speaking, by reflection, at the same moment that the direct picture of the sitter is taken. He would present a three-quarter face to the camera for the direct picture, one adjacent mirror would reflect his profile towards it, another on the opposite side would reflect his full face, and a third sloping over him would reflect the head as seen from above. All the reflected images would lie at the same optical distance from the camera, and would, therefore, be on the same scale, but they would be on a somewhat smaller scale than the picture taken directly. The result would be an ordinary photographic picture of the sitter surrounded by three different views of his head. Scales of inches attached to the framework would appear in the picture and give the means of exact measurement.

Having obtained drawings or photographs of several persons alike in most respects, but differing in minor details, what sure method is there of extracting the typical characteristics from them? I may mention a plan which had occurred both to Mr. Herbert Spencer and myself, the principle of which is to superimpose optically the various drawings and to accept the aggregate result. Mr. Spencer suggested to me in conversation that the drawings reduced to the same scale might be traced on separate pieces of transparent paper and secured one upon another, and then held between the eye and the light. I have attempted this with some success. My own idea was to throw faint images of the several portraits, in succession, upon the same sensitised photographic plate. I may add that it is perfectly easy to superimpose optically two portraits by means of a stereoscope, and that a person who is used to handle instruments will find a common double eye-glass fitted with stereoscopic lenses to be almost as effectual and far handier than the boxes sold in shops.

In illustration of what I have said about photographic portraits, I will allude to some recent experiences of my own in a subject that I have still under consideration. In previous publications I have treated of men who have been the glory of mankind, I would now call your attention to those who are its disgrace. The particular group of men I have in view are the criminals of England, who have been condemned to long terms of penal servitude for various heinous offences.

It is needless to enlarge on the obvious fact that many persons have become convicts who, if they had been afforded the average chances of doing well, would have lived up to a fair standard of virtue. Neither need I enlarge on the other equally obvious fact, that a very large number of men escape criminal punishment, who in reality deserve it quite as much as an average convict. Making every allowance for these two elements of uncertainty, no reasonable man can entertain a doubt that the convict class includes a large proportion of consummate scoundrels, and that we are entitled to expect to find in any large body of convicts a prevalence of the truly criminal charactistics, whatever these may be.

Criminality, though not very various in its development, is extremely complex in its origin: nevertheless, certain general conclusions are arrived at by the best writers on the subject, among whom I would certainly rank Prosper Despine. The ideal criminal has three peculiarities of character; his conscience is almost deficient, his instincts are vicious, and his power of self-control is very weak. As a consequence of all this, he usually detests continuous labour. This statement applies to the criminal classes generally, the special conditions that determine the description of crime being the character of the instincts; and the fact of the absence of self-control being due to ungovernable temper, or to passion, or to mere imbecility.

The deficiency of conscience in criminals, as shown by the absence of genuine remorse for their guilt, appears to astonish all who first become familiar with the details of prison life. Scenes of heartrending despair are hardly ever witnessed among prisoners; their sleep is broken by no uneasy dreams—on the contrary, it is easy and sound; they have also excellent appetites. But hypocrisy is a very common vice; and all my information agrees in one particular, as to the utter untruthfulness of criminals, however plausible their statements may appear to be.

The subject of vicious instincts is a very large one; we must guard ourselves against looking upon them as perversions, inasmuch as they may be strictly in accordance with the healthy nature of the man, and, being transmissible by inheritance, may become the normal characteristics of a healthy race, just as the sheep-dog, the retriever, the pointer, and the bull-dog have their several instincts. There can be no greater popular error than the supposition that natural instinct is a perfectly trustworthy guide, for there are striking contradictions to such an opinion in individuals of every description of animal. All that we are entitled to say is, that the prevalent instincts of each race are trustworthy, not those of every individual. A man who is counted as an atrocious criminal by society, and is punished as such by the law, may nevertheless have acted in strict accordance with his instincts. The ideal criminal is deficient in qualities that oppose his vicious instincts; he has neither the natural regard for others which lies at the base of conscience, nor has he sufficient self-control to enable him to consider his own selfish interests in the long run. He cannot be preserved from criminal misadventure, either by altruistic or by intelligently egoistic sentiments.

It becomes an interesting question to know how far these peculiarities may be correlated with physical characteristics and features. Through the cordial and ready assistance of Sir Edmund Du Cane, the Surveyor-General of Prisons, who has himself contributed a valuable memoir to the Social Science Congress on the subject, I was enabled to examine the many thousand photographs of criminals that are preserved for purposes of identification at the Home Office, to visit prisons and confer with the authorities, and lastly to procure for my own private statistical inquiries a large number of copies of photographs of heinous criminals. I may as well say, that I begged that the photographs should be furnished me without any names attached to them, but simply classified in three groups according to the nature of the crime. The first group included murder, manslaughter, and burglary; the second group included felony and forgery; and the third group referred to sexual crimes. The photographs were of criminals who had been sentenced to long terms of penal servitude.

By familiarising myself with the collection, and continually sorting the photographs in tentative ways, certain natural classes began to appear, some of which are exceedingly well marked. It was also very evident that the three groups of criminals contributed in very different proportions to the different physiognomic classes.

This is not the place to go further into details: indeed my inquiry is far from complete. I merely quote my experiences in order to show the way in which questions of character, physiognomy, and temperament admit of being scientifically approached, and to give an instance of the helpfulness of photography. If I had had the profiles and the shape of the head as seen from above, my results would have been much more instructive. Thus, to take a single instance, I have seen many pencil studies in outline of selected criminal faces drawn by Dr. Clarke, the accomplished and zealous medical officer of Pentonville Prison; and in these sketches a certain very characteristic profile seemed to me conspicuously prevalent. I should have been very glad of photographs to corroborate this. So, again, if I had had photographic views of the head taken from above, I could have tested, among other matters, the truth of Prof. Benedict's assertion about the abnormally small size of the back of the head in criminals.

I have thus far spoken of the characters and physiognomy of well-marked varieties of men: the anthropologist has next to consider the life history of those varieties, and especially their tendency to perpetuate themselves, whether to displace other varieties and to spread, or else to die out. In illustration of this, I will proceed with what appears to be the history of the criminal class. Its perpetuation by heredity is a question that deserves more careful investigation than it has received, but it is on many accounts more difficult to grapple with than it may at first sight appear to be. The vagrant habits of the criminal classes, their illegitimate unions and extreme untruthfulness, are among the difficulties. It is, however, easy to show that the criminal nature tends to be inherited while, on the other hand, it is impossible that women who spend a large portion of the best years of their lives in prison can contribute many children to the population. The true state of the case appears to be that the criminal population receives steady accessions from classes who, without having strongly marked criminal natures, do nevertheless belong to a type of humanity that is exceedingly ill-suited to play a respectable part in our modern civilisation, though they are well-suited to flourish under half-savage conditions, being naturally both healthy and prolific. These persons are apt to go to the bad; their daughters consort with criminals and become the parents of criminals. An extraordinary example of this is given by the history of the infamous Jukes family in America, whose pedigree has been made out with extraordinary care, during no less than seven generations, and is the subject of an elaborate memoir printed in the thirty-first annual report of the Prison Association of New York, 1876. It includes no less than 540 individuals of Jukes blood, among whom the number of persons who degraded into criminality, pauperism, or disease, is frightful to contemplate.

It is difficult to summarise the results in a few plain figures, but I will state those respecting the fifth generation, through the eldest of the five prolific daughters of the man who is the common ancestor of the race. The total number of these was 103, of whom thirty-eight came through an illegitimate granddaughter, and eighty-five through legitimate grandchildren. Out of the thirty-eight, sixteen have been in gaol, six of them for heinous offences, one of these having been commited no less than nine times; eleven others were paupers or led openly disreputable lives; four were notoriously intemperate; the history of three had not been traced, and only four were known to have done well. The great majority of the women consorted with criminals. As to the 85 legitimate descendants, they were less flagrantly bad, for only five of them had been in gaol and only thirteen others had been paupers. Now the ancestor of all this mischief, who was born about the year 1730, is described as having been a hunter and a fisher, a jolly companionable man, averse to steady labour, working hard and idling by turns, and who had numerous illegitimate children, whose issue has not been traced. He was, in fact, a somewhat good specimen of a half-savage, without any seriously criminal instincts. The girls were apparently attractive, marrying early and sometimes not badly; but the gipsy-like character of the race was unsuited to success in a civilised country. So the descendants went to the bad, and the hereditary moral weaknesses they may have had rose to the surface and worked their mischief without a check. Cohabiting with criminals and being extremely prolific, the result was the production of a stock exceeding 500 in number, of a prevalent criminal type. Through disease and intemperance the breed is now rapidly diminishing; the infant mortality has of late been horrible among them, but fortunately the women of the present generation bear usually but few children, and many of them are altogether childless.

This is not the place to go further into details. I have alluded to the Jukes family in order to show what extremely important topics lie open to inquiry in a single branch of anthropological research and to stimulate others to follow it out. There can be no more interesting subject to us than the quality of the stock of our countrymen and of the human race generally, and there can be no more worthy inquiry than that which leads to an explanation of the conditions under which it deteriorates or improves.

SECTION G.—MECHANICAL SCIENCE.

THE following is an abstract of the address of the president, Mr. E. Woods, C.E.:—The president selected the question of railway brakes as his topic. He said that the provision of adequate brake power to control trains was a subject which had latterly much engaged the attention of railway companies and of the Government. In the summer of 1874 a Royal Commission was appointed to inquire into the causes of accidents on railways, and the possibility of removing them by further legislation. One branch of the inquiry naturally led to the consideration of accidents caused by collision; and it appeared from the evidence taken before the Commissioners that trains were generally provided with insufficient controlling power, and that the distance within which, when running at high speed, they could be stopped by the brake ordinarily in use had not been ascertained with any approach to accuracy. It was under these circumstances that the Commissioners applied to the railway companies to institute a definite series of experiments to test the value of hand-brakes, and the effect of various systems of continuous brakes. In conjunction with Col. Inglis, R.E., he was intrusted by the Commissioners with the supervision of the experiments, to the satisfactory conduct of which the railway companies contributed in the most liberal manner. With few exceptions, and up to a comparatively recent period, the companies had remained content with the brake appliances which were common forty years ago. These, no doubt, were sufficient to control the trains in those early days, few as they were in number, and limited in weight and speed. The brakes were applied separately, and by hand-power, always to the tender, and usually to some few of the carriages and to the guard's van or vans, if such accompanied the train. As long ago as 1858 the Board of Trade called the special attention of the railway companies to the fact that the amount of brake power then habitually applied was insufficient to prevent frequent accidents occurring from collisions, many of which they considered might be averted. Particular reference was made to two systems which had come into daily use on the East Lancashire and the Lancashire and Yorkshire railways, namely, the brakes of Newall and of Fay, by means of which trains of ninety to 100 tons weight, running fifty miles an hour, could be effectually controlled by driver or guard, even when proceeding down steep inclines, and brought up within a moderate distance. It was certainly matter for surprise, seeing the advantage of continuous brakes, that the railway companies should have so long tolerated the old system, and been so slow to adopt a method which, instead of being dependent for its due action on the attention of several persons, was effectually placed under the control of one. This lethargy prevailed, too, throughout a period when increased speed had come to be demanded, when augmenting traffic required heavier trains, and when, consequently, more ponderous and powerful engines had to be used—circumstances which ought to have induced the companies to effect simultaneously a readjustment of their brake appliances. After the year 1850 many attempts were made to supersede the ordinary type of brake, some of the brakes introduced being self-acting and put into operation by the momentum of the train, while others acted as sledges or shoes. None, however, proved successful. The continuous breaks of Newall and Fay simply involved a wider distribution of power over the different vehicles of the train, and gave the means of applying that power by one, or, at most, two attendants. It was in that direction that the ingenuity of inventors had recently been turned, and there were now several systems of continuous brakes in successful working on the leading railways, each claiming some special advantages over its rivals, whether as more simple in construction, less expensive in application, or effecting more complete control of the train. The Royal Commissioners desired that attention should be primarily directed to the following points:—1. The distances within which trains running at various speeds could be stopped by the system of brakes in ordinary use on the different lines of the United Kingdom; (2) what results could be obtained by the additional application of brake power; and (3) how far a very large amount of brake power could be suddenly resorted to with safety in heavy trains running at high speeds. For the purpose of the experiments a portion of the Nottingham and Lincoln branch of the Midland Railway was selected as offering a piece of line comparatively level and free from any sharp curves. Six companies furnished eight complete trains, which represented as many systems of continuous brakes comprehended in four classes, namely, (1) Clarke's and Webb's and Fay's brakes, applied by ordinary mechanical gear; (2) Smith's and Westinghouse's vacuum brakes, actuated by atmospheric pressure produced by exhaustion of air; (3) Westinghouse's and Steel McInnes's air brakes; and (4) Clarke's and Barker's hydraulic brakes. The experiments extended over a week, and comprised several series. It was demonstrated that the friction of a complete train, in which the weight of the engine and tender constituted, say one-fourth of the gross weight, inclusive of the atmospheric resistance it encountered in its course, was 42-100ths per cent., or about $9\frac{1}{2}$ lbs. per ton. This result confirmed what long experience had led them to anticipate. It was discovered further that, on a level line, a train running at the rate of forty-five miles an hour could be stopped by hand brakes within 1,000 yards, or, if at the rate of sixty miles, within 1,700 yards. The necessity for some greater control over fast passenger trains was thus rendered obvious. Through the want of a larger amount of brake power much time was lost on a journey, when the stoppages were frequent, the drivers being compelled to slacken speed at long distances from the stopping-places. It seemed, indeed, scarcely to admit of question that a system which was deemed necessary in special cases might be advantageously applicable in all cases; that to render the control of a train complete, brakes should be applied to all, or nearly all, the wheels; and that, at least, the driver, if not the guards, should possess the power of promptly bringing the whole into action. The truth of the principle was now very generally admitted by the leading companies, some of whom had already adopted continuous brakes, while others were preparing to do so. Rather startling disparities were disclosed during the experiments. Some of the disparities were attributable to the contrivances being of comparatively recent origin, but others were clearly owing to the principle upon which the action of the brake was founded. As between the air-pressure and the vacuum brakes there was a loss of 6¼ seconds, which in a train running sixty miles an hour was equivalent to 180 yards additional space traversed in the stop. Three of the experiments involved the application of all available power for stopping. Sand was used, and was found to add sensibly to the stopping power. On an average it made an addition of 1·30 per cent. to the retarding force otherwise brought into play. The trials proved in a very striking manner the great advantage of continuous brakes, for even in their least effective form they afforded more than double the stopping power of the usual hand brakes, whilst in their most effective form the power was quadrupled. He was of opinion that no system could be considered satisfactory which did not produce a retarding power of at least 8 to 10 per cent. of the entire weight of the train, in other words, a power by which fast trains could be stopped in from one-third to one-fourth less time than at present. Obviously the stopping distance was primarily influenced by two considerations:—(1) The length of the interval which elapsed between the brake being put into operation and its taking an effective grip on the wheels; and (2) the amount of pressure brought to bear on each wheel, and the constancy or otherwise of the action after the blocks had gripped the wheels. The unpleasant sensation often experienced during quick stoppages was produced by intermittent and fitful action. After the brakes had been made to bite the wheels their hold became relaxed, a slip took place, followed by successive bites and slips, the latter giving rise to sudden accelerations of speed. The action of a perfect brake should exactly resemble that which gravity would cause if an ascending incline of uniform gradient could be suddenly placed in front of the train to prevent its motion. Under such conditions no inconvenience or danger need be apprehended from the stoppage being accomplished within even a shorter distance than any that was effected during the experiments. A valuable addition of power, under the immediate control of the driver, would be afforded by the fitting of brakes to the engine, and he was glad to find that the recommendation of the Royal Commissioners in this respect had met with prompt attention at the hands of the railway companies. The question of the best material for brake blocks had of late received a good deal of consideration, and it would seem that cast-iron, and even steel, was fast superseding wood. It generally happened that wheels did not become skidded until the speed of the train had been materially reduced. It seemed desirable, therefore, that for ordinary stops the brake pressure should be applied so as to act just short of skidding the wheels, the full skidding power being only used in cases of imminent danger. The general adoption of an effective system of continuous brakes on carriages which had to run from one line to another would be productive of much advantage, for then, in breaking-up and remaking a train at any junction station, the carriages would be found ready-fitted with the requisite appliances for working. If allied companies could only agree to adopt the same system, brake improvements would proceed with far greater rapidity than at present, and public convenience would thereby be promoted. The time had arrived not only when each system should be scrutinised and tested in the most complete manner, but when the companies should clearly set before themselves the conditions which a good continuous brake ought to supply. A study of the different methods which came under his (the lecturer's) notice during the experiments pointed to the following considerations as necessary in view of the provision of perfect brake power for heavy fast trains:—1. The brake power should be applied to all the wheels of all the vehicles throughout the train. 2. The power by which the blocks were forced upon the wheels should be adequate for skidding the wheels on the speed becoming moderately reduced. 3. The driver should have the whole of the brake power completely under his command, and be able to apply it at a moment's notice, as he was the first person likely to discover any obstruction ahead, and was primarily responsible for the regard of the danger signals. He could thus stop the train at once, and no time would be lost by his having to signal danger to the guard. 4. The guards should individually possess the like means of applying the continuous brake, so that they might be able to stop the train without reference to the driver, on an emergency which might manifest itself to them but not to him, such, for instance, as a broken axle, or a carriage getting off the line. 5. The power in hand should be so susceptible of modification that the driver should be able to apply a moderate amount only for effecting ordinary stops, while he kept in reserve a proper excess to be used only on emergencies, or on slippery rails. 6. Full brake application should not require more than a very moderate effort on the part of either driver or guard. 7. The pressure should be steady, and distributed as equally as possible over all the wheels, and, with the intervention of some elastic medium, should act upon the wheels in such a way as to prevent too sudden stopping or the snapping of chains, which produced discomfort and inconvenience to the public. 8. The machinery should be of simple construction, not likely soon to get out of order, and admitting of being easily repaired. 9. Indication should be constantly afforded to driver and guards that the brakes were in proper condition to work or otherwise. 10. The power of working the tender brakes and the van brakes by hand might be advantageously retained. 11. The brakes should be self-acting in case of the severance of the train. 12. Automatic action being provided, means should be furnished the brake attendants for modifying that action instantaneously, according to the circumstances in which the train might be placed after an accident. 13. It would be dangerous, and therefore unadvisable, to give to passengers any power over the brakes. Such seemed to be the principal conditions necessary for realising the conception of a perfect brake—a brake which would constitute an invaluable instrument in contingencies of almost daily occurrence at some place or another in the great railway network of the country.

REMARKABLE PLANTS

III.—The Sensitive Plant (*Mimosa pudica*).

In our ordinary popular conception of the difference between the two kingdoms into which the organic world is divided, we are apt to attribute to one a power of spontaneous motion dependent on the possession of a certain internal mental faculty to which we apply the term voluntary power; while a similar property is not considered to be inherent in the members of the other kingdom. The most recent researches throw, to say the least, considerable doubts on the universal applicability of this test to distinguish animals from plants. Now that the Desmidieæ and the Oscillatorieæ are, by universal consent, relegated to the vegetable kingdom, and that many bodies described by Ehrenberg as animals are found to be particular stages in the life-history of certain vegetable organisms, this character seems but to follow in the wake of others which have one by one been abandoned as absolute discriminating tests between the members of the two kingdoms. Among the more commonly-occurring and familiar movements of vegetable tissues, the dependence of which on external mechanical causes is at present but imperfectly understood, are those motions of the leaves and other parts of plants which are comprised under the common designation of Movements of Sensitiveness or Irritability. It has been well shown by Sachs[1] that these movements are of three different kinds, viz.:—

1. Those periodic movements which are produced entirely by internal causes, without the co-operation of any considerable external impulse of any kind. Such movements may be termed *automatic* or *spontaneous*,

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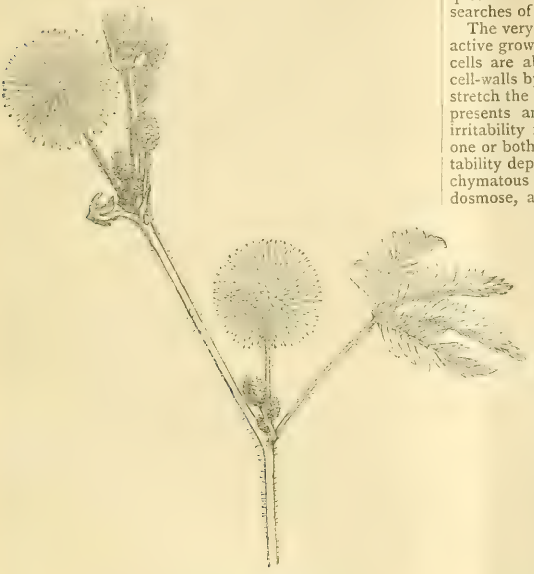
[1] "Text-Book of Botany," English edition, Book III., chap. 5.

and are illustrated by the rhythmical movements of the small lateral leaflets of the trifoliolate leaf of the Indian "telegraph-plant," *Desmodium gyrans*.

2. Those apparently spontaneous motions of leaves and petals which are due to *alternations in the intensity of light and heat*, and therefore obviously to external causes. It is motions of this kind which give rise to the varying diurnal and nocturnal position of the leaves of some plants, and to the closing of certain flowers in the evening or in wet weather.

3. Those movements of foliage-leaves, or in certain cases of organs belonging to the flower, which are due to *sensitiveness to touch or concussion*. A familiar example of this class of movements is furnished by the well-known irritability of the leaves of the Sensitive Plant; and it is to this class that we propose to confine our attention in the present paper.

Two preliminary remarks may be made, which are applicable not only to the special class of movements now under discussion, but also to the two others to which we have alluded above. All these three kinds of movements are manifested only when the parts in question are perfectly mature, and when the peculiarity of their internal structure, which renders the phenomenon possible, is fully developed. In this respect they afford a remarkable contrast to another class of movements exhibited only when the part of the plant is in active growth, of which we have illustrations in the singular phenomena of climbing stems and tendrils described in detail by Darwin in his "Movements and Habits of Climbing Plants." Another peculiarity common to all the three kinds of movements, and again distinguishing them from the movements of climbing plants, is that they belong entirely to the foliar or appendicular organs, *i.e.*, leaves in the wide botanical sense, as including foliage-leaves, sepals, petals, stamens, and carpels, and not in any case to axial structures or stems and branches.



Sensitive Plant (*Mimosa pudica*, D.C.).

With regard to the anatomical structure of the parts which exhibit the phenomena in question, it is seen that in almost all cases a mass of very succulent parenchyma (small-celled cellular tissue), several layers of cells in thickness, envelopes an axial or central fibro-vascular bundle, or a few such bundles running parallel to one another, these bundles not being sufficiently lignified to be hard, and therefore remaining flexible and extensible, and permitting the upward and downward flexions in which alone the movement generally consists; the whole is enveloped by an only feebly developed epidermis. The best known illustrations of these movements are furnished by the two species of "Sensitive Plant," *Mimosa pudica* and *sensitiva*, but are also exhibited by the leaves of several other *Mimosas*, and of species of *Oxalis*, *Robinia*, *Desmanthus*, and *Smithia*; by the stamens of several species of *Berberis* and of many Compositæ, and by the stigmas of *Mimulus*, *Martynia*, *Goldfussia*, *Stylidium*, and *Megaclinium*. The following account of the mechanical forces which set in motion the phenomena in question is taken mainly from the very laborious researches of Pfeffer.[1]

The very succulent parenchyma is, when the plant is in active growth, always in a very turgid condition; *i.e.*, the cells are absorbing sap freely through their permeable cell-walls by endosmotic force; and in so doing tend to stretch the axial bundle, as well as the epidermis which presents an opposing resistance. The sensitiveness or irritability resides entirely in the parenchyma, either on one or both sides of the fibro-vascular bundle. The irritability depends on a two-fold cause: firstly, the parenchymatous cells are perpetually absorbing water by endosmose, and thus placing the cell-walls in a state of tension; and secondly, a slight impulse imparted to the sensitive cells causes a portion of the absorbed fluid to be driven out through their cell-walls. The cause of the movement itself is believed by Pfeffer to be this: that at the moment when the turgid cells are giving off water, the elasticity of their tense cell-walls comes into play, causing them to contract in proportion to the amount of water expelled. Inasmuch as this water escapes into the intercellular spaces of the sensitive tissue, and from thence is partially transferred to other non-sensitive portions of the plant, the sensitive tissue decreases in volume, while the non-sensitive portion in some other part of the organ becomes correspondingly expanded, the epidermis of the sensitive portion at the same time contracting from its elasticity. This side therefore becomes concave, the other convex; and the sensitive organ in consequence bends, carrying with it whatever other organs it may bear, which therefore rise or fall according as the concavity of the curvature is on the upper or under side of the organ. Immediately after this has taken place the organ is no longer sensitive, the flaccid cells having too little turgidity to allow of the escape of any more water. But after a short time they again absorb water; their turgidity increases; their cell-walls become again stretched or tense; and the previous sensitive condition, as well as the original position of the parts, is again restored.

The following is Sachs' and Pfeffer's description of the anatomy of one of the common Sensitive Plants, *Mimosa pudica*. The leaf is bi-pinnate, consisting of a petiole from 4 to 6 centimetres long, with two pairs of secondary petioles 4 to 5 cm. in length, and on each of these from fifteen to twenty pairs of leaflets 5 to 10 millimetres long and 1·5 to 2 mm. broad. All these parts are connected with one another by the contractile organs described above; every leaflet is immediately attached to the rachis by such an organ from 0·4 to 0·6 mm. long, and this again to the primary petiole by another similar organ from 2 to 3 mm. long and about 1 mm. thick. The base of the petiole itself is transformed into a nearly cylindrical contractile organ or "pulvinus," 4 to 5 mm. long and 2 to 2·5 mm. thick, furnished, like those of the secondary petioles, with a number of long stiff hairs on the under side, the upper side being only slightly hairy or entirely glabrous. The pulvinus consists of a succulent parenchymatous tissue of the kind already described. The cells of the under side are thin-walled, those of the upper side have walls about three times as thick. Each cell contains a moderate quantity of protoplasm, a nucleus, small grains of chlorophyll, starch, and, in addition, a large globular drop consisting of a concentrated solution of tannin surrounded by a pellicle.

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[1] Pfeffer, Physiologische Untersuchungen, Leipzig, 1873.

A somewhat slight concussion of the whole plant causes the contractile organs of the primary petioles of all the leaves to curve downwards, those of the secondary petioles forwards, those of the leaflets forwards and upwards, closing like the wings of a butterfly at rest. After irritation the pulvinus is flaccid, and more flexible than before. A light touch on the hairs on the under side of the pulvinus of the primary and secondary petioles is sufficient to produce the movement; in those of the leaflets the lightest touch on the glabrous upper side. When the temperature is high and the air very damp, the irritability is much greater, and any local irritation incites movements in the neighbouring organs, often in all the leaves of a plant, a phenomenon which has been termed "conduction of irritation." If one of the uppermost leaflets is cut off by a pair of scissors, or its pulvinus touched, or if it is placed in the focus of a burning-glass, the irritation immediately takes place, and this irritation is communicated to the next lower pair of leaflets, and in succession to those at a greater distance; after a short time the leaflets of an adjoining secondary petiole begin to fold together from below upwards, and the same with the other secondary petioles; finally, and often after a considerable time, the primary petiole bends downwards; the phenomenon is then conducted to the primary petiole of the next leaf below, as well as to the next one above. It sometimes happens, however, that particular parts appear to be less susceptible, and do not display the phenomena in question until after they have been once passed by. If the plant is left to itself, the leaflets again expand, and the petioles reassume their erect position after a few minutes; the contractile organs are then again irritable.

That the phenomena of irritability are connected with a displacement of water from the succulent tissue and its replacement by air, is shown by the evident and immediate change in colour; the expulsion of the air from the intercellular spaces and its replacement by water causes the whole organ to assume a darker colour. If, moreover, one of the large contractile organs is cut or punctured, a drop of water immediately escapes from it, and if placed in water it again absorbs it eagerly. A variety of experiments by Sachs, Pfeffer, and Brücke also appear to prove conclusively that the sensitiveness resides in the under, and not in the upper side of the organ.

With regard to external conditions which interfere with the sensitiveness of the leaves of *Mimosa*, they become rigid or insensitive from cold when, the conditions being otherwise favourable, the temperature of the surrounding air remains for some hours below 15° C. (59° F.); the lower the temperature falls below this point, the more quickly does the rigidity set in. With regard to the upper limit, the leaves of the sensitive plant become rigid within an hour in damp air of 40° C. (104° F.), within half an hour in air of 45° C. (113° F.), in a few minutes in air of 49° or 50° C. (122° F.). In water the rigidity from cold sets in at a higher temperature, viz., in a quarter of an hour between 16° and 17° C. (62° F.), and the rigidity from heat at a lower temperature than in air, viz., in a quarter of an hour, between 36° and 40° C. A plant immersed in water of from 19° to 21°·5 C. remains sensitive for eighteen hours or more. The maximum degree of sensitiveness appears to be reached at 30° C. (86° C.), at which temperature the plant is so sensitive that the movement is communicated to a number of leaflets almost simultaneously. During the rigidity from heat, whether in air or water, the leaflets are closed, as after irritation, but the petiole is erect, and when irritated, turns downwards.

If placed in the dark, the irritability to touch is not at first affected, but disappears completely if the darkness lasts for a day or more; when again exposed to light, the sensitiveness is restored after some hours. The position of the parts is, however, very different from that in the insensitive condition caused by heat; the leaflets remain quite expanded, but the secondary petioles are directed downwards, and the primary petiole nearly horizontal. The same effects are caused, though in a less degree, when the supply of light is defective. M. Paul Bert states that the irritability of the leaves of *Mimosa* is destroyed by placing the plant under a bell-glass of green glass almost as completely as if placed in the dark; the plants were entirely killed in twelve days under blackened, in sixteen days under green glass; plants placed beneath white, red, yellow, violet, and blue glasses were still perfectly healthy and sensitive, though varying in the rapidity of their growth.

Drought also causes temporary rigidity. If a plant is left unwatered for a considerable time, the sensitiveness of the leaves perceptibly diminishes with the increasing dryness, and an almost complete rigidity ensues, the primary petiole assuming a horizontal position, and the leaflets expanding; watering the soil causes a return of the sensitiveness after two or three hours.

The same effect is produced if respiration is prevented by exhausting the air. If a plant of *Mimosa* is placed under the receiver of an air-pump and the air gradually exhausted, the leaves first of all fold up, no doubt in consequence of the concussion; but the leaflets then expand, the petiole becomes erect, and, while the leaves assume the same position as after prolonged withdrawal of light, they now remain rigid, resuming their sensitiveness when again brought into the air.

Finally, with regard to the effect of poisonous substances, J. B. Schnetzler has pointed out that the substances which destroy the contractility of animal sarcode also destroy the irritability of the leaves of *Mimosa* and other sensitive organs of plants. Curare has no prejudicial effect in either case, while nicotine, alcohol, and mineral acids destroy both. The vapour of chloroform causes transitory rigidity either in the expanded or in the folded position resulting from irritation.

The genus *Mimosa* is a very large one, forming, together with *Acacia*, the greater part of the sub-order Mimoseæ of Leguminosæ, and embracing about 200 species, natives mostly of tropical America, extending also south of the tropics, and into tropical Africa and the East Indies. They have definite stamens (not more than twice the number of petals), anthers not tipped by a gland, and a pod, the valves of which, when ripe, are either detached entire or break into transverse joints. They are mostly herbs, under-shrubs, or climbers; a few erect much-branched shrubs; one or two trees; a large number are spiny. It is only some of the species that are sensitive. *M. sensitiva*, which is also grown in our greenhouses, differs from *M. pudica* in the leaves having only two pairs of pinnæ, and each pinna only two pairs of ovate leaflets, the inner leaflet of the lower pair being always very small. *M. albida*, another sensitive species occasionally seen in hothouses, has elegant flower-heads of a pale pink colour. Our illustration of *M. pudica* is taken, by permission of Messrs. Longmans, from Thomé's "Textbook of Botany," English edition.

A. W. B.

NOTES

THE health of M. Leverrier is so far restored as to enable him to stay at Dieppe during the bathing season. Learning that he intended to travel for his health, the new Minister of Public Instruction offered M. Leverrier a special credit for expenses, on the ground that "it is the national interest to preserve a man who is an honour to the nation."

THE programme of excursions of the French Association has been published in the Havre papers. It includes visits to Fécamp, a town which is rich in memorials of William the Conqueror; to Villiers-sur-mer and Trouville, and to Balbec, Tancarville, and Lillebonne, where a Roman circus has been discovered; a visit to Havre and vicinity, and an excursion to Rouen and a visit to its manufactures and monuments. In his inaugural speech M. Broca, the president, will deal with the same subject as Prof. Allen Thomson at Plymouth. We regret to state that M. Kuhleman, who had been elected president for 1878 at Clermont-Ferrand, has resigned. The Association will have again to choose a president for 1878, and also for 1879; the latter will act as vice-president next year. According to a decision agreed to last year, the 1878 meeting will take place at Versailles during the International Exhibition, the rules not allowing any meeting to be held in Paris. The organisation of that exceptional meeting, and the measures for the reception of foreign members and associates, will require much consideration.

THE *Denver Tribune* of August 2 announces the arrival in that city of the Hayden scientific party, of which Dr. Hayden, Sir J. D. Hooker, Gen. Strachey, and Prof. Asa Gray form part. Southern Colorado had been explored, and the mountains above George-town and Berthoud's Pass, &c., were then to be visited, when the party were to move on to Utah, Nevada, and California.

PROF. WANKLYN has been elected to the chair of Chemistry and Physics of St. George's Hospital, vacated by the death of Dr. Noad, F.R.S.

THE official paper of the French Republic has gazetted the organisation of the jury and the scheme for distribution of awards for the forthcoming Universal Exhibition. Independently of works of art 100 great prizes and exceptional allocations in silver will be distributed by a special jury composed of the presidents of all the juries; 1,000 gold medals, 4,000 silver medals, 8,000 bronze medals, and 8,000 honourable mentions will be distributed by a number of class or sectional juries. The juries will be appointed by the several Governments in proportion to the number of exhibitors.

THE *Frigorifique*, fitted up for the transportation of meat on the Tellier system with methylic acid, has arrived at Havre, from Brazil, with its cargo in an excellent state of preservation. It is stated that a banquet of the meat will be served during the forthcoming session of the French Association at Havre.

MR. G. S. BOULGER, Professor of Natural History in the Cirencester College, reprints from the *Proceedings* of the Cotteswold Naturalists' Field Club, a pamphlet entitled "Notes Preliminary to a Proposed Flora of Gloucestershire." As the title implies, there is no attempt to arrive at an estimate of the vegetable productions of the county, and the publication would appear to have for its main object the inviting of information on the subject (addressed to Mr. Boulger at the Scientific Club, Savile Row) from those who have in any way worked at its flora.

THE last Annual Report of the Smithsonian Institution relating to the year 1875, contains much of great scientific importance. The institution continues to carry on, with admirable efficiency its two great classes of operations—1st, those relating to the immediate objects of the bequest, viz., the increase and diffusion of knowledge through researches, publications, and exchanges, and 2nd, those which pertain to the care and management of the Government collection in natural history and ethnology constituting the United States National Museum, of which the Institution is the custodian. Under the care of the institution this museum bids fair to become one of the finest in the world. During 1874 important meteorological researches were undertaken by the Institution, and its publications embrace valuable works in nearly all departments of science. Among the papers printed as an Appendix to the present Report, are Arago's Eulogy of Volta, De Candolle's Probable Future of the Human Race, Prof. Prestwich's inaugural lecture on the Past and Future of Geology (which appeared in NATURE at the time), a paper on the Refraction of Sound, by Mr. W. B. Taylor, a paper on an International Code of Ethnological Symbols, and Dr. Abbott's elaborate memoir on the Stone-Age in New Jersey.

SOME of our readers may be interested to know that the Ipswich Museum, under the curatorship of Dr. Taylor, contains a very fine collection of crag fossils. Prof. Ray Lankester, in a letter to a local paper, states his conviction, founded upon wide knowledge of such collections, "That the combination of Mr. Canham's collection with the valuable and unique specimens already presented to the museum by Mr. Alderman Packard, when mayor, and by other public-spirited men, has rendered the collection of crag fossils, shells, teeth, bones, box-stones, and clay nodules, by a long way the most complete in existence. I doubt," Prof. Lankester says, "if any other town possesses—certainly no English town does—so complete and valuable a series of specimens illustrative of its local geology."

PETERMANN'S *Mittheilungen* for September will contain a map of considerable interest at the present time, but also of the highest permanent value,—is a map of the region between and including Bulgaria, S.E. Servia, and the Balkans. This is the result of many journeys made by the author, F. Kanitz, between the years 1860 and 1875, and is accompanied with a detailed account of the results obtained. This same number will contain the conclusion of Güssfeldt's travels in the Arabian Desert, and of Polakowsky's paper on the Vegetation of Costa Rica.

THE *Bulletin* of the Paris Geographical Society for June contains a long paper by M. J. Dupuis on his journey in Yunnan.

COL. GORDON, Governor-General of Upper Egypt, has made a contract with Messrs. Yarrow and Co., of Poplar, for the construction of four very light draft steel steamers, for use on Lake Albert Nyanza, and for opening up the navigation of the rivers in Central Africa. These steamers will be carried on land on the backs of negroes, and consequently Messrs. Yarrow and Co. have to sub-divide the packages in such a manner that none shall exceed 200lb. weight. It is estimated that no less than 4,000 men will be employed for the porterage of these vessels.

WE have received No. 3 of *Appalachia*, the journal of the Appalachian Mountain Club, which contains several papers of general interest.

STANFORD'S Library Map of Africa, originally constructed by the late A. Keith Johnston, and of which a new edition is just out, is as fine a specimen of map construction as we have seen. The scale is so large as to admit of exhibiting minute features, and the map not too large to be hung on a wall. It is brought up to the latest date, which is saying a great deal in respect of Africa, and so far as we have tested it, shows everything that such a map ought to do.

OUR agricultural readers would do well to procure a circular issued by the Science and Art Department, South Kensington, giving directions for the collection and forwarding collections of wheat, barley, and oats, the growth of 1877, required to show the variations in quality existing in these descriptions of corn according to the circumstances and conditions influencing their growth. Such a collection is important both from a practical and a scientific point of view.

WE have received a very interesting catalogue of a collection of great interest to archæologists and collectors generally to be sold by Mr. Stevens, of King Street, Covent Garden, at the Alexandra Palace, on Tuesday and Wednesday next. This is the collection known as the Whitfield collection, containing many fine specimens of implements, weapons, ornaments, clothing, &c., from the South Sea Islands and other regions, as well as a number of natural history objects. Those of our readers wishing to form or to complete collections would do well to get a catalogue and attend the sale.

PROF. PIAZZI SMYTH, of the Royal Observatory, Edinburgh, writing to the *Scotsman* under date August 19, 4 P.M., states that in the twenty-seven hours elapsed since the 18th, at one o'clock P.M., the amount of rainfall was 1·349 inch—a greater amount than has been registered at Edinburgh before, within the same length of time, during the present year. "Twice only, on January 1 and July 16, did the day's record just rise above one inch; but each of those days was a Monday's record, summing up a forty-eight hour, in place of the usual twenty-four hour, interval. On each of these occasions, however, of undoubtedly heavy rainfall, as well as the present extra one, the direction of the wind was east. That is not an ordinary direction from which to expect rain, but when it does come from that quarter it has the characteristic, only recently ascertained, of producing a particular band in the prismatic spectrum of sky-light, by which its approach may often be usefully predicted, and by any and every private observer for themselves, even in cases where the barometer may fail."

AMONG the subjects on which papers are to be read during the present session at the Bradford Scientific Association, are—On Colour, by Henry Pocklington; The Structure of Stems, by Mr. J. Abbott; On Grasses, by Mr. W. West; Indigo, by Mr. Whittaker; Pyroxiline, by Mr. J. A. Douglas; Field Geology, by Mr. A. Crebbin.

THE Yorkshire Naturalists' Union paid a visit to Goole Moors recently, where they had a field-day and a general meeting, which appear to have been in all respects successful.

THE leading article in the August number of the *American Naturalist* is an exceedingly pungent address on "Catastophism and Evolution," by Clarence King, who treats the subject with animation and force. Both evolutionists and their opponents will read the article with interest.

WE have received from a Ceylon correspondent an interesting account of the Colombo Museum, which we regret being unable to publish in full. He also sends us a photograph of the museum, which, we believe, is the finest building in the island, not excepting Government House, indeed will compare favourably with similar buildings even at home. This is a work with which the name of Sir W. H. Gregory, who has just completed his term of government in the island of Ceylon, will always be associated. The colony has been increasing in wealth at an unprecedented rate during the last five years, and the governor has done his best to make the intellectual and moral elevation of the people equal their material prosperity. There are few countries where the aids of science are so necessary. There are few countries where those aids have been so greatly neglected. Sir Wm. Gregory saw this, and tried to give to the people themselves those tastes which alone could lead to the proper remedy. With this view the museum was built at Colombo, to be a sort of nucleus for the spread of general scientific education. The Colombo Museum occupies a commanding position in the Cinnamon Gardens, a favourite evening resort. The collection within is a very scanty one, as might be expected from an institution only five months old and in a place where a general taste for science has yet to be cultivated. Most important collections as yet relate to the history, antiquities, and superstitions of the island. A large room is filled with specimens of native manufacture. In the abundant vegetable wealth with which Ceylon has been favoured, the treasures that may lie hidden beneath in its rocks have been treated with comparative neglect. Very little has been done for its geology, as will be evident from a glance at the one glass-case devoted to specimens of Ceylon rocks. We trust, however, that in time a collection will be formed worthy of the building and the island. We ought not to omit mentioning that the museum contains a magnificent collection of snakes (Ceylon) by Mr. W. Ferguson, of Colombo. A catalogue would be of great service and might be made eminently instructive. We hope that the public of Ceylon will soon fill the empty shelves in token of their appreciation of the generosity shown by the Government in giving them a free museum.

WE are pleased to notice that the new building for the Peabody Museum of American Archæology and Ethnology is so far finished as to enable Mr. F. W. Putnam, the Director, to begin work there, and he has now removed the collections forming the Museum from Boylston Hall, where they have been in temporary quarters, to the upper rooms of the new building, which is located near the Zoological Museum, and will eventually form a part of one grand structure. The new Museum is fire-proof, and the building is only to be used for the purposes of the trust, viz., a museum and library (and lecture-rooms eventually) of Archæology and Ethnology. The present portion will cost, when cased, not far from 60,000 dols, and a building fund of 50,000 or 60,000 dols. will still be left for its completion. The original fund for the building was 65,000 dols., and it is proposed always to retain at least 50,000 dols. as a building fund for the future. The present building will supply the wants of the Museum probably for the next ten years. We are also interested to know that the collection of Peruvian articles, obtained about thirty years ago by Mr. John H. Blake, of Boston, and which has been consulted by so many writers on Peru, has just been presented to the Peabody Museum, and will form a valuable addition to the already large Peruvian collection given by the late Prof. Agassiz and his son Alexander.

DR. HORNSTEIN, of Prague, has communicated a paper to the Vienna Academy on the probable connection of the wind with the period of sun-spots. He shows that in Prague, as in Oxford, the average yearly direction of the wind, in the time of minimum to maximum sun-spots, progresses in the direction from south to west, and on the other hand, in the time from maximum to minimum sun-spots, it shows an opposite variation. Dr. Hornstein finds further, that the average wind-strength in Prague likewise exhibits a connection with the eleven-years' period of sun-spots, inasmuch as both phenomena reach their maxima and minima simultaneously. This research is based on 240,000 observations.

THE obvious importance of photography to explorers lends considerable interest to a new process devised by M. Deyrolle, in virtue of which the baggage of an explorer who might wish to carry 300 negative plates measuring 24 ctm. by 18, would only be increased by a weight of six kilogrammes, all included, instruments, plates, developers, and accessories. Glass plates are dispensed with, being replaced by paper coated with a layer of prepared wax, capable of bearing 75° without fusion. The paper is covered with sensitive collodion, prepared so as to retain its properties for two years or more. The development after impression is very simple; into a litre of water is put 20 grammes of citric acid, as much acetic acid, and 3 grammes of pyro-gallic acid; an atom of nitrate of silver is added. The negative is placed in this developer and left in it till the coloration of the image becomes sufficiently intense; then it is passed into a bath of hyposulphide of soda, then washed and dried between leaves of blotting paper. It is then proof against heat and moisture, and may be kept indefinitely in an album. The apparatus itself is so constructed as to be capable of remaining two days in water, even in sea water, without deterioration.

THE most important papers read at the meetings of the Kharkov Society of Naturalists during 1876 are:—"On the Mechanism of the Respiration of Birds," by N. Byeletsky; "On Respiration of Roots," by A. Zaykevich; two entomological papers on the province of Kharkov, by P. Ivanov and V. Yaroshevsky; "On the Arachnidæ *Areneæ*, and on the Conjunction of *Chlamydomonas pulvirulus* and *Stigloclonium*," by L. Reinhard; and the continuation of the "Flora of Ukraina" (*Compositeæ* to *Salsolaceæ*), by K. Gornitsky.

MR. THOMAS S. CAYZER, head-master of Queen Elizabeth's Hospital, Bristol, known as the author of one thousand arithmetical tests and of other approved school-books, has made a complete collection of the principal passages in Latin authors that refer to our island, and editing them with vocabulary and notes, is about to issue the volume through Messrs. Griffith and Farran, as a Latin reading-book, illustrated with many woodcuts and a map, under the title of "Britannia."

THE additions to the Zoological Society's Gardens during the past week include a Slow Loris (*Nycticebus tardigradus*) from India and a Cape Hedgehog (*Erinaceus frontalis*) from West Africa, received in exchange; a Wedge-tailed Fruit Pigeon (*Treron sphenura*) from India, presented by Mr. A. H. Jamrach; an Egyptian Gazelle (*Gazella dorcas*) from Barbary, presented by Capt. J. Graham.

AN ALGERIAN INLAND SEA

AS our readers are aware several schemes have recently been before the public for the creation of an inland sea in North Africa, one of the most ambitious and most impracticable of these being the flooding of a great part of the Sahara. Another scheme which has engaged the attention of the French Government for some time is much more feasible and likely to be attended with good results. The Report of a Commission on the plan proposed by M. Roudaire for the creation of an inland Algerian sea was recently presented to the French Academy of Sciences by M. Favé, and as it contains several points of scientific interest, we propose to lay it before our readers.

Since the French domination was extended in the province of Constantine as far as the town of Biskra, the attention of several observers has been turned to the very marked depressions of the soil, which commence at about 50 kilometres to the south of Aurès, that is, to the border of the Sahara, and extending from east to west. M. Virlet d'Aoust supposed, in 1845, from the measurement of the slope of a river discharging into the Chott (or marshy lake) Mel-Rir, that the bottom of that chott must be below the level of the Mediterranean. In 1849 M. Dubocq, a mining engineer, proved, by a very numerous series of barometric observations, published in 1853, that singular anomaly, which Capt. Vuillemot confirmed in 1856. It was reserved to Capt. Roudaire, to render the fact incontestable and to determine the depth with almost complete accuracy.

After having taken for his starting-point the *embouchure* of one of the two small streams which fall into the sea at the bottom of the Gulf of Gabès, M. Roudaire traversed the steppe of Gabès, 46 metres high, then arrived at the depression of a chott the surface of which he estimated, at sight, at 5,000 square kilometres. He then reached, by crossing a second elevation of 45 metres, that of Kritz, the depression of the Chott Rharsa, situated to the east of the Chott Mel-Rir, from which it is separated only by two elevations of small height. These two slight elevations bound the Chott Asloudj, the surface of which does not exceed 80 square kilometres. The surface of the Chott Rharsa has been estimated at 1,350 square kilometres; that of the Chott Mel-Rir, which has been surrounded by a polygon of levelling, contains 6,700 square kilometres. The three basins which form the Chotts El Djerid, Rharsa, and Mel-Rir have not yet been surveyed in all directions; but M. Roudaire has concluded from various observations that the mean depth of the two Chotts Mel-Rir and Rharsa must not be below 24 metres. The small Chott El Asloudj, which is intermediate, has a mean depth of only from one to two metres, which makes him regard it as a slightly elevated barrier between the two great lakes. If it be admitted that this barrier could be pierced by a trench of suitable depth, and that the water of the sea were led from the Gulf of Gabès to the entrance of the Chott Rharsa, the sea would fill that chott, as also the Chott Mel-Rir, and the depth of water would be sufficient in the two lakes for the navigation of all vessels. Articles of commerce could be transported thence to all parts of the world without any re-embarkation.

Such is the starting-point of a project for an inland sea which M. Roudaire has had constantly in his mind during all his labours: he is confident that the execution is an easy matter, without allowing himself to be discouraged by any obstacle. The enterprise, supposing it to be realised, would certainly not present commercial advantages comparable in any respect to those resulting from the canalisation of the Isthmus of Suez. The products of Central Africa, transported by camels across the desert do not seem to be sufficiently abundant to furnish freight for a large number of vessels. There is no doubt, however, that if the products of Central Africa had no longer to bear the expense of so long a carriage by land, their price would be notably lowered and their consumption increased. But indeed it would be impossible to estimate the benefits which in the future would result from the creation of such an inland sea. Considerations of another kind leave no doubt, M. Favé thinks, as to the improvements which would result from an inland sea covering 13,230 square kilometres, from a climatic point of view and in relation to the fertility of the soil.

Prof. Tyndall was engaged for some years in determining the action which the vapour of water exercises upon radiant heat. He has proved that even with complete transparency to light, the vapour of water absorbs radiant heat to a very notable extent. The vapour of water possesses that absorbent property much more than the air with which it is mixed, in however small a proportion; and its absorbent power increases very nearly in proportion to its mass. Prof. Tyndall has not failed to bring out the influence which the invisible vapour of water contained in the air exercises upon temperature, both during day and night, and he has been able hence to draw immediate conclusions as to its influence upon the life of plants. After having measured directly the quantity of heat absorbed by very minute quantities of vapour of water mixed with air in his experimental tubes, he feels authorised to speak thus:—"Considering the earth as a source of heat, it may be admitted as certain that at least 10 per cent. of the heat which it tends to radiate into space is intercepted by the first six feet of moist air which surrounds its surface." Prof. Tyndall hence draws this conclusion:—"The suppression, during a single night of summer, of the moisture contained in the atmosphere which covers England would be accompanied by the destruction of all the plants which frost kills."

It is not only the cold of night which is increased at the surface of the ground by the dryness of the air, but also the heat of day; so that the variations of temperature produced in twenty-four hours are sometimes very great and very prejudicial to the vegetation of a great number of plants. We may apply these considerations to the region of the chotts, where M. Roudaire, in his expedition of 1874-5, found heat of 25° (C.) during the day, and cold of 8° below zero during the night. After that we need not be longer surprised that the lands comprised between the slopes south of Aurès and the chotts produce very little, however favourable in themselves they may be to vegetation. If we admit with M. Roudaire, agreeing in this point with all explorers of the chotts, that their cavities have at one time formed salt lakes, dried up gradually during the historic period, we shall obtain an explanation of the changes in the production of the soil of the province of Constantine, and of Tunis since the epoch of Roman domination, when the province of Africa was much more populous and much more fertile than at present.

M. Roudaire has sought to find results of observations from which he might conclude what would be the depth of the bed of water evaporated after the creation of the inland sea. He has found this information in the experiments made at the Bitter Lakes traversed by the Suez Canal. At the time of the filling-up of the Bitter Lakes, a waste-weir was constructed intended to regulate the introduction of the water of the Mediterranean. From July 7 to 14 the weir was wrought with only a small number of sluices raised, and the level of the lakes remained stationary. The introduction had been regulated to about 3,540,942 cubic metres, or, in round numbers, 4,000,000, cubic metres per day. This figure, then, gives the quantity of water absorbed by evaporation which, according to the extent of surface, produced a lowering of the level of from ·003 m. to ·0035 m. during twenty-four hours, and that in the hottest month of the year. All the observations made since that time have given essentially the same results, and we must admit, with the engineers of the Suez Company, a general mean of ·003 m. per day, or 1 m. per year. M. Roudaire has added, as a conclusion to be drawn from this bearing on his project:—"The basin of the chotts and the Isthmus of Suez being situated nearly under the same latitude, and possessing a climate absolutely analogous, we must admit that the evaporation which will be produced on the inland sea will be the same as that which has been observed on the Bitter Lakes. The figure ·003 m. is the general mean of the year. The observations which we have made in the chotts with Piche's evaporometer have proved to us that this figure is at least doubled during the sirocco."

Not only would the vapour of water thus diffused through the air serve as a reservoir for the heat emanating from the earth or the sun, but it would have still another mode of action for effecting climatic modifications. The air and its vapour brought into contact with the elevated and therefore cool parts, the Aurès mountains, and other mountains of Algeria, would have their temperature lowered on account of that cause, and the effect would be increased by the radiation of the vapour of water into space; for that radiation would operate almost without check at a height where the air from above, and therefore less dense, is cold and dry. Under the influence of this double cause the moisture would be condensed into rain or snow, and would serve to feed the watercourses which would permanently flow in the beds at present dry during a great part of the year. We should see issuing from the ground, from the same cause, sources which do not now exist. The moisture, discharging itself along the lines of watercourses, would extend its influence on the two slopes of the mountains to countries at a distance from the chotts. We can perceive by calculations the volume and the weight of the masses of water set in motion by evaporation, that these considerations are not chimerical. The 13,230 square kilometres give 39,690,000,000 kilogrammes of water per twenty-four hours, raised by evaporation, *i.e.*, 39,690,000 cubic metres. It will be seen that there is here something to form sources and feed streams or rivers. M. Roudaire has calculated that the quantity of vapour diffused in air whose barometric pressure is ·760 m., and the temperature 12° C., would cover the surface of Tunis and Algeria with a layer of half-saturated air, 24 metres in height. Let us remark that this calculation includes only the quantity of vapour formed during twenty-four hours. The south wind known as the *sirocco*, at present so destructive because it is exceedingly dry, would produce on the surface of the lakes an evaporation much greater than that mean, and would, moreover, lose many of its hurtful effects. In fact, this same wind, which destroys the vegetation of Algeria, has a fertilising influence on the territory of France, because of the moisture with which it becomes charged in crossing the Mediterranean.

Advantages so considerable, which would result from the introduction of the water of the sea into the chotts, explain and justify the perseverance with which M. Roudaire has pursued the idea without allowing himself to be arrested by any of the difficulties which have presented themselves. The greatest of the difficulties, M. Favé thinks, proceeds from the fact that the Chott El-Djerid, the nearest to the Gulf of Gabès, has not, like the others, the bottom of its basin below, but above, the level of the sea. The surface of the ground is undulating; it rises to 20 metres, or even more, at certain points, and descends to zero at other points. M. Roudaire has estimated, somewhat vaguely, that the mean height of the bottom may be about 6 metres above sea-level. Notwithstanding this obstacle, M. Roudaire does not renounce the hope of being able to make the water of the sea reach the Chott El-Djerid in order to turn it afterwards into the other two chotts. He believes he has found a support for this in the nature of the bottom, or, to speak more exactly, in the existence of a water-bearing bed situated at a small depth below the ground.

The Commission, of which M. Favé is the mouthpiece, without pronouncing definitely on the project of M. Roudaire, sufficient data for this not being forthcoming, strongly recommend that active steps be taken to obtain more accurate measurements and other data. The facts which he has adduced they think sufficient to justify serious attention being paid to his proposal, and recommend that the thanks of the Academy be accorded to M. Roudaire for his valuable labours. To these recommendations the Academy agreed.

We should state, however, that MM. Dumas and Daubrée, members of the Commission, are not able to give their entire consent to the recommendation of M. Favé's report. They think that the obstacles to the accomplishment of the scheme are much more serious than have been estimated, and regard the industrial and climatic results anticipated as, to a considerable extent, hypothetical. M. de Lesseps, however, gives his entire concurrence to the scheme of M. Roudaire, and believes in its practicability and the favourable results that would follow its realisation.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

SCIENCE AND ART DEPARTMENT.—The Committee of Council on Education have just issued an important circular on instruction in practical chemistry and in physics. My Lords direct that §§ XLV. and LXXI. of the Science Directory be cancelled and that the following rules be substituted:—1. Payments of 1*l.* 10*s.* and 1*l.* for the first and second class in the elementary stage, and of 4*l.* and 3*l.* for the first and second class in the advanced stage and honours, will be made on the results of instruction in practical chemistry. They will be claimable according to the same rules, and subject to the same deductions on account of previous success as the ordinary payments. These payments will be made on condition—(*a*) That there be a good laboratory—being a room, or part of a room, exclusively devoted to the purpose of the study of practical chemistry—properly fitted with gas and water supply. (*b*) That the student on whom the payment be claimed have received twenty-five lessons at least in laboratory practice since his last examination, each lesson being an attendance of at least one hour and a half's duration on a separate day. (*c*) That a register of the attendance of the students at the instruction in practical chemistry be kept duly posted up from day to day. 2. *Elementary Stage.*—In this stage the knowledge of the students will be tested by special questions set with the ordinary examination paper; but no payments will be made if the laboratory be not furnished with all the apparatus necessary for the individual practice of each student in practical chemistry, and if systematic instruction in practical chemistry be not given. Any student on whom it is intended to claim payments in this stage may be called on by the Inspector of the Department, when visiting the laboratory, to repeat some of the experiments, specified in the Science Directory in the syllabus for the first stage of inorganic chemistry, which he has had an opportunity of witnessing. 3. *Advanced Stage and Honours.*—The results of the instruction in these stages will be tested by a special examination in qualitative analysis to be held on a Saturday during the ordinary May examinations, and lasting, for the advanced stage from 6 P.M. to 10 P.M., and for honours from 2 P.M. to 10 P.M. Payments can only be claimed in these stages provided—(*a*) That the laboratory be fitted up with a separate working place for each student. (*b*) That each student be provided with a complete set of apparatus and chemical tests (as enumerated in Science Form No. 402) kept separate, and in good working order, on the shelves, and in the cupboard or drawers at his own table. (*c*) That the laboratory be also furnished with apparatus for general use, consisting of at least the articles of which a list will be found on Science Form No. 402. From the reports of the examiners and of the inspectors it appears that instruction still continues to be given in physics without a sufficient amount of apparatus to illustrate the teaching of these experimental sciences. My Lords cannot allow examinations to be held in schools where instructions of such a superficial and perfunctory nature is given. They therefore direct that in 1878 no classes be examined which are not furnished with apparatus at least sufficient to illustrate some of the more important experiments; which apparatus the teacher may be called upon by the Inspector of the Department to show his ability to use.

BRISTOL.—From the prospectus for session 1877–78 of University College we are glad to see that that institution is rapidly attaining a position to afford a complete education both in literature and science. The chairs of chemistry, experimental physics, and botany are now filled up, and as the other branches of physical science are down in the programme of the coming session, no doubt professors for them will soon be appointed. The medical school in connection with the University is now fully organised, and we are confident that ere very long Bristol will become one of the chief centres of University education in the kingdom. A very satisfactory report has been presented to the London Clothworkers' Company on the chair of Technical Education founded by funds provided by them.

A NORTHERN UNIVERSITY.—At a recent meeting of the Leeds Town Council a deputation from the Yorkshire College of Science waited upon them to urge them to take steps to obtain Government sanction to found a university for the northern counties of England. This step was undertaken in consequence of the action of Owens College to obtain a charter for the erection of that institution into a university. The Leeds Town Council drew up a memorial to the Privy Council, in accordance with the prayer of the petition, and the Parliamentary Committee was instructed to watch the further progress of the matter.

SYDNEY.—The University of Sydney has applied to the Colonial Government for an increase of endowment from 5,000*l.* to 9,000*l.* With this increased income the university would add, among other subjects, to its present course, all the education necessary for the medical profession, a complete course of natural philosophy, coupled with mechanics and engineering, the addition of organic chemistry and metallurgy to the chemical school, and biology. The salaries attached to these chairs would be 1,000*l.*, with assistants at 250*l.* each. The proposal is still under the consideration of the government, but we cannot doubt, if they have the best interests of the Colony at heart, they will grant the petition of the University.

SCIENTIFIC SERIALS

American Journal of Science and Arts, July.—Contributions to meteorology, being results derived from an examination of the United States Weather Maps and other sources, by E. Loomis.—Germination of the genus *Megarrhiza*, Torr, by A. Gray.—The absorption of bases by the soil, by H. P. Armsby.—Double-star discoveries with the 18½-inch Chicago refractor, by S. W. Burnham.—Relations of the geology of Vermont to that of Berkshire, J. D. Dana.—On certain new and powerful means of rendering visible the latent photographic image, by M. Carey Lea.—On the possibility of transit observation without personal error, by S. P. Langley.—Observations of comets made at the Litchfield Observatory of Hamilton College, by C. H. F. Peters.—On complex inorganic acids, by W. Gibbs.

Annalen der Physik und Chemie, No. 6, 1877.—On the electric currents which arise in the flow of liquids through tubes, by M. Edlund.—On metallic reflection, by M. Eisenlohr.—Contributions to an adequate determination of the plane of vibration of polarised light, by M. Ketteler.—On electric induction on non-conducting solid bodies, by M. Wüllner.—On the thermo-electric properties of gypsum, diopside, orthoclase, albite, and pericline, by M. Hankel.—On the magnetic behaviour of nickel and cobalt, by M. Hankel.—On the relation of friction of gases to temperature, by M. Puluj.—On electric smoke figures, by M. Antolik.—Apparatus for determination of the focal distance of spherical lenses and lens systems, by M. Meyerstein.

Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, t. xxiii., No. 4.—Eighty-six silver coins with Pehlewy inscriptions, by M. Dorn.—Observations of planets at the Academic Observatory of St. Petersburg; determination of the inclination of the orbit of the planet Neptune to the ecliptic, by M. Sawitsch.—Influence of depressor nerves on the quantity of the lymph, by M. Velıky.—Influence of temperature on the galvanic resistance of Siemens wires, by M. Lenz.

Archives des Sciences Physiques et Naturelles, July.—Cretaceous fauna of the Rocky Mountains, by M. Delafontaine.—On chemical equivalents and atomic weights as bases of a system of notation, by M. Marignac.—Observations on some fossil plants of South Tessin and on the deposits which contain them, *àpropos* of the glacial controversy, by M. Sordelli.—On the relations between the intensity of irritation of the sciatic nerve, the height of the muscular contractions, and the time elapsing between irritation and contraction, by M. Lautenbach.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti. Vol. x. Fasc. VII.—Two new parasitic mycetes on vines, by M. Cattared.—On a cause little estimated in pathogenesis of some female diseases, by M. de Giovanni.—The molecular velocity of gas and the corresponding velocity of sound, by M. Brusotti.

Fasc. XII.–XIV.—On more economical composition of electromotors capable of a given effect, by M. Ferrini.—Experimental researches on heterogenesis (second paper), by MM. Cantoni and Maggi.—On the existence of monera in Italy, by M. Maggi.—On a particular reaction of saliva, by M. Solera.—On the state of sulphur in milk and on the normal existence, in vaccine milk, of sulphates and sulphocyanates, by M. Masso.—On a Selachian recently caught in the Mediterranean, by M. Pavesi.—On a new differential function in the theory of elliptic functions, by M. Brioschi.—On differential equations, by M. Casorati.—Quali-quantitative researches on carbonic anhydride, by M. Pollacci.—*Résumé* of meteorological observations at Milan in the Brera Observatory in 1876, by M. Frisiani, jun.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 21.—"The Relationships of the Nerve-cells of the Cortex to the Lymphatic System of the Brain." By Bevan Lewis, F.R.M.S., Pathologist and Assistant Medical Officer at the West Riding Asylum. (Communicated by Dr. Ferrier, F.R.S.)

The anatomical relationships of the nerve-cells of the cortex to their immediate environment, and especially to the surrounding lymphatic structures, is a subject of such weighty importance to the pathologist and physiologist that too much consideration cannot well be paid to what must necessarily be involved in the solution of those mysterious problems connected with the statics and dynamics of the brain. The author of this paper has detailed the results of personal investigations, in which he has been able to confirm the observations of Obersteiner.[1] He alludes to the confusion on this subject traceable in the writings of several English histologists, some of whom, whilst recognising the existence of peri-cellular spaces, do not attempt an explanation of their significance, others openly express their dubiousness with regard to their import, whilst a limited class regard them as morbid productions due to the atrophy and shrinking of the nerve-cell. His attention was first attracted to their significance by (*a*) "the presence in certain morbid conditions of numerous nuclei arranged in definite directions around the nerve-cell, (*b*) the presence of undoubted lymph-corpuscles in clear spaces around the nerve-cells, and (*c*) the appearance of peri-cellular spaces in healthy brain occasionally when the cells appeared perfectly normal, and certainly not atrophic."

This disposition of nuclei (*a*) is most strikingly evident around the nerve-cells of the third layer, and around the still larger cells found at a lower level in the ascending frontal and parietal convolutions of man which have been termed "giant-cells." These "giant-cells," the hypertrophied cells of some writers, are stated by Mr. Lewis to be undoubtedly *normal*, and to a great extent *constant*, elements in these regions. In order to appreciate the significance of this arrangement of nuclei, the non-nervous elements of the cortex are considered, allusion being made to the proliferation of connective elements so frequently met with. These latter are shown not to be *free* nuclei, but to have a delicate investment of protoplasm around them. The non-nervous cellular or nuclear elements are described as disposed in three definite situations: (*a*) irregularly in the neuroglia network; (*b*) regularly around the nerve-cells; (*c*) following directly the course of capillaries.

In the two last positions they are shown to be connected with the lymphatic channels and sacs surrounding the blood-vessels and nerve-cells, and the author regards them as originating in the endothelial elements of these structures. The spindle-cells of the deepest cortical layer in the frontal region are said to be peculiarly prone to the growth around them of these attendant satellites. He continues: "The recognition of these connective and endothelial elements, and the varying conditions imposed upon them by their distinct functional endowments is of essential import when we are dealing with the morbid brain." The peri-cellular sac is then described fully, as well as its varied contour dependent upon the form of the inclosed cell, method of preparation, thinness of section, and the various physiological and pathological conditions existing before death. The close proximity of a capillary to these sacs was invariably observed, and on close examination a connection betwixt the peri-vascular and peri-cellular sheaths was clearly seen. Sections of the cortex in new-born animals were then described, in which a linear arrangement of the cells along the peri-vascular sheaths was observed, each nerve-cell being separated by a clear space from the surrounding neuroglia, the peri-vascular sheaths in the kitten being widely distended. The nerve-cells in these cases were pyriform, and apparently connected to their limiting sacs by a narrow stalk-like process. The writer next dwells briefly upon the developmental bearing of these facts. With regard to the explanation afforded by some observers of the significance of these spaces in senile atrophy, it is shown that whilst the large size and defined contour may be due to shrinking of the degenerated protoplasm of the nerve-cell, "yet the important point is to recognise these spaces as natural structures in an unnaturally distended condition, for their large size appears to me to be due not only to wasting and recession of the inclosed cell, but to a large accumulation of lymph, the lymphatic channels, peri-cellular and peri-vascular, being in a distended condition throughout." It is next shown how readily the lymph current may be obstructed in its flow towards the pia-mater, and how seriously such conditions would affect the nutritive and depurative changes proceeding in the lymph-sac—changes of so vital an importance in the maintenance of the functional activity of nerve-cells. The methods employed in this investigation by Mr. Lewis, include ordinary chrome hardening, the teasing process described by him in the *Monthly Microscopical Journal*, and the examination of films of cortex obtained by his new freezing microtome. His paper is illustrated by six drawings of the microscopic structure of the cortex.

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[1] "Ueber einige Lymphräume im Gehirne" (Sitzb. d. k. Akad. d Wissensch. 1 Abth., Jan. Heft, 1870).

PARIS

Academy of Sciences, August 13.—M. Peligot in the chair.—The following papers were read:—Communication from the Bureau des Longitudes on new operations of astronomical geodesy, by M. Faye. This relates to the astronomico-telegraphic junction of Paris with Neuchatel, Geneva, and Lyons.—Engraving representing the aureola of Venus as seen from the Island of St. Paul, by M. Mouchez. The phenomena is given at three different stages.—A general law of geometric curves concerning the common intervention of each point of a curve and the tangent of this point, in questions of geometrical positions or enveloping curves, by M. Chasles.—New considerations on the localisation of cerebral centres regulating the co-ordinated movements of written language and articulated language (continued) by M. Bouillaud. He replies to some recent objections by Dr. Fournié against localisation of speech in the left cerebral hemisphere.—On the reproduction, by photography, of the rice-grains of the solar surface, by M. Janssen. He has succeeded in this in his solar photographs of thirty centimetres by means of a very short exposure, combined with strong development.—On an example of reduction of Abelian integrals with elliptic functions (continued), by Mr. Cayley.—On the best conditions of employment of galvanometers, by M. du Moncel. He gives an experimental verification of some mathematical deductions.—Note on the central obturator inflamer, by M. Cosson. The state of dryness of the powder seems to have (other things equal) an exceptional importance for the author's apparatus. The Pyrenees mark the true line of separation between the eocene and miocene portions of the tertiary epoch, by M. Leymerie.—A message of sympathy was sent to M. de Lesseps on account of his recent accident.—The system of Sirius, by M. Flammarion. The orbit calculated for the companion of Sirius differs from the orbit observed; the latter crossed the former in 1869 (having left it in 1862) and going beyond it, has followed quite a different curve, wider and less eccentric. M. Flammarion supposes that either the companion will accelerate its motion and return to the west in 1892, or that there is another disturbing body nearer and more rapid, not yet discovered.—Remarks, *à propos* of M. Faye's communication on the relation between the sun-spots and variation of the magnetic declination, by M. Wolf. The anomalies of the one class of phenomena are reproduced in the other, a strong evidence that both are produced by the same cause.—On the equation of Riccati, by M. Genocchi.—Note on the curves which have the same principal normals, by M. Niewenglowski.—On the slipping (*patinage*) of the wheels of locomotives, by M. Rabeuf. The phenomenon is much more general and complex (he finds) than is commonly supposed. The slipping is almost *nil* in ascending an incline, and very pronounced in descending. It increases rapidly with the speed, but appears to be greater, with equal velocity, on descents than on ascents. In descents it varies between 13 and 25 per cent. Its suppression, if possible, would realise a corresponding economy in fuel and wear of machinery.—The *régime* of the winds, and evaporation in the region of the Algerian chotts, by M. Angot. It is shown from figures that the winds favourable to M. Roudaire's project (viz., south, south-west and south-east) are to the unfavourable winds in the ratio of 1 to 9·4. Their vapours would be almost wholly carried towards Sahara instead of Algeria. The average layer of water estimated as removed in twenty-four hours from the projected sea is about 6 mm. This would raise to seventy-eight millions of cubic metres the quantity required to be brought by the canal of communication daily to keep the lake-level constant.—On the vapour of hydrate of chloral, by M. Troost. A second method (for determining equivalent in volume) consists in first vaporising hydrate of chloral, then introducing into the vapour a body capable of removing part of the free vapour of water it may contain (pure neutral oxalate of potash was used completely dehydrated in a stove at 100°). This method led to the same conclusion as the first, viz., that hydrate of chloral exists in the gaseous state, and so that its equivalent corresponds to eight volumes.—Note on some properties of sulphide of cadmium, by M. Ditte.—On some general properties of metallic sulphides, by MM. de Clermont and Guiot. The decomposition (here proved) of sulphides by water at 100° with formation of metallic oxide and sulphuretted hydrogen, is thought a fresh reason for regarding hydrogen as a metal; it displaces true metals in these reactions, and forms a more stable sulphuretted compound.—On some points of the organisation of Bryozoa, by M. Joliet.—On the fecundation of Echinoderms, by M. Giord.—Pyrophosphates in therapeutics; their mode of action, by MM. Pacquelin and Joly. Pyrophosphates, far from being reconstitutive, as commonly supposed, are purely foreign bodies for the system, and their ingestion can only increase its expenditure in the work of elimination, which their presence necessitates. Any reconstitutive value attached to them is probably due to impurities.—On the physiological action of Pau Pereiro (*Geissosperrmun læve*, Baillon), by MM. Bochefontaine and De Freitas. The active principle is a paralysing poison, which appears to abolish the physiological properties of the central grey nervous substance, and especially of the grey bulbo-medullary axis.—On anthracic *bactéridies*, by M. Toussaint.

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THURSDAY, AUGUST 30, 1877

THE ZOOLOGICAL RECORD

The Zoological Record for 1875; *being Volume Twelfth of the Record of Zoological Literature.* Edited by Edward Caldwell Rye, F.Z.S., M.E.S., &c. (London: John Van Voorst, Paternoster Row, 1877.)

ZOOLOGISTS, it must be said, are a somewhat ungrateful set of men. There can be no question of the extreme value to them of "The Zoological Record," and yet they allow, as is notorious, the Association which was formed to continue that useful publication to lead a precarious existence, dependent on the charity of various other scientific bodies. This is not creditable to the zoologists of our own country, nor, though in a less degree, to those of our colonies, past and present. Those of the United Kingdom are unquestionably wealthy as a class, but their wealth is very unequally divided. Many, and among them we are glad to say are some of the best, are amateurs who follow the study simply for their own pleasure, and are sufficiently blessed with this world's goods. Yet they see that many of their brethren have need, without taking any trouble to help them. The number of "literary and scientific" institutions, museums, reading-rooms, and the like, throughout the country, is very great, and there can be scarcely any of them that does not possess one or more members who take an interest in zoological pursuits. But how few of these institutions and so forth, are there on whose bookshelves "The Zoological Record" is to be found! Surely but a very slight amount of exertion is required on the part of such members to get this work taken in by the institution to which they belong? As a rule the library-committees of such bodies are not averse to books of reference, and here is one that is absolutely necessary to every student of or worker in zoology. It is no secret that the Zoological Record Association has the greatest difficulty in "making ends meet," and a very moderate amount of the support we have above indicated would go far to remove the difficulty, and to prevent the possibility of indelible disgrace accruing to the zoologists of this country, by the cessation of this useful annual.

The volume for 1875, which has just been issued, forms the twelfth of the series, and maintains the high character of its predecessors. It must be very satisfactory to Mr. Rye, as editor, to find himself supported by so strong a band of Recorders. Yet the *personnel* is, with one exception, entirely changed from that which was first enlisted by Dr. Günther. The exception is Dr. Eduard von Martens, who, with truly Teutonic tenacity, continues his labours on Molluscs and Molluscoids—nay more, since he first began, he has added the Crustaceans to his cares. The Mammals are in charge of Mr. Alston; the Birds have fallen to the lot of Mr. Salvin. Reptiles and Fishes are taken by Mr. O'Shaughnessy; Arachnids and Myriopods by Mr. Pickard-Cambridge. The editor himself bears the brunt of the battle; not only does he (as becomes one of the staff of the *Entomologists' Monthly Magazine*) look after the Insects as a general subject, but he also takes specially the orders, *Coleoptera*, *Hymenoptera*, *Diptera*, and *Rhynchota*—leaving the *Lepidoptera* to Mr. Kirby and the *Neuroptera* and *Orthoptera* to Mr. McLachlan. Dr. Lütken sweeps all the remaining groups into his net. The services of each of these gentlemen deserve the most conspicuous acknowledgment. Breaking stones on a road is the common expression for employment which combines the dullest and hardest handiwork with the lowest wages. Where mental labour is concerned it may be paralleled by the vocation of a Zoological Recorder. His task is certainly not more easy or exhilarating and he is not requited at a higher rate. There is scarcely one of the whole of these gentlemen, we are convinced, who does not enter upon or continue his occupation simply because he conceives it to be his duty—and his chief reward must be the satisfaction he receives from discharging it to the best of his ability—for it were absurd to call the miserable pittance, which is all that the Association can afford to dole out to him, any remuneration for the hours of weariness which the due execution of the Records requires.

So strongly do we feel the self-denying nature of the work done by the Recorders that we cannot find it in our heart to criticise any portion of it. There is, we think, and it is only to be expected that such should be the case, a marked difference in the execution of the several Records, and one that is not to be accounted for by experience or want of it on the part of their authors. The worst will bear favourable comparison with anything of the kind published elsewhere, and the accuracy of the references is quite beyond praise—for we could name at least one work of similar nature to consult which is often to follow a guide who either did not know or had forgotten the path. More than this—despite the difference of treatment of which we have spoken—there is a wholeness about the work that bespeaks an eminently able editor.[1]

The index to the genera and sub-genera recorded as new in this volume includes nearly *one thousand* names, and the excellent plan (first introduced, we believe, by the late Mr. Crotch) of indicating those names which have been already preoccupied in zoology is still continued. Taking a most merciful view of what constitutes a synonym, the editor has yet to mark fifty-nine of these names (thereby implicating thirty-seven authors) as used before—a far greater proportion than there of course ought to be. Some zoologists in conferring new names evidently pay very little attention to their predecessors' labours, and hence scientific nomenclature is encumbered by these unnecessary terms. One gentleman, indeed, seems oblivious of his own success in genus-making, and apparently has bestowed the same name on what he considers to be two distinct genera within a dozen pages of the same work! This is M. Mulsant, and he stands out as the greatest sinner in this respect. By himself he is guilty of making *three* synonyms, and in conjunction with M. Rey of *three* more. Messrs. Chambers, Schneider and Signoret are each responsible for *four*, Messrs. Chapuis and Reuter, for *three*, and Messrs. Boisduval, Chaudoir, Harvey and C. G. Thomson for *two*. The twenty-five who have committed this crime only once we need not name, and of course it is possible that in some cases their position is defensible, though in reality little is really

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[1] Mr. Rye is so uncommon an editor that we believe he will be grateful to us for having a misprint detected—and it is the only one of importance that, after some study, we are able to point out. *Cyrrhophthalmus* (p. 259) should surely be *Cyphophthalmus*.

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to be urged on behalf of so confusing a practice. When a man has a new genus to describe it should be his first duty to take care that he does not apply to it a name that has been proposed before, and it is not generally difficult to find this out. Of course the punishment ultimately falls on the offender's own head, for in these days somebody is sure to discover the blunder, and generally before long, but meanwhile the inconvenience may be and often is not inconsiderable.

In conclusion, we have but to wish the Zoological Record Association an increasing sale for their useful annual, and to express our thanks to Mr. Rye and his assistants.

ARCTIC METEOROLOGY

Scientific Results of the United States Arctic Expedition Steamer "Polaris," C. F. Hall commanding. Vol. I. Physical Observations. By Emil Bessels, Chief of the Scientific Department, U.S. Arctic Expedition.

THE United States Government has, with its accustomed liberality to science, published in a bulky volume of about 1,000 pages, under the auspices of the National Academy of Sciences, the results of the various observations of meteorology, astronomy, and magnetism, made by the scientific staff of the *Polaris* during the expedition to the Arctic regions in 1871-73. In the present notice we shall refer only to the barometric observations, and the discussion of them, which occupy altogether forty-three pages of the volume before us.

The barometric observations were made hourly at Polaris Bay, 81° 36′ lat. N., 62° 15′ long. W., from November, 1871, to August, 1872, and at Polaris House from November, 1872, to May, 1873, and they are published *in extenso* in this volume. These observations we have examined, and it is evident that they have been made with great care, and that, taken as a whole, they form one of the most valuable repositories of facts which we possess illustrative of the meteorology of the Arctic regions. The errors which do occur are of that class which may be regarded as "inevitable" in such a record of observations, viz., typographical errors, transposed or changed figures, and personal errors of observation which are well known to meteorologists, and admit of easy detection and correction.

On turning to the table of the mean hourly values for the different months (p. 18) calculated from the data just mentioned, we are at once struck with the extraordinary character of the hourly curves as disclosed by these figures, inasmuch as they show a repeated abruptness of change and a capriciousness of form which certainly could not be accepted unless on the clearest proof that they represent well-ascertained facts.

In examining the mean hourly values for December, 1871, the first month for which complete observations were made, it is seen that the calculations made from the individual observations are all correct. If we, however, take the trouble to critically examine the observations themselves from hour to hour, it is seen that there occur two uncorrected readings of 29·371 and 29·777 inches, instead of 29·571 and 29·577 inches, and twelve uncorrected readings in which the observers, as occasionally takes place with the best observers, have read the instrument 0·050, 0·100, or 0·150 inch either too high or too low. Correcting, then, these observations, and calculating afresh the hourly values, we obtain the result given in the following table (columns A.), to which are added the hourly values as printed in the volume (columns B.) :—

	A.	B.		A.	B.		A.	B.
hour.	inches.	inches.	hour.	inches.	inches.	hour.	inches.	inches
midnt.	29·759	29·759	8 A.M.	29 754	29·749	4 P.M.	29·749	29·749
1 A.M	·760	·760	9 ,,	·752	·749	5 ,,	·750	·748
2 ,,	·765	·765	10 ,,	·751	·752	6 ,,	·750	·750
3 ,,	·764	·764	11 ,,	·749	·756	7 ,,	·750	·750
4 ,,	·761	·761	noon.	·743	·740	8 ,,	·745	·741
5 ,,	·760	·756	1 P.M.	·742	·740	9 ,,	·738	·743
6 ,,	·760	·757	2 ,,	·744	·744	10 ,,	·735	·735
7 ,,	·759	·756	3 ,,	·748	·750	11 ,,	·734	·734

Thus, from not submitting the observations to a preliminary critical examination before calculating the averages, half of the resulting averages are faulty, and a monthly curve is obtained which completely fails to represent the physical datum for the ascertaining of which this elaborate set of observations were carried on in all the rigours of an arctic winter.

We are the more desirous of urging this matter on the attention of meteorologists, because the same method of hasty and ill-advised discussion of barometrical observations is widely practised; and, it need scarcely be added, results in the publication of generally accepted averages, which more than anything else are seriously obstructive to any real progress in this intricate but vitally important branch of physical inquiry.

The observations for June, 1872, are free from these errors of observation, but notwithstanding this the hourly monthly values which have been deduced from them do not appear to be satisfactory. On calculating, then, the monthly values from the observations of this month, it turns out that only one of the twenty-four means is correct, the other twenty-three being more or less seriously in error. It is to be regretted that the hourly means for the other months of the period also are so much and so frequently in error, those for December, 1872, for instance, giving a curve which in its essential points is the reverse of the correct one, that the whole of the elaborate discussion of the barometric observations made by the scientific staff of the *Polaris* Arctic Expedition must be rejected.

The averages for the different months have been deduced in two ways, viz., from the twenty-four hourly means, and from the thirty or thirty-one daily means of the month. These two sets of averages would of course agree if the calculations were correct. In the printed tables they are made to agree even to the thousandth part of an inch, by simply placing the calculated average of one column under both columns. Thus the monthly average of June, 1872, is, as deduced from the twenty-four printed hourly means 29·888 inches, and as deduced from the thirty printed daily means 29·860 inches, but in the tables 29·888 inches is printed as the mean of both columns. It is thus evident that the reduction of this very important series of barometric observations requires yet to be made—a work which we hope will be yet undertaken, particularly since the summer and the winter means we have computed seem to suggest important connections between these arctic barometric curves and the curves of lower latitudes.

OUR BOOK SHELF

La Théorie Hugodécimale; ou, La Base scientifique et définitive de l'Arithmologistique universelle. Par le Cte. Léopold Hugo. (Paris, 1877.)

M. HUGO continues to pour forth his pamphlets with their polyglot inscriptions. On this we have "Urbi et orbi. Hic tandem triumphaliter fulget REGULARITAS!" "La pan-imaginarité Hugomathique: CONTINUITAS! CONTINUITAS! TRICONTINUITAS!" We have, in a former notice, glanced at the "Géométrie Hugomoïdale." The object of the present pamphlet is "à vulgariser et à répandre dans les diverses régions civilisées de l'ancien et du nouveau monde, Tou-Kieou, Tchong-Kouo, Fou-Song, &c., &c., cette haute doctrine philosophique, qui, dans sa concision, mérite assurément une place aux premiers rangs de la Philosophie Scientifique. Mais, dans mon isolement de simple philosophe, force me sera d'employer les combinaisons les plus étranges, et de frapper l'attention du lecteur par la singularité même de mon exposition."

One or two extracts must suffice to show how our author proceeds:—

"Évocation Chino-Tibétaine. Nous, suprême Grand Lama, voulons reproduire pour tous l'opération magique hugodécimale.

"Salut! Salut!

"En notre Divan sacré de Hlassa-Potala, parfumé de nuages d'encens, nous étendons la main gauche en désignant et déterminant un point dans l'espace ambiant.

"Salut! Salut!

"De notre main droite étendons le sceptre, pan-scientifique et sacré, sur ce point de l'espace. Salut! salut!

"Apparaît une figure enveloppant le point considéré: figure offrant quatre faces pareilles.

"O Saints Lamas, placez pieusement sur notre tapis drécieux, le premier solide que l'espace régulier vient d'enfanter.

"SSÉ—MIÉ (with figure of tetrahedron).

"Salut! Salut!"

And so on through the ten solids, of which we gave an account in our earlier notice.

The third chapter is taken up with the "Géometrie Pan-imaginaire" and the "Arithmétique Pan-imaginaire," communications made to the Société Mathématique de France, and which we have previously described.

Sufficient notice has been taken of this *brochure* of thirty-two pages, with many figures.

To some of our readers our remarks will serve as a beacon; those who like to secure oddities may perhaps be induced to add this to their stock. Our principal reason for yet noticing another effusion of our author is that we have at last got a notice of him from one of his own countrymen, who calls him "Sans contredit, dans le domaine des sciences, l'un des plus actifs *novateurs* de l'époque" (M. Gérono, *Nouvelles Annales de Mathématiques*, Juin, 1877, pp. 278-280). Like ourselves, M. Gérono confines himself to *extracts*. In his *avertissement* M. Hugo bursts forth with "Écrasons les pan-routiniers! qu'ils tremblent, blottis dans leur petite science, devant l'ouragon hugomatique!" Upon this the French reviewer well remarks:—"Mon avis est qu'il ne faut écraser personne, et que les philosophes réformateurs doivent se garder de prendre l'exaltation des idées pour le sublime des idées. Ce n'est pas sans danger qu'on se lance dans la voie des réformes avec un enthousiasme qui, dans sa marche ascendante, pourrait s'élever jusqu'au délire." The writings of such a visionary perhaps hardly merit a notice; we are disposed henceforth to let him go his own way, trusting that time will clear up many, if not all, of his crotchets.

Mechanik der Bewegungen der Insektenfressenden Pflanzen. Von A. Batalin.

WE have here a record, reprinted from the pages of *Flora*, of a very careful series of experiments on the cause of the "spontaneous" movements of the glands of *Drosera* and other similar organs when irritated say by contact with a fly. Comparing the well-known explanation given by De Vries and others of the movements of tendrils—that contact causes an acceleration of growth in the organ, not on the side touched, but the opposite side, and consequently a concave curving round the touching object—Batalin offers the same explanation of the curvature of the tentacles of *Drosera* when irritated by a fly, viz., an acceleration of growth on the side opposite to the one touched, and in consequence a concave curvature. While admitting the care with which Batalin has performed his experiments, we fail to see how his explanation accounts for some of the well-known phenomena of these singular plants; as, for instance, the fact vouched for by several observers, that glands which are not themselves irritated exhibit the same concave curvature as those that are, and especially those so circumstantially described by Darwin as to the extreme sensitiveness of the tentacles of *Drosera* to the most dilute ammoniacal solutions, while they are quite insensitive to pure water. The "spontaneous" curvature Batalin believes to be a function of growth, and to be displayed in proportion to the faculty of growth possessed by the organ.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Relations between Sun and Earth

PERMIT me to correct a slight misapprehension in Mr. Archibald's very interesting article on the Indian rainfall in NATURE, vol. xvi. p. 340. Mr. Archibald speaks as if my discovery regarding the coincidence of the increase and decrease of the Madras rainfall with the cycle of sun-spots applied to "the whole of Southern India." Now, on the contrary, I guarded against such a generalisation by a sentence expressly inserted for that purpose. "I merely record," I said at p. 9 of my paper, "the statistical evidence collected at a point on the globe's surface, at which, from its tropical situation and physical conditions, such a factor would exercise an influence in a well-marked manner." I insisted on this, as the local influences at work on the rainfall suffice in several parts of Southern India, to disguise the operation of any general law. Mr. Archibald may, however, have been led into this misapprehension from an ambiguous expression in the first sheets of my paper, which were hastily struck off as I was leaving India, with a view to placing the Government in possession of the facts before my departure. In these sheets I find the words "Southern India" used once or twice as a periphrasis to avoid the too frequent repetition of the word Madras. This ambiguity was removed from the paper as finally printed. I need hardly add that the words "the whole of Southern India" nowhere occurred. I hope shortly to show in a more carefully elaborated work, the limitations under which the results arrived at in my former paper can be safely generalised. Meanwhile Mr. Archibald's interesting communications both in NATURE, and in the Calcutta *Englishman* are worthy of careful study. WM. HUNTER

Lanarkshire, August 27

The Telephone

IN the present agitation concerning speaking or telephonic telegraphs, the following extract from M. Le Comte du Moncel's "Exposé des Applications de l'Electricité," edition of the year 1857, vol. iii. p. 110, may be interesting as pointing out how nearly the idea has been forestalled.

"*The Electric Transmission of Speech.*"

"I did not wish to bring forward in the chapter of the electric telegraph a fantastic conception of a certain M. Ch. B——, who believes that it will be possible to transmit speech electrically, because it might have been asked why I had classed among so many remarkable inventions an idea that, presented by the author as it is, is not more than a dream. However, to be faithful to the *rôle* that I have imposed upon myself of speaking of all the applications of electricity that have become known to me, I wish to quote here the information which the author has published on this subject.

"After the marvellous telegraphs which are able to reproduce at a distance writing of this or that individual, and designs more or less complicated, it seemed impossible, said M. B——, to advance further in the regions of the marvellous. Nevertheless, essaying to do something more, I asked, for example, if speech itself would not be capable of transmission by electricity; in a word, if one would not be able to speak at Vienna and be heard at Paris. The thing is practicable. This is how: Sounds, it is known, are formed by vibrations and carried to the ear by these same vibrations, which are reproduced by the intermediate media.

"But the intensity of these vibrations diminishes very rapidly with the distance, from which it follows, even in the employment of speaking trumpets, tubes, and of acoustical horns, the limits which cannot be surpassed are very restricted. *Imagine that one speaks near a mobile plate, flexible enough not to lose any of the vibrations produced by the voice, that this plate establishes and interrupts successively the communication with a battery. You would be able to have at a distance another plate which would execute at the same time the same vibrations.*

"*It is true that the intensity of the sounds produced would be variable at the point of departure where the plate is vibrated by the voice, and constant at the point of arrival where it is vibrated by electricity.* But it is demonstrable that this would not alter the sounds.

"It is evident from the first that the sounds would reproduce themselves with the same pitch in the scale. The actual condition of acoustical science does not permit of saying, *à priori*, whether the same conditions would hold good for all syllables articulated by the human voice. The manner in which these syllables are produced is not yet sufficiently well known.

. . . .

"In any case it is impossible to demonstrate, in the present state of science, that the electric transmission of sounds is impossible. Every probability, on the contrary, is for the possibility. An electric battery, two vibrating plates, and a metallic wire will suffice.

. . . .

"It is certain that, at a time more or less distant, speech wil be transmitted to a distance by electricity. I have commenced some experiments to that effect, they are delicate and require time and patience. But the approximations obtained point towards a favourable result."

PAGET HIGGS

Museums

THE following suggestions may possibly prove useful to directors of museums, and especially of provincial museums. Most of the plans recommended have been tried with success.

It is very desirable that in all collections intended for public instruction manuscript labels should be abolished. The advantages of perfect legibility, uniform style, and an occasional change of cards far outweigh the cost of letter-press. A convenient hand-press costs about 3*l.*; several founts of type in quantity sufficient for museum purposes, may be had for 5*l.* An assistant can be taught printing in a few days; I have at times engaged a printer's apprentice, paying sevenpence an hour for his services.

The proper display of dissected preparations put up in spirit has long been a serious trouble. Most dissections of small size can be pinned out on wax. Young's Paraffin Light and Mineral Oil Company, of West Calder, have lately prepared, at my request, smooth paraffin slabs, coloured deep blue, and cut to 12 in. × 6 in. These can be had at a shilling a pound. Cylindrical glass vessels are objectionable, not only on account of distortion, but because they render it difficult to demonstrate details of structure. Rectangular trays with movable plate-glass lids are far more convenient. These may be made of ebonite for the smaller sizes, and of wood lined with gutta-percha where the cost of ebonite becomes important. I hope before long to get a useful tray cast in glass. The edges must be accurately ground, and the cover secured by light brass clamps. In the bottom of the tray the wax tablet can be securely fixed. It is useless to cement the lid to the tray. Hardly any cement will stand prolonged exposure to dilute spirit, and it is necessary to readjust or clear the dissection from time to time.

Fossils are usually kept loose; in the larger collections they are mounted on tablets of wood or glass covered with paper. The first method is untidy and often causes loss of labels; wooden tablets are costly, difficult to cut of quite uniform size, and liable to warp; glass is also difficult to cut true, and wastes much time in covering with paper. Ten years ago I procured a supply of pasteboard tablets one-tenth of an inch thick from a pattern-card maker and have used them exclusively since. They are cheap (ninepence to a shilling a pound), can be cut perfectly true by machinery, do not warp, and may be had of any colour. Fossils glued to pasteboard with coaguline are perfectly fast; we range them in wall-cases upon shelves sloped to forty-five degrees, and never meet with accidents.

In our geological wall-cases I have introduced above the level of the eye a range of boards, nearly upright, but sloping slightly forwards at the top, upon which maps, sections, photographs, and descriptive notices can be pinned. In a palæontological collection this space is useful for drawings of restored animals

It is much to be desired that the dealers would procure a better choice of zoological models in glass and porcelain. Reuss' foraminifera are still useful, though antiquated; Blaschka, of Dresden, keeps no stock, though he has supplied many of our museums with useful models in glass made from drawings. We want artistic and accurate coloured models of mollusca, hydrozoa, &c., far beyond the present supply.

Stuffed animals, especially stuffed mammalia, are the plague of a curator. I do not refer especially to their liability to moths (insects of all kinds can be kept down by placing saucers of carbolic acid in the cases) but to their grotesque deformity, their unnatural attitudes, and their proneness to contract in unexpected places. A model in plaster or clay, strengthened internally by wires would last for ever, and the skin would stretch over it readily enough when moist. Real skill in modelling is required here, and we have not yet been able to command it. The Schools of Art may in time help us over the difficulty. A well-modelled animal can never be very cheap, but if increased costliness should render set-up quadrupeds comparatively scarce, zoology need not suffer on that account.

Public museums should contain far more than they now do the elementary explanations necessary for the right understanding of the objects exhibited. A text-book illustrated by specimens instead of wood-cuts should be our aim, at least where the wants of the public are more concerned than the wants of special students. I should propose to relegate nine-tenths of our existing collections to cabinets were it not that things out of sight in cabinets are so liable to suffer from neglect. At present we aim at too much, introduce too many departments into a small museum, show too many obscure and uninstructive objects, and spoil everything by over-crowding.

Personally, I do not hold that local collections should be everything in a provincial museum. We have to consider the wants of residents as well as of passing strangers, and what the residents interested in natural history require is a general collection of typical specimens which will teach them something of the elements of their science. It is very easy to make imposing collections of land and fresh-water shells, butterflies, and so forth, which a naturalist passing that way praises because they contain here and there a choice thing, but which either teaches nothing to the uneducated visitor, or else teaches him the very undesirable lesson that the best thing he can do is to make a similar collection for himself. We have had more than enough of unintelligent collecting and unintelligent records of occurrence. Our provincial museums should tell the public that to know something of the structure of animals and plants is better than to know many species.

L. C. MIALL

Leeds, August 17

THE great difficulty, as it seems to me, in promoting and maintaining the efficiency of our local museums lies in providing them with suitable curators; and in this connection an idea which occurred to me last year may prove not unserviceable. I have seen a large number of our provincial museums, and in many of them have found really extensive and valuable collections of natural objects which only require to be rightly named and properly arranged to become admirable educational aids. In few, however, is there enough material to engage the whole time and attention of an able man in taking care of it; indeed a single month devoted to each of the departments of zoology, botany, geology, and so forth, would suffice, and, in many cases, more than suffice, to put each into working order to begin with, and after the first arrangement it would be easy enough to maintain the efficiency of each collection and to add what fresh acquisitions might be made in the course of a week's visit once a year.

Let, then, an association of the younger workers in the various branches of science be formed in London, under the direction of a committee of well-known names, and let it offer to send out every year for short intervals, to such museums as should be ready to pay for them, botanists, zoologists, geologists, and the rest, to name and arrange their several collections; each member so dispatched would then visit several museums in succession, confining his attention in each to the collection made in his own subject, and each museum would be visited by several members, one member for each of its essentially different collections. Thus for a slight expense (payment on the piece-work system) a large number of our Local Museums would be put under the curatorship of a group of specialists, and so be brought into efficient and permanent working order. The idea is simply that of visiting curatorships supplied on the principle of co-operation, and made possible by the facilities for travelling afforded by our modern railway system.

It can scarcely be doubted that in the summer, when lectures and lecturing are over, many scientific men might be found willing and able to undertake the task. W. J. S.

Rainbow Reflected from Water

MR. CROOKES' interesting observation of the reflection of a rainbow—described in his letter in NATURE, August 16—is easily reproduced, on a small scale, experimentally.

I fixed a "spreader" to the nozzle of a garden-engine so as to cause a shower of fine drops of water to spread in the sunshine. The segments of a bright primary rainbow and of a rather subdued secondary one stood out well-defined against the dark foliage of some trees, the remainders of the bows being lost against bright objects and sky behind.

At whatever point the bows were visible, I found that by placing a mirror or blackened glass wetted so as to form a surface of water, in place of the eye, and then observing from a fresh point, the reflections of both bows could be very distinctly seen at the same time that real bows were also visible.

The reflected bows were always apparently smaller in diameter than the real bows which were visible at the same time from the same position. The reason of this is, I presume, that the bows seen in the mirror are not the reflections of the bows visible, at the same time to the eye, but of bows which the eye would see if it occupied the place of the mirror, or rather of that portion of it which is observed. When, for instance, the mirror is one yard below the level of the eye, the drops by which the bows are formed that are reflected by the mirror, are necessarily about one yard below the corresponding drops by which the direct bows seen by the eye are formed; in other words the direct bows are one yard above the bows which are actually reflected. Therefore, when both are cut by a common horizontal line formed by the surface of the mirror, a reflected bow must be the more shortened of the two and its diameter apparently reduced.

I would suggest that this may be the explanation of the displacement of the colours where the real and reflected bows met, which Mr. Crookes observed. ROBERT SABINE

Hampton Wick, August 20

The Greenland Foehn

DANS le dernier numero (406) de votre journal je vois que vous m'avez fait l'honneur de donner un abstract d'un petit travail sur le foehn du Groenland. Malheureusement le rapporteur n'a pas bien compris le danois (ou le norwegien) en quelques endroits, et je me permettrai de vous indiquer les méprises suivantes comme les plus dangereuses.

2ième alinéa.—"Dr. Pfaff has carried on . . . . and these show that the average temperature of *February*, 1872, was – 8°·7 C., and of *February*, 1863 – 31°·6," etc. Les deux mots, "February," sont omis, ce qui fait croire que je parle de la température moyenne de l'année au lieu d'un mois.

5ième alinéa.—"These explanations go a great . . . . when at Jacobshavn shortly before *July*, 9° C. of heat are recorded." Au lieu de "July" j'ai dit "Christmas"; une température de 9° C. est normale en juillet.

Août 21 W. HOFFMEYER

On the Supposed Action of Light on Combustion

IN answer to Mr. Watson's inquiry contained in your last number, I may state that at the meeting of the British Association at Exeter, in 1869, I read a paper under the above title (See *Phil. Mag.* for September, 1869), in which some comparative experiments were made on candles burning in full sunshine and also in a darkened closet. This mode of experiment was adopted because it allowed the results to be tested by weighing. Candles of the same make were used and hard sperm candles preferred as being less affected by variations of temperature than composite. The candles were allowed to burn during four hours. I give one result:—

In the dark (temp. 81° F.) each candle lost 544 grains, or 136 grains per hour.

In the light (temp. 84°) each candle lost 567 grains, or 142 grains per hour nearly.

It is evident that in this case the increase of temperature caused by the bright sunshine led to an increased consumption of material, but the general result was that light has no retarding influence on combustion. C. TOMLINSON

Highgate, August 25

Evolution by Leaps

WITH reference to an article entitled "Evolution by Leaps," in your "Biological Notes" (NATURE, vol. xvi. p. 208), I would call attention to a fact which is not unknown to horticulturists, that a hybrid sometimes proclaims its origin by producing—even on the same rachis—flowers and fruits, some of which resemble one parent and some the other.

Many a time I have plucked a branch of two or three feet in length from a pear-tree growing in a village in Kent, which bore at the proximal end pears of a certain size and description, and on the terminal twigs pears smaller in size, of a different flavour, and later in blooming and ripening.

As this "sport" prevailed throughout the tree, which was large and flourishing, there was no possibility of its being the result of a direct graft. PAUL HENRY STOKOE

Beddington Park

Zygœna Filipendulæ

IN July last I was breeding some *Zygœna filipendulæ* (six-spot Burnet moth) from pupæ taken in a chalk-pit near Cambridge, one of which was developed into a moth with five wings; four of these correspond to the normal wings in this species and are perfect in every respect, as also are five of the legs. The sixth leg (a hind leg) is absent, its place being filled up by the extra wing, which springs from the exact point at which the missing leg would naturally join the body. In appearance the extra wing resembles the ordinary hind wing of the species, but is only about half its usual size. It is of a yellowish-red tinge, and not so thickly covered with scales as the other wings of the insect. Of the sixth leg there is no external trace whatever, as far as I can see; in fact it would seem at first sight as if the leg had, by some means or other, been transformed into a wing.

This moth is subject to a good deal of variation as regards the size of the spots on the fore-wings, two of which are occasionally united; also, in this particular locality, the red colour is replaced by yellow in about 1 per cent. of the specimens. The chalk-pit to which I have alluded is scarcely an acre in extent, and as the species does not seem to occur elsewhere in the immediate neighbourhood, continuous interbreeding must have been going on for a long time.

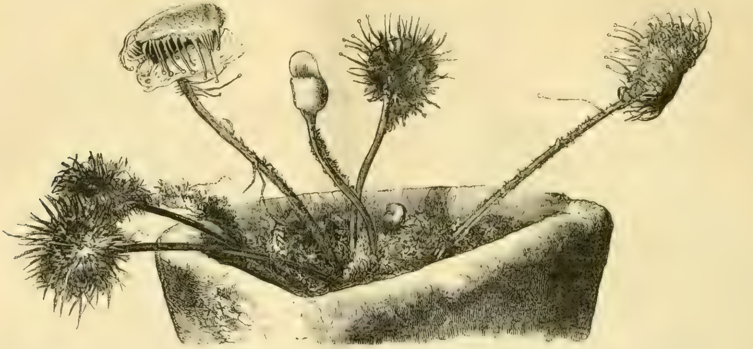
I have never met with or heard of such a curiosity of morphology either in this or any other lepidopterous species before, but some of your readers will doubtless be able to adduce other instances of a similar nature. N. M. RICHARDSON

Clare College, Cambridge, August 21

Drosera

I BEG to enclose a photograph of a specimen of *Drosera rotundifolia* found by me at the Lickey Hills on July 1 this year.

If anything could demonstrate the propensity for fly-catching known to exist in this class of plants, surely this specimen does in the most marked degree. You will see that a moth has been entangled by the hairs of one of the leaves, which leaf has curved itself right over the moth in the most determined fashion. There is every appearance of a struggle having taken place which ended in the defeat and destruction of the moth.



This specimen is, I should imagine, a very typical one, and as such I have sent a copy to Mr. Darwin. WRIGHT WILSON

Birmingham

The Radiant Centre of the Perseids

FROM twenty meteors, mostly with streaks, I deduced the radiant point at R.A. 40°, Dec. 56° N., August 3-7. On August 10 I saw a large number (fifty-seven per hour) of *Perseïds*, many of them with short tracks near the focus, and almost invariably with streaks, from 43° + 58°. On August 12 I observed quite an outburst of precisely similar meteors from a sharply-defined centre at 50° + 55°, and registered fourteen of them, but many others were noted between 12h. and 14h. On the 16th, between the same hours, I saw five paths close to a radiant at 60° + 59°. These had streaks and apparently exhibited the same features of motion, colour, &c., as those recorded on the few preceding nights. Can these four positions represent one and the same system of *Perseïds* with an apparent displacement of the radiant centre on the several nights of observation? The places may be regarded as accurate for the dates, and though quite possibly they are separate showers, it is at least singular they became so well marked one on each night. If the positions include the same system then the focus of divergence appears to have shifted from 40° + 58° on the 3rd-7th to 60° + 59° on the 16th, so that while the declination remained nearly the same the R.A. had advanced twenty degrees, which in D. 59° N. is equivalent to ten linear degrees of space.

It is a capital plan while observing and mapping meteor tracks to hold a perfectly straight rod in the hand, and directly a meteor is seen, to project the rod upon its apparent path, carrying the eye back in the same line of motion and noting the exact point with reference to stars upon which it converges. In the case of slow meteors or meteors with streaks, this is a very accurate method and especially to be recommended in regard to paths presumably a long way from the radiant. Eye-estimates are necessarily less exact, for while the *position* of the track is being noted the more important feature of *direction* is inaccurately remembered. W. F. DENNING

August 17

Fish Commensals of Medusæ

IN the numbers of NATURE for July 19 and 26 (pp. 227, 248) are communications respecting fish-sheltering Medusæ. The *Tröchurus* in Europe appears to be a commensal of the Acaleph as well as the *Pollöchius*. In the eastern waters of the United States, however, so far as I am aware, the Stromatoid fish *Poronotus similis* (*Stromateus similis* of some authors) seems to be the most common, if not the only associate, of several acalephs, viz., *Dactylometra quinquecirra*, *Zygodactylon grœnlandica*, and *Cyanea arctica*. Under the umbrellas of these species small *Poronoti* are to be found in the late summer swimming, sometimes even to the number of twenty or more, but generally much fewer. Mr. Alexander Agassiz, in his "Sea-side Studies," mentions the occurrence of an undetermined "Clupeoid" fish, but no other, under the umbrella of *Dactylometra quinquecirra*; the identification is probably erroneous. At least my own observations were made in the same region and at the same time of the year as Mr. Agassiz's, and only the *Poronotus* was seen. More detailed information respecting this association may be found recorded by Prof. Verrill in the "United States Commission of Fish and Fisheries" reports, Part I., pp. 449-450, 1873. THEO. GILL

Smithsonian Institution, Washington, August 6

Science in Spain

I THINK it may interest the readers of your journal to have some slight idea of the state of natural sciences in Spain. Science is universal, and the efforts made by a nation which has been separated by centuries of intolerance and indifference from the movement and scientific life of other countries, cannot fail to be looked upon with indulgent eyes by those who cultivate science.

Of the three great branches into which we may divide natural science—physics, chemistry, and natural history, the first is in a most backward state in Spain. In almost all the professorships where this science is taught, the instruction given is so out of date, that no mention is made of the modern theory of the correlation of forces or thermo-dynamics, and the text-books used are French works, now quite obsolete. In every one of our upper schools—Institutos de 2da Ensiñanza—there is a professor who teaches physics and chemistry conjointly, who is instructed to go through a course of these sciences, which are reduced by this means to their lowest possible expression. In our universities, there exist classes in which an amplification of physics is taught; this study is part of those required for the preparatory exercises for the faculty of medicine. This course, if we take into consideration the knowledge brought by the pupils who attend it, is more an explanation of what they ought to have learnt than anything else. At the Madrid University alone, there is a class of "imponderable fluids;" the name in itself suggests an idea quite out of date at the present day. At the same university there is also a class of mathematical physics, but it does not form part of the studies required to receive a doctor's degree in the physico-chemical sciences, and is only included in the mathematical sciences. This is unfortunately all the official instruction on the subject which is given in Spain. Almost all the professors follow the theories which were generally admitted before the discoveries of Grove, Mayer, Rankine, Clausius, Tyndall, and Helmholtz.

During the Republican Government in Spain, it was decreed to reorganise these studies in a manner more in accordance with modern ideas, but the short rule of this reforming government prevented this plan from being carried out, or conquering the tenacious resistance of the great majority of the Spanish professors.

Some champions of modern ideas are happily not wanting in Spain. Among them is one of the most distinguished members of the Madrid Observatory, Sr. Jimenez, who has written an interesting volume on the theory of numbers, which obtained a prize from the Spanish Academy of Science. Sr. Jimenez began to publish a few years ago a theory of light in compliance with the most authorised physico-mathematical doctrines. A man of immense and varied intellect, as a dramatic poet, as an engineer, as a mathematician and economist, and one of the principal men of the revolution, Sr. Echegaray has done much to popularise the modern theories of physics, by a volume dedicated to the General Public, and also by his elementary treatise on thermo-dynamics. He is now publishing some studies on light in a scientific review, which are chiefly intended to extend these studies in our scientific circles. Dr. Vicuña, Professor of Mathematical Physics at the Madrid University, endeavours to do the same by his teaching, and by means of the articles and memoirs which he publishes from time to time. Of these may be mentioned his theory and calculation of steam-engines in accordance with thermo-dynamics.

The scientific instruction which is given to the young men who attended the upper school at the Observatory of Marino, at San Fernando, near Cadiz, is much commended. Every day foreign books are more universally read, translated, and understood, the most popular being those by Tyndall. Prof. Barreda, Felin, Ramos, v. Chamorro are great advocates of modern science. Sr. Escrig y Mieg, the professor at the Institute at Guadalajara, has set up some interesting scientific apparatus there, and has introduced in the pneumatic machine an improvement which reduces the injurious space. The barometer constructed by Sr. Torres, the inventor of probably the most accurate barometer known in European meteorology, merits special mention. It is much to be regretted that owing to special circumstances, his instrument could not figure at the interesting exhibition at South Kensington.

At the Free Institution, lately established for teaching at Madrid, by private enterprise, which the readers of NATURE have already seen referred to in your columns, there is a class of experimental physics, according to the latest development of this science. On the evening of January 28, a series of public lectures were begun, with the object of popularising science in Spain. Dr. Simarro, a young professor at this institution, gave the first lecture on light, and repeated some of Tyndall's most remarkable experiments.

Most of these efforts are, however, still limited to the attempt to spread in Spain a knowledge of the actual state of physical science from other countries which are in a more advanced condition, rather than to contribute to general culture works of original investigation. The interesting studies of Prof. Serrano Faligate, on general and biological physics, some of which have been noticed by English reviewers, are almost the only works on the subject which can be mentioned of importance. It is indeed to be hoped this will no longer be the case when these studies are more generally developed, and act as a stimulant to the genius of the Spanish people.

Madrid FRANCISCO GINEZ DE LOS RIOS

OUR ASTRONOMICAL COLUMN

THE ROTATION OF SATURN.—In NATURE (vol. xv. p. 243), reference was made to the discovery by Prof. Asaph Hall of a small, well-defined, very white spot upon the disc of Saturn just below the ring, and to observations which were in progress to ascertain, by means of it, the period of the rotation of the planet upon its axis. Prof. Hall succeeded in following up this spot which was from 2″ to 3″ in diameter until January 2, when the weather having become unfavourable, the planet low, and the spot faint and indistinct, observations were discontinued. From a thorough discussion of the observations at Washington and elsewhere in the United States, Prof. Hall finds for the mean time of the rotation of Saturn—

10h. 14m. 23s·8 ± 2s·30.

It has been necessary to assume that the spot had no proper motion upon the surface of the planet, which is a point on which the observations throw no light.

On the first detection of this spot on December 7, with the view to secure assistance from other observers in noting its central passages on the disc, an ephemeris was circulated from Washington, in preparing which the time of rotation was taken at 10h. 29m. 16·8s., given, as Prof. Hall remarks, "in nearly all the modern text-books as Sir W. Herschel's last and most accurate determination;" notwithstanding this it appears certain that Sir W. Herschel never assigned this period, and its adoption in the Washington ephemeris was so far unfortunate as it may have rather hindered than assisted observations; indeed "through this mistake several observers failed to see the spot."

It is very probable that Prof. Hall has suggested the real cause of the introduction of this erroneous value for the time of Saturn's rotation into so many of the so-called "text-books," the compilers of which rarely concern themselves with references to original authorities, and yet in this case the erroneous value has been given by writers, whom it might well be supposed it was safe to follow. In the *Exposition du Système du Monde*, the first edition of which appeared in An. IV. of the French republican era, Laplace says that Saturn rotates in 0·428, and the ring in 0·437, these figures being decimals of a day; they correspond to 10h. 16m. 19·2s. and 10h. 29m. 16·8s., the former expresses therefore the Herschelian period of rotation (10h. 16m. 0·4s.) to the nearest decimal in the third place, and the latter is the value for rotation of Saturn given in so many astronomical works. Hence Prof. Hall thinks that some one early in the century copied and converted the wrong number from Laplace and "the book-makers have faithfully copied this mistake."

Hansen in his "Allgemeine Uebersicht des Sonnensystems" gives 10h. 29m. 17s. for time of rotation both of Saturn and his ring; Mädler, "Ueber die Weltstellung der Körper unsers Sonnensystems," has 10h. 16m. for the globe and 10h. 29m. 17s. for the ring, but in the early editions of his treatise on Astronomy (as in that of 1849, pp. 251 and 254) he assigns 10h. 29m. 17s. for the globe, and 10h. 32m., after Herschel, for the ring, adding "wahrscheinlich ist sie der des Saturn selbst gleich und beide sind etwa 10h. 30m. in runder Zahl." Sir John Herschel, in the first edition of his Treatise on astronomy in Lardner's "Cabinet Cyclopædia" published in 1833, gives 10h. 29m. 17s. both for Saturn and the ring, and he probably followed Baily's "Astronomical Tables and Formulæ" which appeared in 1827, and where we find at pp. 39 and 59 the same period 10h. 29m. 16·8s. assigned for both rotations, and Baily expressly states that "the elements of the system are taken for the most part from the *Système du Monde* of M. Laplace (fifth edition, 1824), so that it is possibly to this work, which was one of general reference for many years, that the original oversight suggested by Prof. Hall is to be traced. Sir W. Herschel in the *Philosophical Transactions*, 1790, p. 480, states that his observations of lucid spots upon the ring, supposing them to adhere to it, would be explained by "admitting a revolution of the ring itself in 10h. 32m. 15·4s., and in the volume for 1794, p. 28, he finds for the rotation of the globe of Saturn, 10h. 16m. 0·44s., which are the only values that bear his authority.

Prof. Asaph Hall's value must now be taken as undoubtedly a very close approximation to the true period in which Saturn rotates. According to it, the planet's year consists of 25.217 Saturnian days. To the rarity of spots upon the disc of so small and well-defined a character as that which has been recently observed to such useful purpose at Washington, is perhaps to be mainly attributed the want of an earlier reliable determination of the rotation period in confirmation of Sir W. Herschel's, made upwards of eighty years previously.

THE COMET OF 1812.—In anticipation of the return of this comet to perihelion within the next few years, Prof. Winnecke has published ephemerides to facilitate its rediscovery, which have been prepared by Herr Mahn on his suggestion. They appear in the *Vierteljahrsschrift der astronomischen Gesellschaft*, 12 Jahrgang, 2 Heft. Encke's period, 70·7 years, would bring the comet to perihelion again in 1883, but Mr. W. E. Plummer, now of the University Observatory at Oxford, some years since stated that a period of 69·2 years would better agree with normal places which he had very carefully prepared. The comet may therefore visit us in 1881, or possibly much earlier with the unknown effect of perturbation. The sweeping-ephemerides are arranged upon a plan conveniently indicating the line in which the comet should be sought at a particular date. It is a case where the "orbit-sweeper," suggested by Sir George Airy, and advocated by Prof. Winnecke, would, if provided with an object-glass of sufficient optical capacity, render much assistance.

The Companion of Sirius.—In the *Comptes Rendus* of the French Academy of Sciences, August 13, M. Flammarion has a graphical representation of the orbit assigned by Dr. Auwers, to the perturbing companion of Sirius and of the observed course of the small star discovered by Mr. Alvan Clark, with the view to illustrate the increasing differences between theory and observation. Allusion was made to this subject in Nature (vol. xiii. p. 428), where the differences of Dr. Auwer's ephemeris, 1872-75, were given. The latest measures of the Clark-companion at Washington, show for 1877·21, position $(c - o)$, + 6°·9, distance − 0″·88.

Prof. Asaph Hall found no other star in the vicinity of Sirius nearer than one of the thirteenth magnitude, which was measured on February 28, 1877; position 114°·9, distance 72″·09; probably the star seen by Mr. Marth at Malta in January, 1865. An examination of the vicinity with the great refractor was made at the request of M. Tempel, of Florence, who had suspected the existence of several small stars near Sirius.

Satellite of Mars.—One of the newly-discovered satellites of Mars was observed by M. M. Henry at the Observatory of Paris, on August 27.

At 12h. 9m. mean time, position 249° 56′, distance 85″·2, the satellite was very faint, and only observable when the planet was screened from view.

BIOLOGICAL NOTES

The Development of the Nerves in Vertebrates.—Mr. Balfour's discovery that the spinal nerves of sharks and rays are developed as outgrowths from the central nervous system has been followed by a similar revelation with regard to birds. Mr. (now Dr.) A. M. Marshall (of Cambridge) has given an account of investigations respecting the origin of nerves in the fowl (*Journ. Anat.*, April, 1877), describing a longitudinal ridge arising on the summit of the neural canal, and giving off paired processes, the rudiments of the posterior roots of the spinal nerves. Hensen has made analogous observations on the spinal nerves of the rabbit. The anterior roots arise later, distinct from one another, as processes from the spinal cord. Mr. Balfour has endeavoured to solve the difficult question of the relations of the cranial to spinal nerves. He finds as yet no traces in the brain of anything comparable to anterior roots of nerves; all the nerves are posterior roots. The fifth, or trigeminal, arises from the dorsal summit of the hind-brain very early, just like a dorsal root of a spinal nerve. This nerve also, instead of being a compound one, is at any rate in its origin perfectly simple. The auditory nerve and the facial arise by one common root. The glossopharyngeal and vagus have a series of distinct roots. In an adult Scyllium twelve separate strands have been counted in the vagus nerve. This number, and their origin like so many separate spinal nerves, opens up interesting questions in regard to the primitive segmentation of the head and the loss or condensation of segments in the evolution of the vertebrates. Dr. Marshall's observations on the cranial nerves of the chick, so far as they go, correspond to Mr. Balfour's. It appears that there is no definite indication of a limit between head and trunk afforded by the central nervous cord, by the outgrowths from it, or by the mode of development of the nerves. It is open for consideration whether the absence of anterior roots to the cranial nerves may not furnish such a limit; this would be very convenient for morphology.

Insect Aid in Fertilisation of Flowers.—Mr. Thomas Meehan, of Philadelphia, continues to bring forward cases to show that many flowers are not so dependent on insect fertilisation as has been imagined. Recently (*Proc.* Acad. Nat. Sciences, Philadelphia, 1877, p. 128) he has instanced the common mignonette, which usually does not seed when forced in greenhouses in winter. It has been asserted that this is due to the absence of suitable insects to produce fertilisation. But last winter Mr. Meehan's specimens took to producing seed in abundance, two to six perfect seeds in every capsule. This showed that some other circumstance had come into play which affected the reproductive organs, insect aid having been as much absent as in other cases.

Insectivorous Plants.—Dr. C. Cramer, of Zürich, publishes, under the title "Ueber die Insectenfressenden Pflanzen," a useful epitome of all that has at present been recorded respecting the singular phenomenon of "Insectivorous Plants." In a series of papers in *Flora*, on the Mechanics of the Movements of these plants, A. Batalin calls attention to a hitherto neglected paper of Oudemans, published (in Dutch) in 1859, in which he describes the greater part of the phenomena of irritation in Venus's fly-trap (*Dionæa muscipula*), agreeing in almost every point with the description subsequently given by Darwin and others.

Spontaneous Movements in Plants.—M. E. Rodier, of Bordeaux, has described a singular series of automatic or spontaneous movements in a well-known water-plant, *Ceratophyllum demersum*. They consist of a rhythmical motion caused by a curvature of the axis extending over six hours, which is neutralised in the course of the next twelve hours, and followed by a curvature in the opposite direction extending over four hours, which is again neutralised in four hours, the whole cycle thus extending over a period of twenty-six hours. The movement appears to be entirely unaffected by light.

DISCOVERY OF OXYGEN IN THE SUN BY PHOTOGRAPHY, AND A NEW THEORY OF THE SOLAR SPECTRUM[1]

I PROPOSE in this preliminary paper to indicate the means by which I have discovered oxygen and probably nitrogen in the sun, and also to present a new view of the constitution of the solar spectrum.

Oxygen discloses itself by bright lines or bands in the solar spectrum and does not give dark absorption lines like the metals. We must therefore change our theory of the solar spectrum, and no longer regard it merely as a continuous spectrum with certain rays absorbed by a layer of ignited metallic vapours, but as having also bright lines and bands superposed on the background of continuous spectrum. Such a conception not only opens the way to the discovery of others of the non-metals, sulphur, phosphorus, selenium, chlorine, bromine, iodine, fluorine, carbon, &c., but also may account for some of the so-called dark lines, by regarding them as intervals between bright lines.

It must be distinctly understood that in speaking of the solar spectrum here, I do not mean the spectrum of any limited area upon the disc or margin of the sun, but the spectrum of light from the whole disc. I have not used an image of the sun upon the slit of the spectroscope, but have employed the beam reflected from the flat mirror of the heliostat without any condenser.

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[1] Paper by Prof. Henry Draper, M.D. Read before the American Philosophical Society, July 20, 1877 We are indebted to Dr. Draper's kindness for the plate and illustrations which accompany this paper.

In support of the above assertions the accompanying photograph of the solar spectrum with a comparison spectrum of air, and also with some of the lines of iron and aluminium, is introduced. The photograph itself is absolutely free from handwork or retouching. It is difficult to bring out in a single photograph the best points of these various substances, and I have therefore selected from the collection of original negatives that one which shows the oxygen coincidences most plainly. There are so many variables among the conditions which conspire for the production of a spectrum that many photographs must be taken to exhaust the best combinations. The pressure of the gas, the strength of the original current, the number of Leyden jars, the separation and nature of the terminals, the number of sparks per minute, and the duration of the interruption in each spark, are examples of these variables.

In the photograph the upper spectrum is that of the sun, and above it are the wave-lengths of some of the lines to serve as reference numbers. The wave-lengths used in this paper have been taken partly from Angström and partly from my photograph of the diffraction-spectrum published in 1782. The lower spectrum is that of the open air Leyden spark, the terminals being one of iron and the other of aluminium. I have photographed oxygen, nitrogen, hydrogen, and carbonic acid, as well as other gases in Plücker's tubes and also in an apparatus in which the pressure could be varied, but for the present illustration, the open air spark was, all things considered, best. By other arrangements the nitrogen lines can readily be made as sharp as the oxygen are here, and the iron lines may be increased in number and distinctness. For the metals the electric arc gives the best photographic results, as Lockyer has so well shown, but as my object was only to prove by the iron lines that the spectra had not shifted laterally past one another, those that are here shown at 4325, 4307, 4271 4063, 4045, suffice. In the original collodion negative many more can be seen. Below the lower spectrum are the symbols for oxygen, nitrogen, iron, and aluminium.

No close observation is needed to demonstrate to even the most casual observer that the oxygen lines are found in the sun as bright lines, while the iron lines have dark representatives. The bright iron line at G (4307), on account of the intentional overlapping of the two spectra, can be seen passing up into the dark absorption line in the sun. At the same time the quadruple oxygen line between 4345 and 4350 coincides exactly with the bright group in the solar spectrum above. This oxygen group alone is almost sufficient to prove the presence of oxygen in the sun, for not only does each of the four components have a representative in the solar spectrum, but the relative strength and the general aspect of the lines in each case is similar. I do not think that in comparisons of the spectra of the elements and sun, enough stress has been laid on the general appearance of lines apart from their mere position; in photographic representations this point is very prominent. The fine double line at 4319, 4317, is plainly represented in the sun. Again there is a remarkable coincidence in the double line at 4190, 4184. The line at 4133 is very distinctly marked. The strongest oxygen line is the triple one at 4076, 4072, 4069, and here again a fine coincidence is seen, though the air spectrum seems proportionately stronger than the solar. But it must be remembered that the solar spectrum has suffered from the transmission through our atmosphere, and this effect is plainest in the absorption at the ultra-violet and violet regions of the spectrum. From some experiments I made in the summer of 1873 it appeared that this local absorption is so great, when a maximum thickness of air intervenes, that the exposure necessary to obtain the ultra-violet spectrum at sunset was two hundred times as long as at mid-day. I was at that time seeking for atmospheric lines above H like those at the red end of the spectrum, but it turned out that the absorptive action at the more refrangible end is a progressive enfeebling, as if a wedge of neutral tinted glass were being drawn lengthwise along the spectrum towards the less refrangible end.

I shall not attempt at this time to give a complete list of the oxygen lines with their wave-lengths accurately determined, and it will be noticed that some lines in the air spectrum which have bright analogues in the sun are not marked with the symbol of oxygen. This is because there has not yet been an opportunity to make the necessary detailed comparisons. In order to be certain that a line belongs to oxygen, I have compared, under various pressures, the spectra of air, oxygen, nitrogen, carbonic acid, carburetted hydrogen, hydrogen, and cyanogen. Where these gases were in Plücker's tubes a double series of photographs has been needed, one set taken with and the other without Leyden jars.

As to the spectrum of nitrogen and the existence of this element in the sun there is not yet certainty. Nevertheless, even by comparing the diffused nitrogen lines of this particular photograph, in which nitrogen has been sacrificed to get the best effect for oxygen, the character of the evidence appears. The triple band between 4240, 4227, if traced upward into the sun, has approximate representatives. Again at 4041 the same thing is seen, the solar bright line being especially marked. In another photograph the heavy line at 3995, which in this picture is opposite an insufficiently exposed part of the solar spectrum, shows a comparison band in the sun.

The reason I did not use air in an exhausted Plücker's tube for the production of a photograph to illustrate this paper and thus get both oxygen and nitrogen lines well defined at the same time, was partly because a brighter light can be obtained with the open air spark on account of the stronger current that can be used. This permits the slit to be more closed and of course gives a sharper picture. Besides the open air spark enabled me to employ an iron terminal and thus avoid any error arising from accidental displacement of the reference spectrum. In Plücker's tubes with a Leyden spark the nitrogen lines are as plain as those of oxygen here. As far as I have seen, oxygen does not exhibit the change in the character of its lines that is so remarkable in hydrogen under the influence of pressure as shown by Frankland and Lockyer.

The bright lines of oxygen in the spectrum of the solar disc have not been hitherto perceived, probably from the fact that in eye observation bright lines on a less bright background do not make the impression on the mind that dark lines do. When attention is called to their presence they are readily enough seen, even without the aid of a reference spectrum. The photograph, however, brings them into a greater prominence. From purely theoretical considerations derived from terrestrial chemistry and the nebular hypothesis, the presence of oxygen in the sun might have been strongly suspected, for this element is currently stated to form eight-ninths of the water of the globe, one-third of the crust of the earth, and one-fifth of the air, and should therefore probably be a large constituent of every member of the solar system. On the other hand the discovery of oxygen and probably other non-metals in the sun gives increased strength to the nebular hypothesis, because to many persons the absence of this important group has presented a considerable difficulty.

At first sight it seems rather difficult to believe that an ignited gas in the solar envelope should not be indicated by dark lines in the solar spectrum, and should appear not to act under the law, "a gas when ignited absorbs rays of the same refrangibility as those it emits." But in fact the substances hitherto investigated in the sun are really metallic vapours, hydrogen probably coming under that rule. The non-metals obviously may behave differently. It is easy to speculate on the causes of such behaviour, and it may be suggested that the reason of the non-appearance of a dark line may be that the intensity of the light from a great thickness of ignited oxygen overpowers the effect of the photosphere just as if a person were to look at a candle flame through a yard thickness of ignited sodium vapour, he would only see bright sodium lines, and no dark absorption lines. Of course, such an explanation would necessitate the hypothesis that ignited gases such as oxygen give forth a relatively large proportion of the solar light. In the outburst of *T Coronæ* Huggins showed that hydrogen could give bright lines on a background of spectrum analogous to that of the sun.

However all that may be, I have no doubt of the existence of substances other than oxygen in the sun which are only indicated by bright lines. Attention may be called to the bright bands near G, from wave-lengths 4307 to 4337, which are only partly accounted for by oxygen. Farther investigation in the direction I have thus far pursued will lead to the discovery of other elements in the sun, but it is not proper to conceal the principle on which such researches are to be conducted for the sake of personal advantage. It is also probable that this research may furnish the key to the enigma of the D_3 or Helium line, and the 1474 K or Corona line. The case of the D_3 line strengthens the argument in favour of the apparent exemption of certain substances from the common law of the relation of emission and absorption, for while there can be no doubt of the existence of an ignited gas in the chromosphere giving this line, there is no corresponding dark line in the spectrum of the solar disc.

In thus extending the number of elements found in the sun we also increase the field of inquiry as to the phenomena of dissociation and recomposition. Oxygen, especially from its relation to the metals, may readily form compounds in the upper regions of the solar atmosphere which can give banded or channeled spectra. This subject requires careful investigation. The diffused and reflected light of the outer corona could be caused by such bodies cooled below the self-luminous point.

This research has proved to be more tedious and difficult than would be supposed because so many conditions must conspire to produce a good photograph. There must be a uniform prime moving engine of two-horse power, a dynamo-electric machine thoroughly adjusted, a large Ruhmkorff coil with its Foucault break in the best order, a battery of Leyden jars carefully proportioned to the Plücker's tube in use, a heliostat, which of course involves clear sunshine, an optical train of slit, prisms, lenses, and camera well focussed, and in addition to all this a photographic laboratory in such complete condition that wet sensitive plates can be prepared which will bear an exposure of fifteen minutes and a prolonged development. It has been difficult to keep the Plücker's tubes in order; often before the first exposure of a tube was over the tube was ruined by the strong Leyden sparks. Moreover, to procure tubes of known contents is troublesome. For example, my hydrogen tubes gave a spectrum photograph of fifteen lines of which only three belonged to hydrogen. In order to be sure that none of these were new hydrogen lines it was necessary to try tubes of various makers, to prepare pure hydrogen and employ that, to examine the spectrum of water, and finally to resort to comparison with the sun.

The object in view in 1873, at the commencement of this research, was to secure the means of interpreting the photographs of the spectra of stars and other heavenly bodies obtained with my 28-inch reflector. It soon appeared that the spectra of nitrogen and other gases in Plücker's tubes could be photographed, and at first some pictures of hydrogen, carbonic acid, and nitrogen were made, because these gases seemed to be of greatest astronomical importance on account of their relation to stars, nebulæ, and comets. Before the subject of comparison spectra of the sun was carefully examined there was some confusion in the results, but by using hydrogen the source of these errors was found out.

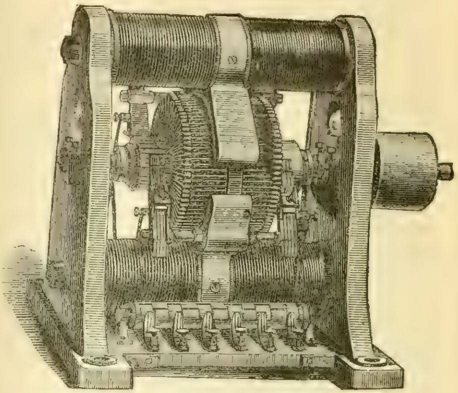
FIG. 1.—The Gramme Machine.

But in attempting to make a prolonged research in this direction, it soon appeared that it was essential to be able to control the electrical current with precision both as to quantity and intensity, and moreover to have currents which, when once adjusted, would remain constant for hours together. These conditions are almost impossible to attain with any form of battery, but on the contrary are readily satisfied by dynamo-electric machines. Accordingly, I sought for a suitable dynamo-electric machine and motor to drive it, and after many delays procured a combination which is entirely satisfactory. I must here acknowledge my obligations for the successful issue of this search to Prof. George F. Barker, who was the first person in America to procure a Gramme machine. He was also the first to use a Brayton engine to drive a Gramme.

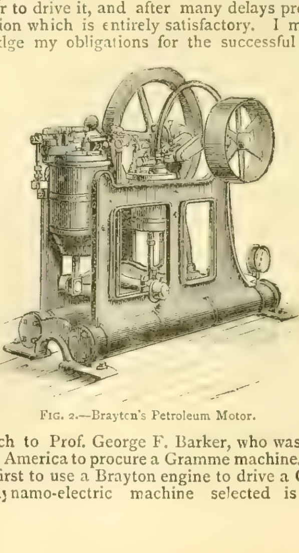
FIG. 2.—Brayton's Petroleum Motor.

The dynamo-electric machine selected is one of

Gramme's patent, made in Paris, and is a double light machine, that is, it has two sets of brushes, and is wound with wire of such a size as to give a current of sufficient intensity for my purposes. It is nominally a 350 candle-light machine, but the current varies in proportion to the rate of rotation, and I have also modified it by changing the interior connections. The machine can produce as a maximum a light equal to 500 standard candles, or by slowing the rotation of the bobbin the current may be made as feeble as that of the weakest battery. In practical use it is sometimes doing the work of more than fifty large Grove nitric acid cells, and sometimes the work of a single Smee.

The Gramme machine could not be used to work an induction coil when it first reached me, because when the whole current was sent through the Foucault interruptor of the Ruhmkorff coil, making 1,000 breaks per minute, the electro-magnets of the Gramme did not become sufficiently magnetised to give an appreciable current. But by dividing the current so that one pair of the metallic brushes, which collect from the revolving bobbin, supplied the electro-magnets, the other pair could be used for exterior work, no matter whether interrupted or constant. The current obtained in this way from one pair of brushes when the Gramme bobbin is making 1,200 revolutions per minute is equal to 100 candles, and is greater in quantity and intensity than one would like to send through a valuable induction coil. I usually run the bobbin at 622 revolutions per minute, and this rate will readily give 1,000 10-inch sparks per minute with the 18-inch coil. Of course a Plücker's tube lights up very vividly and generally; in order to get the maximum effect I arrange the current so that the aluminium terminals are on the point of melting. The glass, particularly in the capillary part, often gets so hot as to char paper. The general appearance of the machine is shown in Fig. 1.

As long as the Gramme bobbin is driven at a steady rate the current seems to be perfectly constant, but variations of speed make marked differences in the current, and this is especially to be avoided when one is so near the limit of endurance of Plücker's tubes. A reliable and constant motor is therefore of prime importance for these purposes. A difference of one per cent. in the speed in the engine sometimes cannot be tolerated, and yet at another time one must have the power of increasing and diminishing the rate through wide limits. The only motor, among many I have examined and tried, that is perfectly satisfactory, is Brayton's Petroleum Ready Motor.

This remarkable and admirable engine acts like an instrument of precision. It can be started with a match, and comes to its regular speed in less than a minute; it preserves its rate entirely unchanged for hours together. Moreover, it is economical, cleanly, and not more noisy than a steam engine. The one of two-horse power I have, ran for six months, day and night, supplying water and air to the aquaria in the Centennial Exhibition at Philadelphia. At any time on going into the laboratory it can be started in a few seconds, even though it has not been running for days.

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New York

THE NATURAL HISTORY OF THE JENISSEI

AN account of the Swedish Overland Expedition to the Jenissei in the summer of 1876, the cost of which was defrayed by Mr. Oscar Dickson of Gothenburg, has appeared in the *Göteborgs Handels Tidning.* The expedition was under the leadership of Dr. Hjalmar Théel of Upsala, who was accompanied by Botany-Docent W. Arnell, Philosophy-Candidate F. Trybom, Zoologist, and Rector M. Brenner, Botanist, from Finland. Docent Sahlberg, Entomologist, from Helsingfors, also went with the expedition to the Jenissei with the intention of prosecuting independent researches there. The party travelled by Nischni-Novgorod, Perm, Tjumen, Tomsk, and Krasnojarsk on the Jenissei, arriving at the last place on June 8.

We regret that our space permits of our giving only the following account of the natural history of the Jenissei by Dr. Théel:—

The Jenissei has a length of about 1,660 English miles below Krasnojarsk. The banks are sometimes pretty high and bold, sometimes low, alternating in this respect with each other, so that, when the left is high, the right is the opposite. Where the bank is low and exposed to inundations, willows thrive beyond everything. The high banks are clothed with *Pinus obovata* and *cembra*, and larch. At Jeniseisk the river is about 1½ versts broad, gradually widening northward, till at Kurejka it is five versts broad. Between Tolstonos and Goltschika the river widens and assumes the appearance of a lake more than sixty versts wide. Here the tides are quite observable. At Dudinskoj a depth reaching twelve fathoms was found.

The Russian population of the Jenissei Valley is very sparse and uncivilised, and inferior, as far as the fine arts are concerned, to some of the Asiatic races. Cattle rearing is in its infancy, though there are perhaps few regions more suited for it than the valley of the Jenissei. Cows are met with as far as Dudinskoj, but their proper management did not appear to be understood. At villages on the upper Jenissei, with as many as forty or fifty cows, a glass of milk could scarcely be obtained. The making of cheese is completely unknown, the making of butter nearly so. There are horses as far north as Dudinskoj, sheep only to Vorogova, and no goats north of Jeniseisk. Cultivation is at a still lower standpoint, rye not being at present grown below Antsiferova, sixty-seven versts north of Jeniseisk, and oats to Zotina, 60° 55′ N. lat. Potatoes are grown to Turuchausk, but are there very small. For some years Skoptzi settled on the Chantajka river, 68° 20′ N. lat., have successfully grown potatoes.

Fish forms the principal food of the people, and during summer nearly every one is a fisher. Fishing is carried on with various kinds of nets, with lines and hooks, and even with leister and torch. There are found in the Jenissei pike, ruffe, perch, burbot, *Cyprinus curassius*, tench, *Thymallus vulgaris*, several species of the family *Leuciscus*, among them one which strongly resembles our common roach, a kind of *Petromyzon*, *Gasterosteus pungitius*, a kind of bullhead (*Cottus*), &c. All these are of inferior importance for domestic use, and mostly serve as food for dogs. The more valuable are the sturgeon, salmon, and coregonus. There are two varieties of sturgeon, the common sturgeon or "Ossetrina," *Accipiter sturio*, and the sterlet, *Ac. ruthenus*. The *Ossetrina* is caught along the whole Jenissei, and sometimes reaches a weight of 225 lbs. The sterlet is not found north of Dudinskoj, and commonly weighs 3 or 4 lbs., but sometimes reaches 18 lbs. There is another called the prickly sturgeon, "Kosterska," believed to be the young of the *Ossetrina.* There are many varieties and transition forms of sturgeon, rendering their proper classification difficult. The salmon is most numerous in the upper course of the river at Minousinsk, where a profitable fishery is carried on. Two types are distinguished, "Tajmen" and "Kunschja." The former is caught in greatest numbers in the upper course of the river, and weighs 40 to 60 lbs.; the latter is found in lakes on the *tundra*, and very seldom in the Jenissei below Dudinskoj. At the Nichandrovska Islands a salmon, probably a Tajmen, was caught, which was nearly five feet long and weighed between 80 and 100 lbs. Of the Coregonus the following species were found in the Jenissei:—Njelma (*C. leucichthys*), Tschir

(*C. nasutus*), Muksun (*C. muksun*), Peljedka (*C. pelet*), Omul (*C. omul*), Common Siklöja or Seldj (*C. albula?*).

The common Coregonus is said to be found in the Jenissei the whole year round. The Tschir, Njelma, and Muksun are seen almost simultaneously in early spring, the Tschir first beginning to ascend, and then the other two almost simultaneously, or the Njelma rather earlier. Finally masses of the Siklöja, and last of the Omul, make their appearance. These seldom go above the rapids between Podkamennoje Tunguska, and Asinova. There is no accurate information about the *Pelet*, but it does not appear to go far from the mouth of the river.

The bird world was sparingly represented on the Jenissei. In the south the *Passeres* were most numerous. In the neighbourhood of the limit of trees on the *tundra* and at the Briochovska and Nichandrovska Islands the swimming birds and waders first became more numerous. *Colymbus septentrionalis*, *Harelda glacialis*, *Oidemia fusca* and *nigra*, *Fuligula marila*, *Anas pœnelope* and *acuta*, and *Cygnus bewickii* occurred here in great numbers, but with a few others were the only species of the order *Natatores* that could be found in those northern regions. A number of birds, for instance geese, *Anser segetum* and *albifrons*, and swans, occur first at the period of migration in autumn, when the uncommon red-necked goose, *Anser ruficollis*, is also met with not unfrequently. Altogether 140 to 150 species, of which only fifteen to twenty were extra-Scandinavian, have been observed during the summer, among them about twenty-five *Natatores* and twenty *Raptores*. It is singular that, for instance, at Tolstonos, 69° 55′ N. lat., accordingly beyond the limit of trees, many small birds belonging to the order *Passeres* occur. Schmidt there found ten species, to which number we are able to add four more, viz., *Fringilla linaria*, *Emberiza pusilla*, *Saxicola œnanthe*, and *Phyllopneuste trochilus*. A number of birds in Siberia are found drawing more and more to the west. It is stated, for instance, that the species, *Alauda alpestris*, *Emberiza rustica*, and *pusilla*, &c., which formerly could only be met with in Siberia and Eastern Russia, are now found in Finland and Western Russia, and indeed even within Scandinavia. In the time of Pallas the Ural formed the western limit of the *Emberiza aureola*, which is now common in the whole north of Russia. It is therefore not impossible that part of the birds at present peculiar to North Russia and Siberia may in the future belong to the fauna of Sweden. Trybom states that at Krasnojarsk, the insect fauna was abundant and very unlike the Scandinavian. As the party descended the river the insects diminished in number more speedily than could have been expected, those strangest to the Swedes generally disappearing first. Where the wood had been burned lately, and vegetation had not been able to regain its ordinary condition, the insect fauna was also poor. Compared with the phanerogamous plants occurring within the same area the Scandinavian insects taken overhead are immeasurably more numerous than those of the Jenissei River Valley. It is on the *tundra* that the most which are common to Scandinavia are found. In the collections made the *Coleoptera* are most numerously represented, then the *Hymenoptera*, *Diptera*, *Lepidoptera*, *Neuroptera*, and *Orthoptera*. The number of species collected is believed to exceed 1,000, among them about fifty kinds of diurnal *Lepidoptera*. Of these two-thirds are Scandinavian. Of the insects collected about Krasnojarsk only the half are Swedish. The four species (*Colias palæno*, L., and *Boothii*, Ross, *Pieris napi*, L., and *Argynnis pales*, W. V.) found at the Nikandrovska Islands are all Swedish. The distribution to the north or south was for many species different from that in Scandinavia. Thus *Pararga hiera*, Hübn., ceased there at least three degrees farther south than in Sweden. *Lycæna acis*, Ochs., was not found farther north than at Nikulina, 60° 25′ N. lat. *Pieris daplidice*, L., was, on the other hand, found as far as Fatianova and *Lycæna argiolus*, L., at Turucharsk (65° 55′ N. lat.), *Heteropterus sylvius*, Knock., which was very common as far as Krasnojarsk, was also found there. *Argynnis aphirape*, Hübn., is found in Sweden six degrees farther south than it was seen on the Jenissei, but *Arg. freja*, Thbg., on the contrary, two degrees more to the south in the latter place. *Polyommatus helle*, W. V., is pretty common on the Jenissei, two degrees farther south than in Sweden.

Arnell states that the moss flora of the Lower Jenissei, like that of the whole of Siberia, generally may be said to be almost completely unknown to science, only eighteen species of mosses being previously known. The number collected during the expedition may, perhaps, as far as may be judged before the material is thoroughly worked out, be reckoned at about 300, many being foreign to Scandinavia and many even new to science. The most peculiar localities on the Lower Jenissei are the tree stems on the banks, which are periodically overflowed. The stems receive a coating of earth often to a height of many feet above the ground, and form an excellent locality for mosses. Here are found not only some Scandinavian mosses as *Leskea polycarpa*, *Myrinia*, *Amblystegium riparium*, *Fontinalis hypnoides*, *Neckera undulata*, *Homalia trichomanoides*, *Pylaisea* (in an unending variety of forms), &c., but also and especially by two non-Scandinavian mosses, namely, the uncommon genuine *Timmia negapolitana* and an exceedingly pretty *Eurhynchium concinnum*, formerly referred to *Mynrella* or a peculiar family of *Achrolepis*, but undoubtedly belonging to *Eurhynchium*.

The masses of decayed stems found in the forests in incomparably larger numbers than in Scandinavia form another peculiar locality. They are characterised especially by *Dicrana fragilifolium* and *fuscescens*, which here exhibit all possible transition forms to each other, and a number of *Hepaticæ*, part of them foreign to Sweden. Mountain localities, especially with primitive rocks, are seldom met with on the Jenissei. The mosses peculiar to primitive rocks were found very sparingly. *Grimmia* and *Rhacomitrium* were seldom met with. Of these two families *Grimmia apocarpa*, which is not particular as to what it grows on, is the only species which in some degree is distributed over the region. Only once was a *Rhacomitrium* found in the whole distance from Krasnojarsk to the limit of trees, about 1,660 miles; first north of the limit of trees the family began to take to some extent the place it has in Sweden. The moss flora of the Lower Jenissei may be said to be specially characterised by its richness in *Mnia* and *Marchantiaceæ*. The following were also richly represented :—*Splachnaceæ* (with eight species), *Polytrichum*, *Bryum* (particularly towards the north, in very beautiful forms, partly new), *Webera*, *Dicranum*, *Encalypta* and *Sphagnum*, &c. Some of the greatest Scandinavian rarities were found, as *Orthothecium intricatum*, *Mynrella julacea* and *apiculata*, *Hylocomium oakesii*, *Pogonatum capillare*, *Oligotrichum lærigatum*, *Cinclidium subrotundum*, *all with fruit*. Enormous masses, in which two species of *Riccia* occurred, were found close to the water's edge on the clay banks inundated during summer the whole way from the Nikandrovska Islands to Jeniseisk. In Scandinavia the northernmost representative of this family does not go farther north than about the sixty-second degree of latitude. Extensive collections of fresh-water algæ were also made, but no detailed account of them can yet be given.

Arnell states that somewhat over 700 herbaceous plants were collected during the summer at about sixty different points on the Jenissei; of these about 200 are foreign to Sweden. Several families and races, as *Gymnospermæ*, *Androsace*, *Pedicularis*, and *Anemone* are richer in species on the Jenissei than in Scandinavia; the following races numerously represented in Sweden are sparingly met with on the Jenissei :—*Hieracium*, *Campanula*, *Veronica*, *Tri-*

folium, *Geranium*, *Sedum*, &c. The forests which clothe the banks of the Jenissei consist to a great extent of non-Scandinavian trees, namely, of the larch, *Pinus cembra*, *Abies sibirica*, and the *Abies obovata*, which scarcely differs from *Pinus abies*, and of *Populus nigra*. Of the trees common to Scandinavia the most important are the birch (*Betula verrucosa* and *glutinosa*), pine, aspen, bird's cherry, and mountain ash. Besides, the *Salices* play a very important part on the Jenissei, inasmuch as they form extensive woods on the low banks periodically overflowed; these *Salices* often grow to uncommonly large sizes, and consist in part of non-Scandinavian species, one of which, *Salix vitellina*, is the most common of all, and spreads over the whole of the region examined by the botanists of the expedition.

The bush vegetation too in Siberia exhibits differences from that of Scandinavia. On the Jenissei *Alnaster fruticosus* is important beyond others, because it forms thickets, and especially towards the north increases in mass, going in that direction beyond the limit of trees. Among other bushes foreign to our flora there occur on the Jenissei the Siberian pea tree (*Robinia*), *Spiræa confusa*, *sorbifolia* and *salicifolia*, *Sambucus racemosa*, *Cratægus sanguinea*, *Cassandra calyculata*, peculiar types of roses, &c. Among the bushes common to Scandinavia the most important are the black and red currant, dwarf birch, *Lonicera cærulea*, which is far more widely distributed than in Sweden, juniper, *Myrica gale*, raspberry, *Empetrum nigrum*, *Vaccinium vitis idæa*, and *myrtillus*, &c. Towards the north the bushy *Salices* play an important part, as in our northern regions. On the Jenissei there has been found only one species of *Alnus*, which is specially interesting as not being either of the Swedish species, but perhaps the genuine *Alnus pubescens*. On the other hand there are absent on the Jenissei many of our trees and bushes, as the nobler deciduous trees and fruit trees, and, what may be said to be distinctive of the Jenissei flora, heather, which is so extensively distributed in Sweden, is wanting.

TEMPERATURE AND HUMIDITY OF THE AIR AT DIFFERENT HEIGHTS

A MEMOIR on the temperature and humidity of the air at different hours, by Dr. H. E. Hamberg, based on observations made by him during the summer of 1875, at heights varying from 2 inches to 22 feet above the ground, was published recently in the *Transactions* of the Royal Society of Sciences at Upsal. The memoir is a valuable one, and is of interest to more than the mere meteorologist, it being evident that the inquiry is so handled as to bring it into close connection with such difficult questions as convection currents in the free atmosphere and the diffusion of vapour through the air.

In clear weather the temperature of the air nearest the surface was lower than that above it, from two to three hours before sunset to at least two to three hours after sunrise. At all the six heights the temperature fell to the minimum at the same hour, viz., about 3 A.M.; but while it continued from this time to rise steadily at all the heights, the lowest temperatures continued to be observed in the strata nearest the ground till several hours after sunrise. From this remarkable result Dr. Hamberg concludes that the increase of temperature in the lower strata of the air in the early part of the forenoon is not an immediate and direct consequence of the heating of the ground, but is rather to be attributed to the absorption by the air, or more strictly by its aqueous vapour, of the heat received from the sun's rays or reflected from the ground.

Over uneven ground covered with vegetation the temperature near the surface is generally higher over those parts of the field which rise above the general level. Thus even slight elevations of only one or two feet have the air immediately resting on them often 2° higher or more, whilst on the other hand, a trench or depression one or two feet below the general level has the air resting on it often 2°, or more, lower than the air over the level portions of the field, a result of considerable practical importance in agriculture and horticulture.

The latent heat set free on the formation of dew appears from the observations clearly to retard the lowering of the temperature, but not to the extent which might have been expected. When, on the deposition of dew, the temperature of the air near the surface has fallen below 32°, as soon as the dew is congealed into hoar-frost the temperature of the lowest stratum of air in contact with the ground instantly rises to 32°; but at the same time the temperature of the air higher up steadily remains lower than 32°.

The absolute humidity of the air on clear nights on which no dew is deposited decreases from the ground upwards, just as happens during the day; but on the other hand, with dew, the humidity is least nearest the ground, and increases with the height, and this influence of dew, in diminishing the humidity, extends upwards to at least twenty-two feet, the height to which the observations were carried. Since his observations clearly show that the absolute humidity begins in the evening to diminish near the ground before any dew is observed to be deposited, and also diminishes at all heights on those nights during which no dew whatever is formed, Dr. Hamberg is of opinion that the diminution of the humidity of the air during night is to be sought for in other physical causes than the deposition of dew.

Several of the points discussed will doubtless be made subjects of further investigation by others. In all cases it is most desirable, indeed absolutely necessary, to a critical valuation of the observations, that the authors give woodcuts and descriptions of the exact position and mode of protection adopted in the case of each thermometer employed in the observations. For such refined inquiries the method of observation must necessarily be a refined one; in other words, such as will certainly secure the necessary comparability among all the instruments.

THE CHRONOMETERS OF SWITZERLAND

WE find in a recent number of the *Bulletin* of the Society of Natural Sciences at Neuchâtel an interesting report of the Neuchâtel Observatory on the annual competition of chronometers for prizes awarded yearly by the Observatory. The report gives some idea of the degree of perfection reached in Switzerland in the construction of chronometers. The 231 chronometers (four box and 227 pocket chronometers) admitted to the competition out of 270 sent in were submitted to a severe test, including changes of temperature and of position during periods of from two weeks to two months, and the results of the trial appear as follows. The average diurnal variations in the rates of the box-chronometers proved 0·32 sec. (against 0·20 in 1874), and of the pocket ones 0·46 sec. (against 0·53 in 1874), and there was but two per cent. of these last, the average diurnal variation of which reached 1 sec. The various systems of escapements appeared, as was observed in former years, to have but little if any influence on the degree of precision of watches, provided they remain constantly in the same position, vertical or horizontal. The compensation for changes of temperature proved to be altogether satisfactory, the average variation of 167 chronometers submitted to variations of temperature from 15 to 25 Cent. degrees being but 0·13 sec. for each Centigrade degree. It must, however, be noticed that only 51 per cent. of them showed variations below one-tenth of a second for each degree, and that 10 per cent. showed variations above 0·3 sec. Finally, the differences between the rates during the first and the last weeks were: for box-chronometers, 2·11 sec.

after a period of two months; and for pocket-chronometers, 1·57 sec. after a trial of six weeks; and the average differences between the maximum and minimum rates proved to be, for box-chronometers, 3·23 sec., after a two months' trial; and for pocket-chronometers, 6·22 sec. and 5·75 after six weeks and one month's trials. These figures show certainly that there is enough to do yet in raising the industry to the high degree of perfection which is desirable, but the steady progress during the last ten years is remarkably seen in a table showing the increase of precision of the Swiss chronometers in every direction. Thus the average diurnal variation, which was as high as 1·27 sec. at the competition of 1864, regularly decreases to 0·46 in 1875; the average variations of rate produced by changes of position, being 8·21 sec. ten years ago, is now but 1·97; and the defective compensation for temperature rapidly decreases from 0·48 sec. for each degree to 0·16, and now it is but 0·13. Besides, the report states, some of the best chronometers reach as high a degree of perfection as to make them comparable with astronomical clocks. Thus the box-chronometer which received the first prize is a true phenomenon of its kind. Its mean diurnal variation is as low as 0·08 sec., *i.e.*, that of good astronomical clocks; its mean weekly rate changed after a two months' trial only by 0·57 sec.; the difference between the maximum and minimum rates is but 0·94 sec., and the imperfect compensation for temperature is 0·04 sec. for each degree; finally, its characteristic number, calculated by the Greenwich method, reaches but 8·90 sec. The two best pocket-chronometers realise perhaps a yet greater success, their average diurnal variations being respectively but 0·13 and 0·17 sec.

THE BRITISH ASSOCIATION

THE two *soirées* that were held in the Guildhall, the first on the evening of Thursday, the 16th instant, and the second on Tuesday, the 21st, were very fully attended.

At the second *conversazione* several objects of scientific interest were exhibited. At the centre table Prof. Herbert McLeod showed his beautiful cycloscope, an instrument which formed the subject of a paper read by Prof. McLeod before Section G on Wednesday week. Mr. Silvanus P. Thompson, of University College, Bristol, showed his apparatus for exhibiting certain optical illusions, upon which a paper was read by him in Section A. Prof. Osborne Reynolds showed the apparatus by which his paper upon the rate of progression of groups of waves was illustrated; and Mr. J. W. Swan exhibited a modification of the Sprengel pump.

In the picture gallery Dr. Graham Bell had his articulating telephones at work.

There was great competition for the tickets for the excursions for both Saturday and Thursday. The excursion to Lee Moor under the guidance of Mr. Spence Bate, F.R.S., was originally limited to 100, but there were more than 300 applications for tickets, and extra waggonettes had to be put on. The party, after having visited the China Clay Works of Messrs. Martin, the largest establishment of its kind in the world, divided into three parties: the first walked across the Moor to Sheepston, to examine some prehistoric remains recently discovered by Mr. Spence Bate. Another party under the charge of Mr. Martin took a walk to Shell top and Pen Beacon, from which fine views may be had; and a third detachment remained in the grounds of Mr. Martin, which are unique in their way, from the intricacy of their laying out.

The popular excursion of the day was, however, that up the Hamoaze and Tamar, to H.M.S. *Cambridge*, under Great Albert Bridge at Saltash, into the Sound, and visiting the Breakwater and Eddystone Lighthouse. The Admiral of the Port placed three Government steamers at the disposal of the Association, and there was tremendous crowding to get on to the boats. Upon reaching H.M.'s gunnery ship *Cambridge*, the gunnery and torpedo practice began, and some splendid feats of firing at long ranges were exhibited.

On the same day there was a dredging excursion under the superintendence of Mr. Gwyn Jeffreys and Mr. Hearder.

While these excursions were going on a select party was, at the invitation of the Mayor and Corporation of Exeter, visiting that ancient city. At the luncheon, the toast of "The British Association," was proposed by Sir Stafford Northcote, Chancellor of the Exchequer, and responded to by Mr. Spottiswoode, F.R.S., the President-elect.

The excursions on Thursday last were first to Liskeard, the Cheesewring, and the Caradons, at the invitation of the Mayor of Liskeard. The second excursion was by way of the Tamar to Morwellham to the celebrated Devon Consols Copper Mines, taking on its way the fine old mediæval mansion of Cotehele, which was thrown open to the members by the Earl of Mount Edgcumbe, to whom it belongs. The last of Thursday's excursions was to Totnes, Torquay, and Brixham, and like the Exeter excursion, was only by special invitation. It was divided into four sub-excursions (*a*), Archæological, visiting Totnes and Berry Pomeroy Castles; (*b*), Mechanical, visiting the Experimental Works of Mr. Froude, F.R.S.; and (*c* and *d*), Antiquarian and Geological, the first to Kent's Cavern, under the guidance of Mr. Vivian, and the second to Brixham, with Mr. Pengelly, F.R.S.

The Plymouth meeting of the British Association for 1877 has been decidedly a quiet one; its attendance as a whole has been below the average, and its funds are proportionately low; but it has done good work, and it has been marked by several papers of great scientific interest. The discovery by Prof. J. C. Adams of the original papers of Newton respecting the rotation of the apse of the moon, the exhibition of the articulating telephone of Dr. Graham Bell and the very valuable suggestions contained in the address of Prof. Carey Foster, must all help to mark the Plymouth meeting in the annals of the Association as a valuable one, notwithstanding its failure in points of attendance and pecuniary position.

The following are some of the figures connected with the recent meeting:—

Number of tickets issued to Old Life Members ...	161
,, ,, ,, New ,, ,, ..	19
,, ,, ,, Old Annual Subscribers	238
,, ,, ,, New ,, ,, ...	58
,, ,, ,, Associates	447
,, ,, ,, Ladies	283
There were also present, of Foreign Members	11
Making a total of	1,217

The total receipts from the sale of tickets amounted to 1,267*l*.

REPORTS.

Prof. O. Reynolds presented the *Report of the Committee appointed to consider what Effect Reversing the Screw had on the Steering of a Steamer under Full Way.*—Since the last meeting of the Association the Committee had carried out further experiments, and the results now obtained show that the larger the ship the more important the effect of reversing the screw became. In answer to the request of the Committee, the Admiralty had made a trial with H.M.S. *Speedy*, but the conditions under which it was conducted precluded the possibility of more light being thrown on the subject. The greatest speed was five knots, and the effect of the rudder with the screw reversed was so small that the vessel in most instances turned her forward end into the wind. The Admiralty had been urged to have experiments made with larger and more powerful ships, but as yet had not assented

The Committee forwarded copies of their last year's report to the Admiralty, the Board of Trade, the Trinity House, and other corporations, but no intimation had been received as to any action being taken upon it. The report was discussed at the conference of the Association for the Reform and Codification of the Law of Nations last year at Bremen, where a resolution was agreed to declaring that the existing international rules for preventing collisions at sea were not satisfactory, and it was desirable the governments of maritime states should take counsel together with a view to amend the rules and adapt them more carefully to the novel exigencies of steam navigation. This showed that the subject had already attracted considerable attention, and it was important to notice that the conclusions of the Committee had not yet in the smallest degree been controverted. Numerous collisions had happened during the year, which, to judge from the law reports, might in many instances have been avoided had the effect of reversing the screw been known and acted upon; but it did not appear as if a consideration of this had influenced any of the judgments given. The collisions had for the most part been with small ships, and so had not come much into notice. The loss of the *Dakota*, however, was a disaster of the first magnitude, and would unquestionably cause the subject to be considered by the authorities.

Report of the Committee for commencing Secular Experiments on the Elasticity of Wires.—The Committee have been chiefly occupied with preliminary arrangements and preliminary experiments.

A room has been fixed upon in the tower of the University buildings in Glasgow for suspending wires for the secular experiments. In this room there is an available height of sixty feet. A tube of cast-iron, within which the wires are to be hung, is at present being erected, and will be ready in two or three weeks. The tube is to be 60 feet high and 9 × 4½ inches in cross section.

Wires of gold, platinum, and palladium have been supplied by Messrs. Johnson and Mathey, and with these it is proposed to commence the secular experiments. These wires have been specially drawn for the Committee. Each of them weighs one grain per foot.

A cathetometer suitable for making observations on the wires after they are hung up in their place has been designed and is being constructed by Mr. James White, instrument-maker, Glasgow. Preliminary experiments have been undertaken for the purpose of determining Young's modulus, and the breaking weight of the gold, platinum, and palladium wires.

Some experiments have also been undertaken in connection with the subject under investigation as to the effect of continued application of force on the breaking-weight of steel wire and soft iron wire, and results of importance have been obtained. These experiments are still being carried on, and numerical results will be given in a future report. It is found that when a weight nearly as great as the breaking-weight is kept for a long time—several days, for instance—and applied to pull out a soft iron wire, the effect is to increase largely the strength of the wire. It is often increased by as much as 6 or 7 per cent.

Report of the Committee on Luminous Meteors, by J. Glaisher.—The Committee have to record a year of very active research and of diligent and successful observations of shooting stars, fire-balls, and aërolites since the last report. The toilsome work of mapping and projecting star showers, and comparing and arranging the radiant point in lists, has occupied so much attention that they have been obliged to postpone till next year the work of furnishing observers with a *résumé* of the known star showers. The autumn and winter months were marked by numerous large fire-balls observed in England and abroad, some of which are of very special interest. Two, if not more, aërolites have fallen in America, and one at Constantine, in Algeria. Besides the magnificent meteor seen in the United States on December 21 last, from which one of these aërolites was projected, an equally splendid aërolite passed over Cape Colony on March 16 last with loud explosions, but no aërolites are known to have fallen from it in its flight. Much of the attention of the Committee has been engaged in the continued examination and comparison of star showers, and valuable work has been performed by Mr. W. F. Denning. There have been no marked star showers for one or two years, but some examples of frequency on certain nights have occurred. The August shower of 1876 and of the present month have both been below the average. The work of the Committee has, as in former years, been chiefly performed by Prof. A. S. Herschel.

The Report of the Committee appointed to consider the Ordnance Datum Level.—After detailing the various causes which they found had led to the uncertainties referred to in the communications made in 1875, the Committee came to the following conclusions:—1st, That of the two tide gauges at Liverpool, now purporting to refer to the old dock sill, the zero of that fixed at the south-east corner of the Canning dock was about 5·64 inches above that on the river face of the Canning Island, Liverpool. 2nd. That in order to reconcile the statement in the ordnance book of levelling, "that the datum level for Great Britain is 8-10ths of an inch above the mean tidal level obtained from the records of the self-recording tide-gauge on the St. George's Pier, Liverpool," with the usual facts which the Committee have collected, it is necessary to bear in mind that the records of the self-acting gauge referred to were the observations of one month only of the year 1859, and that the mean tide of that period was 7·8 inches below the mean decade from 1864 to 1873. 3rd. That the difference of levels between the old dock sill and the ordnance datum, given in the ordnance book of levelling as 4·67 feet, is correct on the assumption that the zero of the gauge on the face of the Canning Island, and not that of the gauge in the Canning dock, be taken as the correct level of the old dock sill, and that, as stated in the ordnance book of levelling, the ordnance datum be taken at 8-10ths of an inch above the mean tide level of the month of March, 1844, as ascertained by the ordnance department. 4th. That it is thus apparent that the ordnance is an entirely arbitrary level, and could not be again obtained from tidal observations. The committee had thought it advisable to take advantage of the present inquiry in order to obtain information as to some of the various local datum marks in use in the British Isles, and to endeavour to ascertain the difference of each relatively to the ordnance datum, which would thus be the means of comparison between them. In order to enable the Committee to carry out this work they begged to be re-appointed.

Report on the Conditions under which Liquid Carbonic Acid exists in Rocks and Minerals, by W. N. Hartley, F.R.S.E.—In a paper read at the Glasgow meeting of the Association, Mr. Hartley described the method of determining the exact temperature at which the carbonic acid sometimes found inclosed in minerals becomes gaseous. This temperature is called by Prof. Andrews the critical point, and has been determined by him in the case of pure carbonic acid prepared artificially to be 30°·92 C. Mr. Hartley gives a table showing the critical point of carbonic acid inclosed in various minerals in which certain variations from Dr. Andrews' number are apparent; these, however, may be accounted for when the critical point is below the normal point by the carbonic acid being mixed with some incondensible gas like nitrogen.

It seemed desirable to ascertain whether the presence of liquid carbonic acid in rocks was not of frequent occurrence, and whether the immense number of cavities dispersed through various minerals which are usually considered to contain water may not often contain liquid carbonic acid, or whether the occurrence of this body is characteristic of certain formations. A considerable number of minerals was examined, including sapphires, zircons, garnets, topazes, and sections of fluor spar. Incidentally the inquiry led to some very interesting results concerning the motion of the bubbles in fluid cavities when influenced by some source of heat, of which the following is a summary:—

1. The bubbles in certain fluid cavities approach a source of heat brought near them.
2. The bubbles in certain cavities recede from the source of heat.
3. That 5° C. rise of temperature suffices to cause the apparent attraction.
4. That a rise of ½° C. will in some cases cause the apparent repulsion.
5. That in certain cases a bubble which receded from the source of heat at ordinary temperatures approached it when raised to 60° C.; the source of heat always being from ½° C. to 5° warmer than the specimen.
6. That this could occur in cavities containing liquid carbonic acid as well as water, but that it made no difference whether the carbonic acid was raised above its critical point or not.

Mr. Hartley has also examined a remarkable vibration of minute bubbles in fluid cavities first noticed by Mr. Sorby. It was found that these bubbles approached a warm body brought near them, and that they ceased moving, and clung for some time to the warmer side of a cavity. The conclusion arrived at for these phenomena is as follows:—It is impossible to imagine a body which is not gaining or losing heat, or at the same time gaining and losing heat; it is therefore impossible to imagine it as entirely throughout of a uniform temperature. It is evident, then, that an easily movable particle, which can be set in motion by exceedingly slight differences in temperature, will make the transference of heat from one point to another plainly visible. The minute bubbles in the cavities are such particles, and these vibratory motions afford ocular demonstration of the continual passage of heat through solid substances. A further continuation of the research was extended to the conditions under which solid particles exhibit the Brownian movement.

Concerning the presence of liquid carbonic acid in minerals, Mr. Hartley finds that it is not of common occurrence, but only occasionally met with. He also describes in his report the means of demonstrating in certain cavities the continuity of the gaseous and liquid states of matter as shown by Dr. Andrews in his well-known experiments. Regarding the proportion of gaseous and liquid carbonic acid to water in the cavities, some important generalisations have been arrived at.

Mr. Hartley gives reasons in the report which cause him to fix the temperature of formation of the mineral in the case of topaz somewhere above 342° C., the critical point of water. In certain other cases in which the cavities differ in the nature of their contents, the water, he thinks, must at the time of their formation have been in the liquid state. It is possible to determine within certain limits the temperature which a rock or mineral has endured if liquefied carbonic acid is found inclosed in it.

Report on some Double Compounds of Nickel and Cobalt, by J. M. Thomson.—On attempting to prepare the so-called conjugated sulphate of nickel, cobalt, and potassium mentioned by Vohl (*Ann. Chem. Pharm.* lxv.), who assigns to it the formula $NiCoK_4(SO_4)_4 . 12H_2O$, it was found that the several fractions deposited consecutively form a solution containing molecular quantities of the simple potassio-sulphates of the two metals, possessed different colours, and showed also the property of dichroism to a remarkable degree. The operation having been repeated several times with a like result, it was determined to examine whether any regular replacement of the two isomorphous metals took place. Quantities of the two salts were dissolved in a sufficient quantity of water, and the resulting solution evaporated gently over a water bath at a temperature of 80°, the crystals being allowed to deposit in successive fractions.

The crystals of the conjugated salts are oblique prisms, having a tendency to modification when allowed to grow to any great size. The first fractions have a greenish grey colour when seen in the mass, showing the preponderance in them of the nickel potassic sulphate over the corresponding cobalt salt; the latter fractions, however, gradually become more crimson in colour as the cobalt potassic sulphate preponderates over the nickel salt. Details of analyses are given in the report, showing the different quantities of nickel and cobalt contained in the several fractions.

It is shown that Vohl's formula may be correct for isomorphous metals having a considerable difference in their atomic weights, but fails when two metals, such as nickel and cobalt, having the same atomic weights, occur in the conjugated salts, as they give rise to replacements requiring a very high molecular formula to express their constitution.

The examination of the optical properties of the several fractions possesses some interest. It was observed that the colours shown through the different axes passed in a direct order down the spectrum. In the first fractions the more highly refractive rays of the cobalt spectrum mingle with the green of the nickel, whilst in the latter, the two rays are those adjacent to each other in the cobalt spectrum.

That these salts or fractions are not merely isomorphous mixtures is shown by the fact that large crystals taken for analysis exhibit throughout the same dichroism. If, then, the phenomenon of dichroism is dependent on molecular constitution, as seems probable, it follows that all bodies exhibiting dichroism must be definite chemical compounds, and therefore the molecular formulæ of some of these must be far more complicated in their structure than is at present imagined.

Abstract of the Thirteenth Report of the Committee for exploring Kent's Cavern, Devonshire.—The Committee, in their Twelfth Report, read at Glasgow last year, brought up the history of their researches to the end of August, 1876. They have now the pleasure of continuing that history to the end of July, 1877. During the intervening eleven months the work has been continued without interruption, on the same method and under the same daily superintendence as heretofore. The workmen named in the Twelfth Report are still employed on the exploration, and continue to give unqualified satisfaction.

On November 2, 1876, Mr. Busk, a member of the Committee, visited the cavern, accompanied by one of the superintendents, when he inspected that portion of the work which was then in progress, as well as the principal parts where the exploration has been completed.

The researches continue to attract large numbers of visitors, most of whom are admitted by the authorised guide, who, under well-defined and strictly-observed regulations, conducts them through such branches of the cavern as are of general and popular interest, but not to those in which the work is in actual progress, or has not been begun. The superintendents have also had the pleasure of accompanying a large number of visitors, including men of all professions and of various countries.

The Bear's Den.—The chamber termed the Bear's Den measures about sixty-seven feet in length, from north to south nearly, from eight to thirty-eight feet in width, and from eight to fifteen feet in height, the last dimension being measured from the bottom of the excavation. The limestone roof is extremely rugged, fretted, and water-worn.

Adjacent to its western wall is a vast boss of stalagmite, which the superintendents have preserved intact on account of the inscriptions which crowd it. One of these, "William Petre, 1571," is of considerable interest on two accounts: 1, the date is, so far as is at present known, the earliest in the cavern, and the only one belonging to the sixteenth century; 2. Its genuineness can scarcely be doubted, as it is known that at the period in question there was a William Petre, a native of South Devon, quite a young man, and a nephew of Thomas Ridgway, who then resided on the estate in which the cavern is situate, and of which he was the proprietor. Moreover, in a lately discovered lease, dated 1659, and conveying "closes, ffields, or pieces of ground," mention is made of "one close called Kent's Hole," thus showing that the cavern was so well known about the middle of the seventeenth century as to render it probable that it was known also, at least, as early as towards the close of the sixteenth.

As the Rev. Mr. MacEnery broke ground in every part of the Bear's Den fifty years ago, its original condition can only be learned from the description of it which he has left, and which may be given in the following very condensed form:—

"The floor of the Bear's Den was studded with conical mounds of stalagmite supporting corresponding pendants from the roof. Fallen masses of limestone were strewed about, and some of them were incorporated in the crust. An irregular sheet of stalagmite, about a foot thick, overspread the floor, and was based on a shallow bed of indurated rubble containing tubes of stalactite collected in heaps in particular places, a great abundance of *album græcum*, an unusual proportion of bear's teeth, and an iron blade much corroded. Points of stalagmitic cones were observed to protrude upwards into the rubbly bed, and were found to rise from a lower sheet of stalagmite. The cones of this lower sheet were precisely under those of the upper, denoting that they were successively deposited from the same tubes above, but the lowermost set exceeded by double the thickness of the uppermost, and the depth of the stalagmite sheet was in the same proportion. The lower sheet extended over the entire area of the den, but the superincumbent bed of rubble, and its overlying thin sheet of stalagmite, 'thinned out' towards the sides. The removal of these partial beds displayed the entire surface of the lower sheet, which exhibited a most singular appearance. Over the whole area it was cracked into large slabs, resembling flags in a pavement. The upper sheet was not in the least fractured. The average thickness of the cracked sheet was about two feet. It possessed the hardness of rock, and but for its division into insulated flags it would have been almost impossible to pierce it.

"The first flag we turned over displayed a curious spectacle. Skulls and bones of bear, crowded together, adhered to its under surface. Flag after flag disclosed the same phenomenon; but in one place numerous skeletons lay heaped on each other; the entire vertebral column and its various other bones, even to the phalanges and claws, were discovered lying in their natural relation, in a state of preservation as if belonging to the same individual. The remains of bear prevailed here to the exclusion of all other animals. Some of the teeth were of the most dazzling enamel, and the bones of their natural fresh colour. Others, on the contrary, were of a darkish brown; even the enamel was of a greenish tinge. Owing to the induration of their earthy envelope, or their incrustation by stalagmite, few were extracted entire. Two skulls were buried in the stalagmite as in a mould, and were brought away in that state. In no case were the remains broken or gnawed by the jaws of carnivores. The long bones were generally found entire; and when observed broken, it was only mechanically from pressure. The bones were highly mineralised, heavy, brittle, and easy of fracture; and, when struck, rang like metallic substances."

The portions of the stalagmitic floor which Mr. MacEnery had failed to break up, chiefly adjacent to the walls and other confines of the Bear's Den, furnished the Committee with two good examples of the remarkable cracks of which he speaks. One of these was in the north-east corner, where a crack, about half-an-inch wide, extended from wall to wall, a distance of about twelve feet, passing quite through the stalagmite, which was nowhere less than two feet thick, but without faulting it in the slightest degree, or, so far as could be observed, in any way affecting the underlying deposit. Mr. MacEnery, however, states, though somewhat obscurely, that in some instances a derangement had taken place in the materials covered by the broken stalagmite. The second existing crack varies from ·25 to 2·5 inches wide, and passes completely through the boss of stalagmite already mentioned, but without faulting it. No such cracks appear to be mentioned by Mr. MacEnery as occurring elsewhere, nor have the Committee met with anything of the kind in any other branch of the cavern.

The ground broken by Mr. MacEnery extended to a depth of from eight to twenty inches over almost the entire area of the Bear's Den. As was his wont, he left the excavated materials almost where he found them, and there were amongst them a large number of specimens which had been overlooked or neglected, including 1 tooth of horse, 1 of fox, 2 teeth of deer, 4 of hyena, 4 of mammoth, upwards of 200 of bear, very numerous bones, especially of the vertebral column and feet, a crowd of fragments of bone, numerous balls of coprolite, and a few bits of coarse pottery.

It cannot be doubted that such cracks as Mr. MacEnery describes must be a probable source of uncertainty respecting the position and relative chronology of some of the objects found in the underlying deposit, especially where this deposit shared in the disturbance.

In accordance with Mr. MacEnery's description and the foregoing considerations, the deposit the Committee had to excavate was the breccia, with a small amount of cave-earth lying on it here and there.

The excavation in the Bear's Den was limited, as in other branches of the cavern, to a depth of four feet below the bottom of the stalagmite, and the limestone floor was nowhere reached.

The "finds" in the Den were 216 in number, of which 12 were in the stalagmite; 101 in the first or uppermost foot-level, 47 in the second, 32 in the third, 23 in the fourth, or lowest, and 1 in a small recess. Omitting those found in the stalagmite and the recess, 32 of the "finds" were in cave-earth, 65 in a mixture of cave-earth and breccia, and 96 in the breccia; whilst the matrix of the remaining 10 must be regarded as uncertain. The colour and other characters of the specimens, however, indicate with tolerable certainty to what beds and eras they belong.

Besides a considerable number of bones and pieces of bone representing every part of the skeleton, the specimens included upwards of 620 teeth of bear, 24 of hyæna, 10 of horse, 7 of fox, 5 of mammoth, 4 of lion, and 1 of wolf (!), or of dog (?). There were also 20 "finds" of coprolite and 11 flints.

Amongst the bones the skull of a bear may be mentioned, which, to re-quote the language of Mr. MacEnery, was "buried in the stalagmite as in a mould, and was brought away in that state." Many of the specimens are of considerable interest, but perhaps none of them differ so much from those mentioned in previous Reports as to require detailed description.

None of the flints found in the Bear's Den are of so much interest as many of those exhumed in other branches of the cavern, and described in previous Reports.

A pillar of stalagmite was met with, in November, 1876, under the following peculiar circumstances:—It measured about fifty-one inches in basal circumference, and three feet in height. The base was of nondescript outline, but everywhere above the pillar was rudely elliptical in horizontal section, and it measured thirty inches in girth at the height of one foot, where it least. When found, however, it was in two parts, having been divided along an almost horizontal plane, where it was thinnest. Each segment stood perfectly erect, but not one on the other; for though the bottom of the upper segment was on precisely the same level as the top of the lower, the upper portion had been moved westward to the extent of fifteen inches horizontally, and stood there on the breccia. It cannot be doubted that when the dislocation occurred the pillar had reached its full height, and the breccia had accumulated round it to the height of one foot; that is, it had reached the level of the plane of fracture. It is difficult to see how, by any possibility, the deposit could at that time have reached a greater height; and difficult also to understand how anything other than human hands could have shifted the upper segment and placed it so as to have preserved its erect position. On the other hand, it is just as difficult to see what motive man could have had for such a work. The whole, when found, was completely buried in the breccia, and the top of the upper segment was about a foot below the bottom of a thick remnant of the stalagmitic floor, which was intact and not cracked.

Rats, undoubtedly attracted by the candle grease dropped by the workmen, continue to present themselves wherever the work is in progress, irrespective of the distance from daylight.

The Tortuous Gallery.—As soon as the work in the Bear's Den was completed, the exploration of a narrow passage opening out of its southern end, and termed "The Tortuous Gallery," was begun. Its height varies from 15 to 6 feet, and its width from 1·5 to 4·5 feet. It proceeds in a southerly direction for about 23 feet, and then turns sharply towards the east. Ground had been broken, here and there, by the earlier explorers up to 11 feet from the Bear's Den. Everywhere farther in there was a continuous unbroken floor of stalagmite from 1·5 to 3·5 feet below the limestone roof. The underlying deposit was exclusively the breccia, or, so far as is known, the oldest the cavern contains. Its upper surface formed a continuous declivity, at a mean gradient of 1 in 2·5.

The "finds" met with in the Tortuous Gallery up to the end of August, 1877, were but fourteen in number, and the objects they contained were of but little importance. Six of them were in the first or uppermost foot-level—all near the entrance; two in the third; and six in the fourth—all at some distance from the entrance. They included, besides bones and bone chips, fourteen teeth of bear—some of them being in portions of jaws—and one tooth of horse. The latter was found on the surface, near the Bear's Den, with three bits of coarse, friable, black pottery.

On reviewing the work of the last eleven months, the superintendents cannot but express disappointment at not having found the very large number of choice specimens which Mr. MacEnery's glowing description had led them to expect in the Bear's Den. Nevertheless, the discoveries they have made not only justify his description, but show that in that branch of the cavern the osseous remains were almost entirely confined to the uppermost foot of the breccia, and mainly to its actual surface. So long as the lower levels remained untouched the belief that they were equally rich would naturally have prevailed; and it cannot be doubted that in disposing of this belief satisfactory work has been done.

No trace of *Machairodes latidens* has been met with since the Glasgow meeting.

Fifth Report of the Committee for Assisting in the Exploration of the Victoria Cave, drawn up by R. H. Tiddeman, secretary.—The work has been carried on almost continuously throughout the year until July 14, when the low state of the exploration fund rendered it advisable to give up working for the present. Prof. Busk has reported on the bones submitted to him. Out of 181 determined bones and teeth he reports of ox 46, deer 14, sheep or goat 16, hare 3–4, fox 5, bear 41, wolf 4, hyæna 30, rhinoceros 11, elephant 3, badger 7.

Of the ox one is *Bos primigenius*, the other probably *Bos longifrons*. Of the bears some are not unlike *Ursus spelæus*; others are undoubtedly grisly bear. The hyænas are, as usual, individuals of various ages. Rhinoceros is represented by at least eleven well-marked specimens, all of which are clearly referable to *R. leptorhinus*.

Three or four fragments of elephants' teeth occur. Fourteen specimens of deer belong to red deer, but there is no clear indication of reindeer.

A small ruminant, probably goat, occurs; some of the bones appear to be rather recent. Badger, fox, a small wolf, hare, rabbit, several birds, and water-vole, complete the list of those which have been determined from the bones obtained in the year.

A great part of the work this year has been expended in lowering the levels in chambers A and D. An adit has also been cut from the further end of Chamber A to the end of Chamber D. This part was completely filled up to the roof with several beds of clay and stalagmite. These were all of earlier age than the hyæna bed, which was the great deposit of early pleistocene age. They were almost entirely free from animal life of any kind. The only specimens found were near the bottom of them, and in one spot, consisting of teeth of a small wolf. This, then, is by far the oldest inhabitant of the cave. The presence of wolf of course implies the presence of other animals.

The Committee is now working with a view to disclosing the old bed of the river which first formed the cave.

Third Report of the Committee for Investigating the Circulation of Underground Waters in the New Red Sandstone and Permian Formations of England, and the Quantity and Character of the Water supplied to various Towns and Districts from those Formations, drawn up by C. E. De Rance (secretary), with supplemental report by T. M. Reade.—No less than 10,000 square miles of England and Wales are occupied by the new red sandstone and permian formations, which absorb not less than ten inches of rainfall annually, and probably more where the overlying drift is pervious or absent, and the sandstone open and permeable.

The Rivers Pollution Commissioners classify waters in the order of their excellence, for general fitness for drinking and cooking, as follows :—

A. Wholesome.	1. Spring water.	Very palatable.
	2. Deep well water.	
	3. Upland surface water.	Moderately palatable.
B. Suspicious.	4. Stored rain water.	
	5. Surface water from cultivated land.	Palatable.
C. Dangerous.	6. River water to which sewerage gets access.	
	7. Shallow well water.	

The average amount of hardness of the water of the deep wells of the new red sandstone tabulated by the Rivers Pollution Commission being 17°·9, and that of the springs no less than 18°·8, the relation of hardness of water to the rate of mortality of the persons drinking it becomes a matter of great importance.

The Commissioners give three tables of statistics that bear directly upon this point :—

From Table I. it appears that in twenty-six towns, inhabited by 1,933,524 persons supplied with water, not exceeding 5° of hardness, the average death-rate was 29°·1 per 1,000 per annum.

From Table II. we learn that in twenty-five towns inhabited by 2,041,383 persons drinking water of more than 5°, but not exceeding 10°, the average death-rate was 28°·3 per 1,000.

Table III. gives sixty towns, with an aggregate population of 2,687,846, drinking water of more than 10° of hardness ; the average death-rate was only 24°·3.

Of the towns in Table I. none are supplied from the new red or permian formations.

In Table II. three are so supplied.

In Table III. ten are so supplied, from which it will be observed that the largest number of towns supplied with new red water are found in the table with the lowest death-rate and the hardest water.

The same result is obtained if we compare towns of corresponding populations and occupations supplied with soft waters from surface areas and those supplied with deep well water in the new red sandstone. Thus :—

	Per 1,000.
Manchester, 351,189 inhabitants, average death-rate	32·0
Birmingham, 343,787 ,, ,, ,,	24·4
And again—	
Stirling, 14,279 ,, ,, ,,	26·1
Tranmere, 16,143 ,, ,, ,,	18·8

The averages are, of course, also dependent on many external causes. Thus, Greenock and Plymouth, both supplied with soft water, with an equal number of inhabitants have a death-rate respectively of 32·6 and 23·3 per 1,000, due to difference of density of population, Greenock only having one house for every twenty-eight people. And again, Liverpool and Birkenhead, both supplied with moderately hard water in the one, an old and densely-populated town with a site saturated with what is injurious to health, the death-rate is 31 per 1,000, while Birkenhead, a new town on an open site with wide streets, has a death-rate of only 24 per thousand, though mainly inhabited by a poor and struggling class of persons.

Still it is worthy of note that the five inland manufacturing towns with the lowest death-rate are all supplied with hard water, and all from the new red sandstone.

	Population.	Mortality per 1,000 per annum.
Birmingham	343,787	24·4
Leicester	95,220	27·0
Nottingham	86,621	24·2
Stoke-on-Trent	130,985	27·9
Wolverhampton	68,291	25·9
Average	144,981	25·5

And again the average death-rate of twelve inland non-manufacturing towns supplied with soft water was 26·0 per 1,000, while that of twenty similar towns supplied with hard water was only 23·2.

When, however, the mortality of the districts, including the principal English watering places, is compared, there appears to be little variation in the death-rate, whether the population be supplied with soft, moderate, or hard water, so that it may be safely concluded that where sanitary conditions prevail with equal uniformity, the rate of mortality is practically uninfluenced by the degree of hardness of the water drunk, and the Rivers Pollution Commission are of opinion that soft and hard waters, if equally free from deleterious organic substances, are equally wholesome.

The Committee are of opinion that it is desirable that they should continue to inquire into areas where new red and permian waters might be obtained by means of deep wells. Looking to the national importance of utilising the underground waters of England, it is desirable that the sphere of this inquiry should be extended so as to include the oolites, which are often not made available for the supply of the population living upon them until the water is hopelessly polluted with sewage. The result of their labours, since the formation of the Committee, has been to prove that there is an available supply of water from the new red sandstone and permian of England of not less than a billion and a-half of gallons of water, the quality of which is remarkably free from organic impurity, and the hardness of which does not in the least appear to affect the health of the population at present taking their supply from it. The death-rate of this area compares well with the best soft-water districts.

Mr. J. Mellard Reade, C.E., F.G.S., added a special report *On the South-West Lancashire Wells*, in which he analysed the information he had obtained for the Committee through the printed forms of inquiry, supplemented by further inquiries which had suggested themselves to him. For the purposes of comparison Mr. Reade selected three nuclei or centres, about which the most important systems of wells are grouped, viz., Liverpool, Birkenhead, and Widnes, and illustrated them by maps and vertical sections showing the relative water-levels reduced to a common datum.

The President thought it important to note the influence of heavy and long-continued rain in relation to absorption by rocks. When rain lasts only a short time, even if it were very heavy, only a little was absorbed ; but if the rainfall were spread over a longer time, a larger proportion would sink into the rocks. M. Lebour described the method adopted by the French engineers for representing the underground water-contours on maps, there being also lines showing the strike of the rocks ; he commended this method to the consideration of the Committee.

SECTION A.—MATHEMATICAL AND PHYSICAL.

On the Relative Apparent Brightness of Objects in Binocular and Monocular Vision, by Silvanus P. Thompson, B.Sc.—It is a common idea that objects appear brighter when seen with the two eyes than with one. There appear, however, to be exceptions to this statement. The following is a method of submitting the question to photometric measurement :—The comparison-photometer employed consists of a cardboard screen, having an aperture divided into two equal portions. One half is covered with tissue paper and illuminated directly from behind. Behind the other half is set, at the polarising angle, a mirror of black glass. Light from a second lamp falls upon a screen of tissue paper, whose light is then reflected in the mirror. Thus the two halves of the aperture may be illuminated equally, but with light in one case wholly unpolarised; in the other, wholly polarised. Let two Nicol prisms be now taken, having their principal sections placed parallel and perpendicular, respectively to the plane of polarisation of the mirror, and let one Nicol be placed in front of each eye. One eye only will receive the whole of the polarised light, while the unpolarised will be equally distributed, half to each eye. The total amount of light received upon the retinal surface will be the same from each half of the aperture; but their apparent illuminations will be unequal, that of the polarised light appearing the greater. By comparing the distances at which the lamps must be placed, it appears that light is more powerful in producing an effect when concentrated upon one eye than when equally distributed to the two, though according to what law experiments are not yet sufficiently numerous or exact to determine; but, on the other hand, the light so concentrated on one eye does not produce the sensation of twice as much illumination as the half of the light viewed by both eyes at once.

A paper by Mr. C. Meldrum was read *On the Diurnal Variations of the Barometer and Wind in Mauritius.* Mr. Meldrum remarked that in 1875, 1876, and 1877, the number of cyclones had been much below the average, and that there had not been any one great storm such as that which occurred in the periods 1860-63, and 1870-73. This, so far, confirms the hypothesis of a connection between the frequency of sunspots and the frequency of cyclones.

With regard to the rainfall the evidence in favour of a cycle corresponding with the sunspot cycle has much increased. Dr. Hunter, of Calcutta, has lately found for Madras a rainfall cycle identical with that which the author had previously found both for India and various other parts of the world. Mr. Meldrum has recently discussed the rainfalls of thirteen stations in the French colonies for various periods from 1832 to 1872, and obtained results nearly the same as those that had been found for 144 stations scattered over both hemispheres. Dr. Fritz, of Zurich, has shown that the severest hailstorms and the highest levels of the rivers occur on the years of maximum sunspot. In short there can, he thinks, be little doubt of an eleven-year rainfall cycle, and when its laws are known they will probably be of much practical use.

Account of a Meteor which passed over Bhawnepoor, in India, in October, 1873, by Major G. Noel Money.—In the beginning of October, 1873, I was staying for a few days at Bhawnepoor, capital of the independent state of the same name, which is situated along the left bank of the River Sutley, and north of the great sandy desert of Bikaneer.

Early one morning I was roused from my sleep by a sound exactly resembling that which would be produced by half-a-dozen express trains passing close to the house at the same moment. The room was as light as the brightest noonday. Before I had time to collect my thoughts, two violent explosions in rapid succession shook the whole house; the doors and windows rattled for fully ten or fifteen seconds. Earthquakes being of not unusual occurrence in the north of India, particularly at that time of the year, I naturally concluded this was something of the kind, and hurried out of the house. As I did so the light faded, and I was surprised to find, as I reached the verandah, that it was still night, although the first streaks of dawn were visible in the east. The native servants were running out of their houses in the greatest alarm; I asked what was the matter. "God knows! the sky has fallen," was the reply.

After breakfast we heard that a shower of stones had fallen eighteen miles off to the north-east of Bhawnepoor, and later in the day some pieces were brought in. The largest was an irregular mass, as far as I can recollect about three feet long, and a foot thick; still hot, blackened outside as if by the action of fire, of which it smelt strongly, of a dark grey colour inside, and very heavy. I have now a piece which I broke off this large mass; although no bigger than a man's fist, it weighs nearly two pounds. The natives who brought these in said there were many more; one, they declared, was as large as a bullock-cart, and so hot that they could not touch it.

It was afterwards ascertained that a second shower of pieces, apparently the result of the second explosion, fell about thirty miles beyond the first. It is satisfactory to know that there was scarcely a possibility of deception as regarded these pieces; there not being such a thing as a stone, rock, or pebble the size of a pea, within the radius of a hundred miles from Bhawnepoor, the soil being either pure alluvial deposit or the finest sand.

The accounts given by native eye-witnesses of this meteor were varied and unreliable, and one could only arrive at a satisfactory result by an exhaustive process of comparison, but I was fortunate enough to meet, the same day, a thoroughly trustworthy eye witness in the person of an European overseer who was superintending the works at a new palace, which was in course of erection for the Nawab at Bhawnepoor. This man had gone down to the works before daybreak to look after a brick-kiln, and being in an open space had an uninterrupted view of the meteor. He described it as a large ball of fire, as big as twenty moons, which passed, with a roaring sound, directly over his head in a north-easterly direction. It lit up the whole sky, the light being perfectly dazzling, and left behind it a flaming track of red, green, and yellow. Before passing out of sight two explosions in quick succession took place, at each of which a shower of sparks seemed to fall, but no alteration appeared in the size and shape of the meteor itself.

It has always been a subject of surprise to me that no attempt was made by Government to collect any information regarding this meteor. Had reports been called for from the various districts it would have been easy to ascertain where it was first and where last seen. Some estimate might then have been made as to its size and distance from the earth's surface.

To give some idea of its magnitude, I may mention that at Dera Ghazi Khan, seventy miles north of Bhawnepoor, it was seen and heard nearly as plainly as it was by us. At a place 200 miles north and a little west of Bhawnepoor, it was so brilliant that a native gentleman was, as he informed me, startled from his sleep by the sudden light, and ran out of his house thinking the next house must be on fire. He did not, however, hear any explosion.

Some soldiers of my regiment in Terar, in Afghanistan, 400 miles north of Bhawnepoor, told me that they had also seen it, and that it was so unusually large and brilliant that the moollahs (Mahomedan priests) were much exercised in mind about it, considering that it must forebode some calamity.

Very little notice was taken of the occurrence in the local papers, but this is to be accounted for by the fact that it passed over the most desert and thinly populated district in the whole of India. I believe, however, I am right in saying that it was also seen in Ajmere and Jypore, over 400 miles to the south-east of Bhawnepoor.

On the Determination of Temperature Coefficients for Insulating Envelopes, by T. T. P. Bruce Warren.—At the Exeter meeting of the British Association I read a paper on electrification, in which I endeavoured to show that the rate of variation in the insulation resistance of a core or cable under changes of temperature could be determined for any period of contact. A statement was made in that paper which has led to the belief that india-rubber has the same constant for correcting from one temperature to another, and for any period of contact.

Prof. Fleeming Jenkins, Mr. Latimer Clarke, and others have pointed out that this phenomenon is not met with in gutta percha, or any other insulator with which they are acquainted. This has led me to re-examine the matter, and to consider carefully the experimental data upon which the paper was founded.

The method of representing graphically the decrease of resistance due to increase of temperature corresponding to one minute's electrification, can be followed out for two, three, or any number of minutes. In this way a series of logarithmic curves are obtained for any required duration of contact; these curves are generated by a constant which must first be ascertained by experiment for changes of temperature at the end of one, two, three, &c., minutes.

This was omitted in the previous paper, or at least not dealt with as the importance of such a subject required.

The phenomenon of electrification, from what has just been pointed out, must appear to every electrician to have received additional importance, so as no longer to be regarded as an unintelligible or inapplicable fact. One very important consequence of its being reducible to an intelligible variation is that we can now calculate not only the changes in the resistance of an insulator due to variation of temperature, but we can ascertain with the same precision any required change due to prolonged contact, at any required temperature. The resistances at different temperatures under different durations of contact will, when tabulated, represent a series of logarithms, the base of each system being the ratio between the resistances for the same differences of temperature, but corresponding to different periods of contact.

From these facts, electrification phenomena are capable of receiving a mathematical rendering, which must prove of great use to telegraph engineers.

If the temperature coefficient were constant for all and every period of contact, we should obviously obtain a series of curves with ordinates increasing in a constant ratio, which would mean that the resistances did not diminish as we reach the higher temperatures. Now as the temperature coefficients for increased duration of contact diminish, the curves more nearly approach each other as the temperatures are increased, which agrees with the fact that electrification ratios are reduced less and less as the resistance itself diminishes. A very curious result arises from this treatment of the subject, which I have not yet had sufficient time to examine, but which may be mentioned here as probably it may assist us to explain something of the nature of electrification. To determine approximately the electrification ratio and consequently the resistance at any required temperature and for any period of contact, calculate first the temperature coefficient for the required temperature, which is simply the expansion of the ratio for 1° F. to that power corresponding to the degrees of difference. Using this as the factor, extract the root of the ratio for any two given periods of contact, this will give very nearly the electrification ratio corresponding to the same period of contact at the required temperature.

It thus appears that electrification, which is an inseparable property of all insulators follows some law of variation in which the temperature coefficient of the insulator itself is a function.

I hope to communicate to a future meeting the mathematical development of the application of logarithmic functions to electrification and thermal charges in insulating media.

Notes on the Volumes of Solutions, by J. A. Ewing and J. G. MacGregor, D.Sc.—In a paper by the authors published in vol. xxvii. of the *Transactions* of the Royal Society of Edinburgh, containing an account of experiments on the density and electrical conductivity of certain saline solutions, notice is directed to the fact that the density of very weak solutions of sulphate of copper and sulphate of zinc is greater than it would be on the hypothesis that the anhydrous part of the salt dissolves without increase of volume in the whole of the water present, including the water of crystallisation. On the other hand the density of comparatively strong solutions is less than this hypothesis would make it. From this it follows that if a small quantity of one of these salts in the anhydrous state were added to water, it would cause contraction, while a larger quantity of the salt would produce expansion. The amount of such contraction, however, as indicated by observations of density, was so small, that the authors were unwilling to speak positively as to its existence until they had applied a direct volumetric test. They have now done so, with the result of confirming the deduction drawn from their earlier experiments.

The apparatus consisted of a large bottle, 2744 c.cm. in capacity, through the cork of which projected a vertical tube of 0·66 cm. in bore. The bottle, as well as a part of the tube, was filled with distilled water, and the salt was introduced in quantities of ten grammes at a time. The resulting change of volume was shown by the rise or fall of liquid in the tube. In order to eliminate the effect of variations of temperature, a second precisely similar bottle and tube were prepared and filled with water, and the two were placed together in a large tube full of water.

The second bottle acted as a thermometer, and the expansion or contraction due to the introduction of the salt into the first bottle was indicated by the difference between the changes of level in the two tubes. After the introduction of each dose of salt the bottle was rolled about for a time, so as to secure thorough diffusion and solution, and then an interval of at least six hours elapsed before readings were taken, in order that the heat given out by the hydration of the salt might be dissipated.

The following results have been obtained in the case of anhydrous sulphate of copper:—The maximum contraction occurs when the proportion of anhydrous salt to water is about one to fifty, and the amount of contraction is then 0·00043 of the original volume of water. As more salt is added the solution begins to expand, and with one part of salt to eighteen of water the volume is equal to that of the water originally present. After this any further addition of salt produces expansion beyond the original volume. The rate of expansion per unit quantity of salt appears to increase continually, but at first it is negative.

The above numbers are given subject to correction by more elaborate experiments that are now going on. The authors hope to extend the inquiry to other salts. They have already examined the behaviour of anhydrous sulphate of soda, but with that salt no contraction whatever has been observed; the solutions expand rapidly from the first.

On Magnetic Induction as affecting Observations of the Intensity of the Horizontal Component of the Earth's Magnetic Force, by Charles Chambers, F.R.S., Superintendent of the Colaba Observatory, Bombay.—The magnets used in observations of deflection and vibration, which observations are necessarily made in the field of the earth's magnetic force, are subject to the inducing action of that force; and it is the universal practice of magnetic observatories, sanctioned by the most eminent writers on terrestrial magnetism, to apply corrections on account of induction both to the deflection and vibration observations. The object of this communication is to advance theoretical reasons, supported by experimental evidence, against the propriety of the particular correction applied to the vibration observation. This correction is based on the assumption that the vibration magnet is susceptible of induction longitudinally but not transversely or not so sensibly; and the assumption probably rests on what the writer regards as a false analogy between a permanent magnet and an induced magnet. The former, when removed from the influence of a strong magnetising action, remains a magnet by virtue of its own internal forces, whilst the latter is a magnet by virtue of external forces alone; it does not therefore follow that because the power of a permanent magnet, measured by its magnetic moment, cannot be made by the same means nearly as great transversely as longitudinally, therefore the same may be said of an induced magnet. Indeed, in his treatment of the subject of the deviations of the compass, Sir George Airy gives to each elemental portion of a ship's iron as great a susceptibility to induction in one direction as in another; and in the more elaborate treatment of the same subject, in which Poisson's equations are taken as expressing the fundamental conceptions of the theory, terms representing transverse induction are still retained as of comparable magnitude in presence of others representing longitudinal induction.

Applying the Astronomer-Royal's theory to the particular case of the vibration magnet, its induced magnetism becomes an assemblage of elementary magnets, whose magnetic axes are all parallel to the magnetic meridian, and which, since they sensibly retain their parallelism to the meridian during the oscillation of the magnet, give rise to no moment of restitution, hence, according to this view, no correction would be required.

According to Poisson's theory, the amount of the correction is matter for experimental inquiry, and cannot be safely determined on *à priori* grounds. It may be objected, however, that the swinging of a ship being a slow motion compared with the oscillation of a magnet, the theory of the deviations of the compass must be modified in its application to the case in question; and this is, no doubt, a correct view, for the theory regards the inductive action as being, at every moment considered, sensibly carried to its limit of effectiveness; whilst it is not only conceivable, but doubtless the fact, that where, as with the oscillating magnet, the motion is reversed every few seconds, the transverse inductive action only partially approaches its limit. On this account we should be prepared to expect then, that even if the transverse induction were as great as the longitudinal when time for full development of the induction was allowed, it would be in defect in the case of the vibrating magnet.

In the years 1873 and 1874—long before these views of the subject of induction first occurred to the writer—he had had made in Bombay a careful comparison of two Kew unifilar magnetometers by means of practically contemporaneous observations. The result was to show a persistent difference in the values of the horizontal force yielded by the two instruments, far exceeding any probable errors of observation, and, after a careful examination of each single observational quantity and of each constant entering into the computations, the writer came to the conclusion that no error of the magnitude of that in question could have its source anywhere but in connection with the induction corrections. The values obtained for the horizontal force were, in British units of force—

With magnetometer No. 17.	With magnetometer No. 23.
8·0701	8·0841
8·0698	8·0823
8·0762	8·0916
8·0764	8·0945
8·0694	8·0965
8·0707	8·0904
8·0757	8·0844
8·0756	8·0821
	8·0902
	8·0858
	8·0905
	8·0880
Mean = 8·0730	Mean = 8·0884

No. 23 showing an excess over No. 17 of ·0154 British units of force, or of ·0019 of the whole horizontal force.

We observe that the greatest value given by No. 17 is less than the least value given by No. 23, and infer that the difference between the two means cannot be attributed to probable error of observation, the value of which for a single determination (about ·001 of the whole force) is, moreover, much smaller. If we now remove the corrections applied for induction to the vibration observations, the mean value yielded by No. 17 becomes ·0004 of the whole force greater than the mean yielded by No. 23. It thus appears that a small correction, such as we have already seen reason to expect, is required for the vibration observation, but—on an average for the two instruments employed—only of about one-sixth of the value of that which it is the custom of magneticians to apply; and as this small quantity scarcely exceeds the probable error of the mean determination of the horizontal force, it is yet premature to attribute it to any definite cause. Whilst, however, the experiments afford no sufficient reason for applying this small correction, they speak very distinctly in favour of no induction correction at all for the vibration observation as against the common practice.

To show that the error that we have been discussing is not of that minute order that is usually disregarded, we may mention that it would amount, in the case of the unifilar magnetometer used at the Observatory, to about eight times the probable error of an observation.

SECTION B.—CHEMICAL SCIENCE.

On a New Mechanical Furnace used in the Alkali Manufacture, and for Calcining Purposes generally, by James Mactear, F.C.S.—The author exhibited and explained the construction of a working model of the furnace which he has introduced for the calcination or so called carbonating of soda, ash, or alkali, and which is also applicable to many other operations, notably that of calcining copper ores, especially as required in that branch of copper manufacture called the "wet process."

These furnaces are now being widely adopted by alkali manufacturers with great success, the saving in labour having been over 60 per cent., and of coal over 20 per cent., while the quality of the work done is much superior to hand work.

On an Improved System of Alkali Manufacture, by Mr. James Mactear.—The author described his improved system of manufacturing blend ash or crude alkali, and claims that it has the following advantages:—

1. By its use the output of the furnaces has been increased from 50 per cent. to 70 per cent.
2. There is a large saving during the lixiviation and in coal.
3. There is a much reduced quantity of waste.
4. There is a considerably increased yield of alkali from a given amount of sulphate of soda.
5. There is a considerable saving in wages.

The process is now widely adopted in Great Britain, and is also most successfully used in France.

On the Regeneration of the Sulphur employed in the Alkali Manufacture, by the "Mactear Process," as conducted at the works of Messrs. Charles Tennant and Co., St. Rollox, by James Mactear, F.C.S.—The author described his process as conducted at Messrs. Tennant's works, at St. Rollox, and showed how by its adoption the nuisance arising from alkali waste deposits and the drainage therefrom had been removed. He also gave details of the cost of manufacturing sulphur by this process, and a description of the plant employed.

The "Mactear Process" is specially applicable to those cases where the drainage of the waste deposits is allowed to flow into streams or rivers, as by collecting the drainage liquor and treating it in the manner described by the author, not only is a great source of nuisance removed, but a new outlet is obtained for hydrochloric acid, while the sulphur is produced at a cost which leaves an amply remunerative margin to the manufacturer.

Messrs. Tennant and Co. now recover weekly about thirty-five tons of refined sulphur by this process.

The Action of Various Fatty Oils upon Copper, by W. H. Watson.—This communication enumerates a number of experiments showing the extent to which different oils act upon copper, the conclusions arrived at being that paraffin and castor oils have the least action upon copper, whilst the action of sperm and seal oil is slight. The rest of the oils examined—linseed, olive, almond, colza, sesame, and neatsfoot, all act considerably upon copper, the action of linseed oil being especially great. The author concludes from experiments that the comparative action of different oils cannot in all cases be decided upon from the appearance of the oils after exposure to copper plates, though minute quantities of the metal may be easily detected in most oils from the colour produced.

On Changes in Candles produced by long Exposure to Sea-water, by Prof. Gladstone, F.R.S.—Mr. Latimer Clarke had sent the author some specimens of candles recovered from the wreck of a vessel sunk off the Spanish coast in 1702, which have remained submerged for a period of 173 years. The wick has rotted away, leaving scarcely any trace of its existence, while the fatty portion has become a friable heavy substance of a dull white colour. The candles bore evidence of having been formed by dipping. The fat may be easily separated from the rest by ether.

After exhaustion with ether there remained a strongly alkaline white ash, consisting of carbonate and chloride of calcium and sodium, with traces of potassium and magnesium. From analysis it appears that the fat has been converted in great measure into calcium and sodium salts, doubtless by the slow replacement of the triatomic group C_3H_5 in the stearine, by three atoms of the metal, with the simultaneous production of glycerine. Though the calcium in sea-water is far less abundant than the sodium, it appears to have had a much greater effect, and it is impossible to say whether the one salt may not have been made by double decomposition from the other. The author pointed out as an interesting point that whereas the fats have been in contact with a practically unlimited quantity of sea-water for 173 years, and a chemical change between them has been possible, the double decomposition has proceeded so slowly that the reaction is only about half completed at the present time.

Contribution to Chemical Dynamics, by C. R. Alder Wright and A. P. Luff.—Guided by certain theoretical speculations, the authors are endeavouring to trace out the connections between the chemical habitudes of certain substances and the temperatures at which their mutual inter-reactions are first noticeable, and notably the relations between the heat developed during their actions and these temperatures of initial action. Experimenting on oxides of copper and iron in different condition as to molecular structure, they find, that whilst the initial temperature of action varies within certain limits with the molecular state of the metallic oxide, when they are subjected to the ordinary action of carbon oxide, hydrogen, and carbon, a given form of either oxide is invariably acted on by carbon oxide at a lower temperature than by hydrogen, and by hydrogen than by carbon; whilst the heat evolution during the reduction of the metallic oxide by carbon oxide is always greater than that during reduction by hydrogen, which again is less than that during reduction by carbon. Similarly, so far as they can be compared, that metallic oxide is acted on at a lower temperature, in the production of which there is less heat evolution (*i.e.*, in the reduction of which there is most heat evolution). How far these generalisations are applicable to other metals is under examination, as are several other collateral points.

On the Coal brought Home by the late Arctic Expedition, by T. Wills.—The coal occurs on the side of a narrow mountain gorge about two miles from Discovery Bay, the winter quarters of H.M.S. *Discovery*. It appeared in the form of a slight saddleback thickest in the centre, becoming continually smaller at each end; the thickness of the seam at the deepest visible portion was 25 feet, and its visible length 250 yards; neither the bottom of the seam nor the underlying strata were visible at any place. Overlying the coal was very friable carbonaceous shale containing impressions of miocene plants, and above this shale a hard fossil red clay stone similar to the red miocene rock of the Disco coal beds, but sterile. The seam is almost uniform in character, and is very free from clayey veins. On obtaining the sample of coal Mr. Wills expected to find it to be a lignite, as cretaceous or tertiary brown coal does occur in these high latitudes, and more especially as the impressions of miocene plants in the overlying strata seems to indicate a more recent period than the true carboniferous; but it turns out that this is not the case, for the coal in appearance and on analysis cannot be distinguished from a bituminous coal of exceedingly good quality belonging to the true carboniferous period. Mr. Wills, from recent information, understands that miocene plants have been found in the strata underlying the coal, in which case there can be little doubt that the coal is a miocene coal, although differing greatly from most specimens of such coals. The following is the result of several analyses:—

Specific gravity	1·29
Moisture	2·38
Ash	6·21
Sulphur	·96
Carbon	76·95
Hydrogen	5·43
Oxygen Nitrogen } by difference	6·78
	100·00

On comparing these figures with the result of the analyses of a mixture of thirteen different seams from English coal-fields, Mr. Wills has found that the Arctic coal possesses very nearly the same composition.

On Hederic Acid and Resin of Scammony, by C. T. Kingzett.—In a paper *On some New Reactions in Organic Chemistry*, and their ultimate bearings the author in conjunction with Dr. Hake has described a number of instances in which bodies, for instance camphor, gives with strong sulphuric acid and sugar a violet-coloured product. Other bodies give this colour with sulphuric acid without the addition of sugar, and by means of these reactions the constitution of many substances may be in a measure predicted. Hederic acid, $C_{16}H_{26}O_4$ (Posset, also Davies), a constituent of ivy leaves, gives this colour best with sulphuric acid, and so also in a less degree does resin of scammony. In the present paper the author describes the process by which he has isolated glucose from these respective substances, thus confirming the hypothesis given in his original paper alluded to. Incidentally it is shown that the root of the convolvulus scammonia contains no alkaloid, and some information is given regarding a volatile oil obtained below 90° on distillation of scammony resin.

Albumen of Commerce, by C. T. Kingzett and M. Zingler.—In the patent process described by the authors, albumen solutions are bleached and preserved by passing a current of air through them in presence of oil of turpentine at a temperature of about 40° C. Under these conditions the turpentine oxidises, producing hydrogen-peroxide, camphoric acid, &c., the former of which bodies effects as it forms the bleaching of blood serum or other albuminous solutions, while the camphoric acid, &c., preserves them in the liquid condition entirely free from putrescible or other changes.

Alkaloids from Japanese Aconite, by Dr. Paul and C. T. Kingzett.—The authors have isolated from Japanese aconite an alkaloid of the formula $C_{29}H_{43}NO_9$ which is crystalline, but does not form crystallisable salts. They also show that when the alkaloidal principle is extracted by Duquesnel's process it is accompanied by the salt of an alkaloid, perhaps aconitate of aconitine; and from this it is suggested that the so-called aconitine obtained and analysed by Wright and others, has never been obtained absolutely pure, being probably a variable mixture of the alkaloid with the above salt.

Further Researches on Aconite Alkaloids, by Dr. C. R. Alder Wright and A. P. Luff.—Aconitine, $C_{33}H_{43}NO_{12}$, the active principle of *Aconitum napillus* is readily saponified by heating with water acids and alkalies into *benzoic acid*, and a new base termed by the authors *aconine* pseudaconitine, $C_{36}H_{49}NO_{11}$, the chief active alkaloid of *A. ferox*, similarly gives rise to *dimethyl-proto-catechuic* acid and *pseudaconine*, $C_{27}H_{41}NO_8$. These two decomposition products, aconine and pseudaconine, are comparatively inert physiologically. A number of their compounds and derivatives have been studied and a method for the approximate analysis of the impure alkaloids met with in commerce under the name "aconitine," has been devised, based on the quantitative estimation of the benzoic and dimethyl protocatechuic acids formed on saponification.

On Pyrocatechin as a Derivative of Certain Varieties of Tannic Acid, by John Watts, D.Sc.—From the known reactions of gallotannic acid and catechutannic acid, and of their derivatives, the author considered it probable that all the blue producing tannins would yield pyrogallol on distillation, while the green producing tannins, would yield pyrocatechin: on experiment such was found to be the case. The gallotannic acids distilled were,—valonea, oak-bark, divi, mysotolaves, sumach, and mimosa bark; and the mimotannic acids, rhatany, tormentil, and hemlock bark. The yield of pyrocatechin from rhatany was very considerable.

These results point to the conclusion that the blue- and green-producing tannins are related to each other in the same manner as pyrogallol and pyrocatechin. The author anticipates being able to bring forward shortly further experiments in support of this view.

On the Formation of the Black Oxide of Iron on Iron Surfaces for the Prevention of Corrosion, by Prof. Barff, M.A. (Cantab.)—The author pointed out the cause of his many failures in his first experiments and the failures which others had experienced in obtaining a perfectly adherent and coherent coating of black oxide, as arising from moisture in the steam with which the articles operated on were oxidised. When perfectly dry steam is used and no air admitted into the muffle, or oxidising chamber, then in all cases a perfect protecting film is formed. The process is exceedingly simple: a wrought-iron muffle containing the iron articles to be operated upon, is heated to a dull red heat, all the openings closed, and dry steam turned in, and the muffle kept filled with the steam during the whole operation, which lasts from three to five hours; the fire is then raked out, and the articles allowed to become black in an atmosphere of steam; after this the steam is turned off, and the muffle and its contents are allowed to cool slowly. The temperature to which the muffle is heated varies according to the nature of the articles operated on—from 350° to 700° C. More recent experiments seem to show that the process may be further simplified by using superheated steam of such a temperature that the external application of heat to the muffle is unnecessary. A considerable number of cast-iron, wrought-iron, and steel bodies which had been coated were exhibited. Many of these had been out of doors for months; others had been kept in fresh water or in sea-water for a similar length of time, but not the slightest indication of further oxidation was visible. Even strong nitric and sulphuric acids are without action on this coating of black oxide.

SECTION C.—GEOLOGY.

The Post-tertiary Fossils procured in the late Arctic Expedition; with Notes on some of the Recent or Living Mollusca from the same Expedition, by J. Gwyn Jeffreys, LL.D., F.R.S.—The fossils were collected by Capt. Feilden and Mr. Hart, the Naturalists of the Expedition, and by Lieut. Egerton and Dr. Moss, two of the officers of H.M.S. *Alert*, in very high latitudes, viz., between 82° and 83° N.L. The furthest point reached by the Expedition was 83° 20′ 26″. These fossils were found in mud-banks or raised sea-beds at heights ranging from the level of the sea to 600 feet above it. They consisted of eighteen species of mollusca, one of actinozoon, one of foraminifera, and one of marine plants, being altogether twenty-one species, all of which now live in the Arctic seas. The author gave a list of the species, and showed their distribution in a recent or living as well as fossil state; and he added some remarks as to the recent mollusca procured in the Expedition, and as to the apparent abundance of marine animals in the "Palæocrystic Sea" of Sir George Nares. Prof. Rupert Jones, Dr. Moss, Mr. Woodall, and Mr. De Rance took part in the discussion which ensued on the reading of this paper.

Sketch of the Geology of the Coast from the Rame Head to the Bolt Tail, by W. Pengelly, F.R.S.—Mr. Pengelly expressed his partial acceptance of Mr. Jukes' views. He believed the upper old red sandstone to be the equivalent of the lower Devonian, each containing *Phyllolepis concentricus*, which is not found at any other horizon. The author also called attention to the metamorphism which has taken place in the rocks at and near Prawle Point, for which no sufficient cause is now apparent. He supported the suggestion of Dr. Holl and Mr. Jukes that south of Prawle Point there may be a boss of granite now submerged, to which the change in character of the rocks is due. As evidence of this he spoke of a beach, in which many granitoid pebbles occur, but with this exception the pebbles are strictly local. He thought that these pebbles were probably derived from the now submerged granite. The age of the metamorphism is clearly pre-triassic, for the triassic strata of the district contain pebbles of metamorphosed rock.

On the Drift of Plymouth Hoe, by J. H. Collins.—The author stated that excavations were nearly always going on in the neighbourhood of Plymouth Hoe, and that fresh sections of the so-called raised beaches and glacial deposits were continually being exposed.

He had lately visited the Hoe, Mt. Batten, and Deadman's Bay, in company with Mr. Whitley of Truro, and had found gravels, sands, and clays lying in the hollows of the limestone, and filling fissures and caverns. The gravels were sometimes cemented by stalagmite into a conglomerate. The pebbles were composed of quartz, limestone, tourmaline schist, greenstone, blue and red grit, hard clay-slate, schorl rock, granite, elvan, flint, chert, stalagmite, and one pebble of granite; all of which the author considered had been derived from the rocks of the neighbourhood within a few miles. None of the pebbles were in the least degree ice-scratched, and there were very few angular fragments of any kind.

The gravels had yielded bones of rhinoceros, elephant, and other animals of the so-called "Mammoth period." The author discussed the evidence of local denudation, and adopted or arrived at the following conclusions:—

1. The deposits are not raised beaches.
2. They are not glacial.
3. They were formed rapidly.
4. Gravels, fissure deposits, and cave deposits are of the same age.
5. That they belong to the Mammoth period.
6. There is no evidence in the immediate neighbourhood to carry back their formation more than a few thousand years.

Notes on the Devonian Rocks near Newton Abbot and Torquay, with Remarks on the Subject of their Classification, by H. B. Woodward, F.G.S.—After having alluded to the imperfect state of the information respecting the Devonian rocks, especially in regard to local details of structure, the writer pointed out that the succession of strata near Newton Abbott and Torquay was (in descending order) as follows:—3. Limestone; 2. Slates; 1. Red Sandstones. He noted the resemblances in lithological characters between these beds and the lower carboniferous rocks and old red sandstone, with which they were classed fifty years ago by De la Beche. He likewise drew attention to their relations with the Culm measures, observing that while there were indications of conformability to them, no positive proof to the contrary had been established; and the supposed instances of unconformability were all of them, as Jukes had considered, capable of explanation by faults and other disturbances. Attention was drawn to some striking cases of such phenomena. The impossibility of accepting fossil evidence alone was insisted upon, inasmuch as its value in classification could only be gained after the stratigraphical relations of the beds had been made out, and at present the exact horizons from which many of the species had been collected was not determined.

Further, the theory that the Devonian rocks were the equivalents in time to the old red sandstone required the existence at this period of a great barrier between the marine deposits of the former group and the freshwater accumulations of the latter, and there was no physical evidence in support of this. Taking all the facts into consideration, Mr. Woodward argued that they were in favour of the classification proposed by Jukes, which regarded the lowermost Devonian rocks as old red sandstone, and the slates and limestones as lower carboniferous, formed in an area which constituted a zoological province differing to some extent from that in which these rocks were deposited further north in the British area.

On the Devonian System in England and Belgium, by Prof. G. Dewalque.—Having surveyed, last year, the Devonian system of this country, I avail myself of the meeting of the British Association to offer a few remarks on the results of my survey. As my visit was short I cannot lay claim to a minute acquaintance with this great formation in England; but, as well acquainted with it in Belgium and the Rhenish provinces, I hope the following remarks may prove of some interest to the Association:—

I had not time to visit South Devon. As regards North Devon my conclusions are as follows:—1. The metamorphic character is more prevalent there than in Belgium, especially in the middle and the upper divisions. 2. All this series is perfectly continuous, from Barnstaple to Lynton. Nowhere is there a reappearance of such identical rocks as to prove a fault, by repetition of the series. 3. The sandstones of Baggy Point and Marwood (*Cucullæa zone*) perfectly agree, both lithologically and palæontologically, with certain portions of our "Psammites du Condros." The red sandstones of Pickwell Down correspond to the lower part of these Psammites. 4. The limestone of Ilfracombe represents, as has been previously stated, on palæontological evidence, the "stringocephalus limestone" (*Calcaire de Givet*) of Belgium and Germany; but the lithological appearance of the rock is very different. Hence it is easy to compare this Devonian series with that of the Continent. In this respect I differ but little from Mr. Etheridge. 5. The Devonian limestone is much more abundant on the Continent than on this side of the Channel. I think, moreover, that the same is to be said of the carboniferous formation, that is to say, the mountain limestone is replaced in North Devon (at least in part) by the beds of Barnstaple and Pilton. In the slates of Pilton I found beds and nodules of siliceous concretions, which represent, I think, the *chert* of the carboniferous limestone, or the so-called *phthanites* of our "calcaire carbonifère."

As to the old red sandstone I spent a week in Hereford, but saw very little of it. I could only hammer conveniently the "cornstones," of which I had from the descriptions a very imperfect notion. Such limestones occur identically in Belgium, with red shales, sandstones, and conglomerates in the northern trough, or "bassin de Namur." This fact seems to me of the highest value, for it leads me to this paradoxical conclusion: the old red sandstone of the United Kingdom is a marine formation, probably formed in the same ocean as the Devonian. The old red of Belgium lies regularly between limestones with *Stringocephalus Burtini* and others with *Spirifer disjunctus*. That is certainly a marine formation, and the same must be the case with the English old red sandstone.

On the Succession of the Palæozoic Deposits of South Devon, by A. Champernowne, M.A., F.G.S.—The Great Devon limestones, the author concludes, are, as Mr. H. B. Woodward has said, the highest rocks of South Devon, and the belief in a series of slates and red sandstones overlying them, is a fallacy. The beds which do succeed the limestones are the Culm measures (upper carboniferous), and from the field-work of Messrs. Woodward and Read there is reason to believe them perfectly conformable. In this case the difference between the Devonian and carboniferous limestones would be one of life distribution—a geographical, and not a chronological, difference. This would probably have been long ago recognised had the characteristic ichthyolytes of the old red occurred in the Staddon beds.

Note on the Carboniferous Coast-line of North Cornwall, by S. R. Pattison, F.G.S.—The portion of coast described extends from near Bude to Boscastle, and belongs to the formation first identified by Prof. Sedgwick in connection with the diagnosis made at Bideford by him and Sir R. Murchison as culm, or lower coal measures. Bude lies in or on the centre of the formation. The strata have a general northerly dip, and proceeding southwards down the coast of course lower beds become exposed. The Bude beds contain thin films of culm, with associated plant-remains in a very fragmentary condition. Prof. Morris many years ago in a note published in the *Proceedings* of the Geological Society of Cornwall, identified some of those remains as *Calamites*, *Sigillaria*, and *Asterophyllites*. Prof. Hull states the number of species in the North Devon beds, of which these are the continuation, at twenty-three, and Mr. Townshend Hall at twenty-six. The Bude beds are continued by foldings and succession downwards, but on arriving at St. Gennys a system of deep-blue schistose sandstones appear and form the base line of the cliff along the remarkable coast landslip which extends for two miles. From these dark-blue beds fragments or nodules containing *goniatites* appear on the beach. Then comatile-beds extend from Carne Beak to the cliffs in the parish of St. Juliott. They are most abundant at the St. Gennys end of the landslip. Here, at a sand-path descending to the beach, on the beach, are huge fragments of fallen rock containing very fine large impressions of plants, especially sigillaria. Proceeding towards Boscastle, at the gloomy gorge of Pentagion, the soft black shales, so characteristic of Boscastle, form the bulk of the cliffs, but below them rises a slaty rock once quarried, and in this I found the usual fragmentary plants of the Bude rocks. This, with the associated soft black beds, is the farewell rock of the carboniferous, for at the cliff, on the south side of Boscastle, slates arise under the black shales, which at the summit contain traces of crinoids, and are the commencement of the Devonian slates, continued hence to Tentagel, and well known as Devonian.

These few facts seem to verify the general conclusion arrived at by former observers, and, when more fully investigated and the fossils identified, will help to correlate the carboniferous of North Cornwall with the divisions now established elsewhere. They seem at least to show that there are provinces in our local geology still holding out temptations of further conquest to the geological explorer.

Notes on the Palæontology of Plymouth, by R. N. Worth, F.G.S.—This paper did not enter into any controverted questions of stratigraphical geology, but simply noticed the main features of the palæontology of the limestone of Plymouth and its associated rocks. The Plymouth rocks were commonly classed as Middle Devonian, and consisted of slates, limestone, and slates and sandstones, in order from north to south. The northern slate rocks did not locally contain fossils; but in the vicinity of Saltash, &c., they did. The Plymouth limestone formed a band nearly half a mile in width and nearly six and a half miles in length. It originated clearly enough in a fringing coral reef, and in its origin and constitution was therefore essentially organic. The rocks on the south of the limestone were of a more complicated character than those on the north. Slates, limestones, shales, grits, ash-beds and sandstones, alternated with each other in remarkable fashion, while faults and contortions by no means solved the riddle. These rocks in part were largely fossiliferous. In the variety of its organic remains the Plymouth limestone was not so rich as most of the other chief limestone districts of South Devon—Wolborough and Barton, for example; but those that did occur were for the most part abundant. The leading peculiarity was that while at the western end of the limestone—that was to say, at the Dockyard, Mount Wise, and at Stonehouse, in the quarry behind St. George's Hall, molluscs of various kinds occurred, at times in great profusion; at the eastern end of the limestone—Cattedown, Oreston, &c., they were comparatively rare, and over considerable areas altogether absent. And in like manner, the branching corals were found chiefly at the western end of the limestone; and the genuine reef builders at the eastern. There did not appear to be any difficulty, however, in accounting for this. Molluscs could only find a habitat on the exterior portions of the reef, and it was evident that the eastern section of the limestone more particularly had been subjected to a considerable amount of denudation, and that the outer beds had to a large extent been removed. Bivalves and univalves were rarely associated, but kept to distinct areas, where they sometimes occurred in great abundance. The peculiar interest of the palæontology of Plymouth consisted in the products of the ossiferous caverns and fissures at Oreston, the Hoe, and Yealmpton, including the bones of the mammoth, hippopotamus (?), *Rhinoceros tichorhinus* and *leptorhinus*, cave lion, cave hyæna, cave bear, ancient bear, the lesser bison, long fronted ox, horse, ass, &c., and a vertebra of the whale.

On the Geological Significance of the Result of the Boring at Messrs. Meux's Brewery, Tottenham Court Road, by R. A. C. Godwin-Austen, F.R.S.—It is now very generally known that this undertaking, after passing through a great thickness of chalk, met with a very insignificant representative of the sands which underlie the chalk in the south-east of England, and thence passed at once into strata which, by characteristic fossils, were identified as of upper Devonian age. This is just as had been anticipated as to the absence of any portion of the oolitic series there,[1] and confirmed what many years since had been supposed to be the subterranean structure of the south of England; indeed, it may be fairly stated that geologists generally have been of opinion that a band of palæozoic rocks, extending from Westphalia westwards, passed somewhere beneath the secondary formations of the south-east of England.

The importance of determining the course of such palæozoic band was, that along the whole of the exposed part of its course, as from its extreme eastern place to near Valenciennes, it had dependent on it, on the north, the productive coal-measures of Westphalia, Belgium, and the north of France. From Valenciennes westwards the coal-measures are not exposed at the surface, but are reached beneath the chalk formation; but from the underground workings at Douay, Béthune, &c., the relation of the several members of the palæozoic series are known to correspond exactly with those where the series is exposed; as is the case also where they are again seen at the surface in the Boulonnais, and at sundry other valleys of elevation along the axis of Artois.

The whole of the coal-measures of Belgium and the north of France must be understood as occupying a trough formed out of the older members of the great palæozoic series, and the explanation given of the preservation of this extended and narrow band of coal-growth surface is that it has resulted from a contraction of the earth's crust in a south to north direction, at some time subsequent to the completion of the palæozoic series (coal-measures included), whereby along this line a series of east and west undulations were produced, in the deepest or most considerable of which, portions of the coal-growth surfaces became included so as to be preserved during the subsequent periods of denudation and removal.

From the consideration of the physical features of a line of country of elevation and disturbance, which crosses the European continental area for 300 leagues, it was inferred that like results were due to like causes here; the line of under-run of the palæozoic strata was conjecturally carried along by where it has just been met with; so it may reasonably be supposed that certain other phenomena which in like manner have resulted from the same disturbances should also correspond, and serve for guidance.

For the present it has not been ascertained in what *direction* the highly-inclined Devonian strata at Tottenham Court Road were dipping, a most important point in the considerations involved. It may safely be supposed that from their position any palæozoic rocks at such place must be trending east and west. The occurrence may seem to be an isolated fact, but there are other inferences which tend to give it importance.

The 653 feet of chalk strata were horizontal, or with only a very slight north dip. The Devonian strata gone through dipped uniformly at an angle of 30°. The section therefore corresponds exactly with those of the north of France.

In Belgium, and the north of France, it is on the *south side* of the palæozoic trough that the high inclines occur, as happens along the whole line from Liége to Trelon. On the north the beds are flatter and spread out wider. From this it may be supposed that it was the north side of the trough which was hit upon at Messrs. Meux's, and that it is a trough at this place follows necessarily from the circumstance that the beds so highly inclined were as low as the Devonian.

Bearing in mind that the whole of this part of Europe we are now considering formed part of the area over which the Devonian or lower carboniferous series preceded or was overlaid by the upper or true carboniferous formations, and that where one occurs the other follows everywhere, the fact of the inclination of the beds at Tottenham Court Road involves this, that the higher portions must soon follow—the mountain limestone on the Devonian, and the coal-measures on the mountain limestone.

This reasoning applies equally whether the Devonian strata at Tottenham Court Road may be dipping north or south, but thus much has been ascertained, that London just overlies the edge of a great coal-field, and the probability is that the coal-field lies to the north.

What seems to suggest that the coal in this direction may have considerable extension is derived partly from a study of the geological features of our own island, and partly from what is the case in Belgium. It is dependent on what was the original form and extent of the coal-growth surface, and on the places at which the greatest amount of contraction and subsequent denudation of the surface took place.

Mr. Whitaker described the deep borings around London, and gave an account of the strata traversed by them. He suggested that some of them should be continued deeper, and thought that in place of a "Sussex boring" or a "Kentish boring" they ought to have a general scheme for investigating the range of the palæozoic rocks. Mr. Lebour suggested that under London (as often occurs in Belgium) the rocks might possibly be inverted or reversed by oblique faults, so that Devonian rocks under certain circumstances might overlie the coal-measures. Mr. Topley, in reply, defended the past action of the Sub-Wealden Committee.

On a New Method for Studying the Optical Characters of Minerals, by H. C. Sorby, F.R.S.—The author first described the principles on which this method depended, and showed that the great difference between the appearance seen with the naked eye and the microscope is due to the object-glass being able to collect divergent rays. In looking with a low magnifying power at a small circular hole seen through a section of a crystal, very different phenomena present themselves, according to its optical characters. If it has no double refraction, only one well-defined circular hole can be seen. If the mineral possess double refraction and only one optic axis, like calcite, two images of the hole are seen. If the section be cut perpendicular to the axis, two circular holes are seen directly superimposed, but at two different foci. If the section be in the plane of cleavage, two widely-divided images are visible, the one due to the ordinary ray being circular, and the other, due to the extraordinary ray being distorted and drawn out in two opposite planes at two different foci. When the section is cut parallel to the axis, this image due to the extraordinary ray is still more elongated, but the images are directly superimposed. We thus at once learn that the mineral has double refraction, has an optic axis, and also what is the direction in which the section is cut. In the case of crystals like Arragonite, which have two optic axes, there is no ordinary ray, and at the focal points we see the circular hole drawn out in opposite planes into crosses. The character of these crosses depends upon the direction of the section, but the fact of the crosses being seen at once proves that the mineral has two optic axes. Some facts are better observed if, instead of a circular hole, we examine through the crystalline plate a grating with two systems of lines at right angles to one another. We then obtain what the author calls unifocal or bifocal images, according to the systems of crystallisation. Crystals without double refraction have only one unifocal image; crystals having one optic axis have one unifocal and one bifocal image; whereas crystals having two optic axes give two bifocal images. The definition of unifocal images is independent of the position of the lines, whereas in the case of bifocal images the lines are distinctly visible only when they are parallel or perpendicular to a particular axis of the crystal, and, spread out, become obscure and disappear when rotated to a different azimuth.

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[1] See "Report of Coal Commission," vol. i., pp. 431-432.

The above-named general characters differ so much in different minerals, that they furnish a most valuable means for their identification.

On the "Great Flat Lode" South of Redruth and Camborne, by C. Le Neve Foster, B.A., D.Sc.—In this paper the author described an important tin lode which is wrought in various places for a distance of three and a half miles. In some places it occurs, for instance at Wheal Uny, at the junction of the clayslate (killas) and granite, but in other mines it lies entirely in granite.

Its characteristics are :—

1. A *leader* of true fissure vein, generally only a few inches wide, and filled with clay, fragments of the inclosing rocks, and tin or copper ores, dipping 30° to 50° S., and striking from 20° to 45° N. or E. (true).

2. The *lode*, from four to fifteen feet wide, on one or both sides of the leader, consisting mainly of schorl-rock, containing grains and veins of tin ore. It yields from 1 to 3 per cent. tin ore.

3. A *capel*, or non-stanniferous or slightly stanniferous schorl-rock, separating the lode from the killas or granite.

4. Absence of any *wall* or plane of separation between the lode and capel, or between the capel and granite. The author said that all the appearances pointed to the fact that the lode and capel are merely altered granite. In confirmation of this vew he explained that he had found cavities in the lode resembling felspar crystals in shape, and probably left by its removal; furthermore the microscopic examination of the capel shows apparently pseudomorphs of quartz after felspar.

If it is admitted that the mass of the "Great Flat Lode" and its capels are altered rocks once containing felspar, we are driven to conclude that that rock must once have been granite, because of the gradual passage of the capel into granite. Supposing this view to be correct, we must adopt a similar explanation in the case of many of the important tin lodes in Cornwall.

The author ventured the opinion that half the tin ore obtained in Cornwall is now derived from altered granite.

On some Tin Mines in the Parish of Wendron, Cornwall, by C. Le Neve Foster, B.A., D.Sc.—The author described the tin deposits of the following mines:—Balmynheer, the Lovell, and South Wendron. The author supposes that the tinny rock is an altered granite, and he brings forward in support of his argument the fact that pseudomorphs of quartz and of gilbertite after orthoclase, are found in the stuff from the Lovell, and that there is a gradual passage from the tin rock into granite.

On some of the Stockworks of Cornwall, by C. Le Neve Foster, B.A., D.Sc.—The author divided the tin stockworks into three classes according as they occur in killas granite or elvan, and then described the mode of occurrence of tin ore at some of the most important.

The Carboniferous Limestone and Millstone Grit in the Country around Llangollen, by G. H. Morton.—The author described the carboniferous limestone exposed in the Eglwyseg ridge near Llangollen, North Wales. He stated that the finest section is exposed at the Ty-nant ravine on the bed of Cefn-y Fedw, and that the country around must be considered as the typical area of the lower carboniferous series of North Wales. The millstone grit or Cefn-y-Fedw sandstone, which reposes on the limestone in the same district, was also described. The following tabulation explains the succession and thickness of the entire series :—

Tabular View of the Carboniferous Limestone and Cefn-y-Fedw Sandstone in the Country around Llangollen.

		feet.	
Cefn-y-Fedw sandstone.	Aqueduct grit or upper sandstone and conglomerate	70	Upper Cefn-y-Fedw, Dee Bridge, or millstone grit series.
	Upper shale	30	
	Dee Bridge sandstone ...	30	
	Lower shale with fire-clay and bands of limestone.	18	Middle and lower Cefn-y-Fedw or Yoredale series.
	Middle sandstone	200	
	Cherty shale	50	
	Lower sandstone and conglomerate	250	
	Sandy limestone	75	
Carboniferous limestone.	Upper grey limestone ...	300	Upper carboniferous limestone.
	,, white ,, ...	300	
	Lower ,, ,, ...	120	Lower do.
	,, brown ,, ...	480	
		1923	
	Upper old red sandstone	300	

The following table shows the gradual attenuation of the carboniferous limestone towards the south-east.

Attenuation of the Carboniferous Limestone.

Subdivision.	Ty-nant.	Tan-y-Castell.	Trevor Rocks.	Bron-henlog.	Fron.
Upper grey limestone	300	300	250	66[1]	88[1]
,, white ,,	300	250	140	99	27[2]
Lower ,, ,,	120	115	117	104	,,
,, brown ,,	480	360	100[3]	26[3]	,,
	1200	1025	607	295	115

This section shows how the limestone diminishes in thickness with the rise of the Wenlock shale towards the south-east. Between the Ty-nant ravine and Tan-y-Castell it has thinned out 200 feet, and at Fron-y-Cysyllte, four miles from the former place, the attenuation is not less than 900 feet.

The list of fossils collected by the author contained seventy-seven species. Of these fifty-eight occur in the upper grey limestone and only eighteen in the lower brown limestone. If the carboniferous limestone is simply divided into upper and lower limestone, thirty-eight species are peculiar to the two upper subdivisions and nineteen to the two lower subdivisions—twenty species being common to both. However, the species are by no means confined to the subdivisions in which they are found near Llangollen, for they occur at different horizons in other districts.

On the Occurrence of Branchipus *or* Chirocephalus *in a Fossil State in the Upper Part of the Fluvio-Marine Series (Middle Eocene), at Gurnet and Thorness Bays, near Cowes, Isle of Wight*, by Henry Woodward, F.R.S.—Mr. Woodward referred to the great interest surrounding the geology of the Isle of Wight from the labours of Ibbetson, Forbes, Mantell, Prestwich, Bristow, and many others, and the rich fauna contained in its strata, much of which still remains to be described, although the stratigraphical geology has been well worked out by the officers of the Geological Survey. Mr. Woodward called attention to a thin band of freshwater limestone occurring at the base of the cliff, belonging to the Bembridge series, from two to twelve inches thick, which at places is full of remains of plants and insects. *Dytiscus*, *Curculio*, *Formica*, &c., and what is most remarkable, the diaphanous bodies of a small phyllopod crustacean, without a hard shelly covering. This little crustacean is closely related to the "Brine-shrimp" (*Artemia salina*), so abundant in the brine-pans at Lymington at the present day. *Branchipus*, or *Chirocephalus*, is a freshwater crustacean found living in ponds in Devonshire and Kent. Its preservation is due to the admirable nature of the fine argillaceo-calcareous rock, in which it has been entombed in such numbers, the delicate outline of its gill-feet being stained with iron, so as to be as well shown as in a photograph.

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[1] Upper portions been denuded.
[2] Reposes on the Wenlock shale.
[3] Base not ascertained with certainty.

SECTION G.—Mechanical Science.

On Compound Turbines, by Prof. Reynolds.—The combination of centrifugal pumps not having hitherto produced the anticipated results, the author had endeavoured to discover the cause of the apparent anomaly, being satisfied that theoretically the increase in the number of pumps should produce a proportionate increase in the quantity of water raised. Properly connecting his pumps, the result was as theory had justified him in expecting; and the reason why others had failed to attain the same end was that the supply of water had not been adequate, and air had got in instead of water.

On the Difference of the Steering of Steamers with the Screw reversed when under Full Way, and when Moving Slowly, by Prof. Osborne Reynolds.—Referring to the Report on this subject the author said the fact that the results which had been established by the Committee were so little known to pilots and seamen, besides being likely to excite surprise, would tend to cast a certain amount of discredit, if not on the truth of the results themselves, at least on their importance. It seemed as if nautical men must have formed their opinions from experience, and such was the faith of the English people in the practical that it was very difficult indeed for them to believe that a few landsmen, calling themselves scientific, could teach sailors how to steer ships. So strong was this feeling that it was to collisions they must look in the hope of preventing collisions. This sounded like a bull, but it was perfectly true, for nothing but disasters would awake our rulers to the idea that something was wrong. Fortunately, or unfortunately, such disasters were not wanting. There were the cases of the *Ville du Havre* and the *Loch Earn*, in which the collisions were undoubtedly due to the steamers having turned in the opposite direction to that intended. These and other disasters furnish evidence enough of the mistakes which had been perpetrated, and of the importance as well as the truth of the results the Committee had established. He fancied that the ignorance which existed was due to the fact that few seamen had turned a ship under full way with the screw reversed, and contented themselves by arguing as to what must happen in such a case from their experience in manœuvring their ships when moving slowly. Of such manœuvres they had had abundance, but as soon as they got beyond their experience, they adopted the seemingly obvious, but entirely erroneous opinion that the way of the ship would cause the rudder to act as if she was going ahead in spite of the screw being reversed. He felt strongly that in speaking thus in a town like Plymouth he ran the risk of being looked upon as impertinent. If he were wrong he was impertinent, and no one would feel it more than he should. It was not a pleasant task to point out imperfections, however accidental they might be. Even if one saw the wheel coming off an omnibus, all the thanks he was likely to get for pointing it out to the conductor was to be asked if he could not tell him something he didn't know. Of course they must learn as they went on, and all he, with deference, asked of seamen was to try the experiments for themselves, and then aid the Committee in bringing facts under the notice of the Legislature. Their own interests demanded this, for as things now were great injustice might be done to the captain who in a case of emergency adopted the very best course to save his ship.

Mr. William Froude thought the question which Prof. Reynolds had so ably dealt with of immense importance, and deserving minute consideration. Having himself had some experience of small steam launches, he had been surprised at the effect produced by the working of the screw, so that he concurred with the conclusion at which Prof. Reynolds individually, and the Committee collectively, had arrived. If sailors would occasionally listen to the advice of outsiders, it would do them no harm. Ignorance as to the effect of reversing the screw upon the way of a ship would often lead a captain into danger which might be avoided.

Sir William Thomson urged that the Committee should be reappointed, so that the Admiralty might have another opportunity of confirming or refuting the conclusions. This was undoubtedly nothing less than a national question, for the conflict now going on between Russia and Turkey proved that skill in manœuvring was of vital importance in torpedo warfare. In olden times the glory of England was maintained by the facility with which her ships were manœuvred, our navy being a match against the navies of all the world in this respect, and he hoped nothing would occur to destroy that pre-eminence.

On the Resistance of Ships, by Mr. William Froude, F.R.S.—The object of the paper was to show the effect produced on the resistance to a ship's motion by the lengthening or shortening of the flat middle body between the bow and stern. The results were based upon experiments made at Chelston Cross with models having the same ends, but different lengths of parallel body inserted amidships. By separating the effect of the frictional skin resistance, which was proportional to the wetted surface, he proved that the increase or diminution of the power required to propel a ship, in consequence of the alteration of the length of the parallel body, depended very largely on the coincidence, or want of coincidence, of the wave crests travelling alongside the ship with the points at which the reduction of breadth by the fine lines began. When this diminution coincided with a wave crest there was no loss, but rather a gain of speed; while when it coincided with a wave hollow the loss of speed, or increase of resistance, was considerable.

The Elevated Railway of New York, by Capt. Douglas Galton.—The first portion of this railroad was completed for steam-traction at the beginning of 1872, and was originally constructed for a rope railway, which did not prove successful. This section consisted of single "Phœnix" columns, nine inches in diameter, spaced from 26 to 30½ feet apart along the axis of the roadway, and carrying two pairs of rolled deck or "I" beams of shallow depth, one pair under each rail. This structure was originally deficient both in vertical and lateral stiffness. The deflections of the girders were too great, and the oscillations of the columns too large. In the next alteration the columns consisted of clusters of round solid wrought iron bears, four and a quarter inches diameter, grouped by two and by four, braced together so as to form a single support, and carrying rolled channel bearers, two under each rail. The bars were bent so as to branch like a Y at the top, the columns composed of two bars forming a bracketed support under the beams, and those composed of four bars giving longitudinal stability to the structure. Although imperfect and needlessly expensive, this style of column might be considered the parent of all subsequent improvements, and to have furnished a valuable hint for future designs. The line was single with sidings to allow trains to pass. Its length, including the sidings, was 7¾ miles, but it was now proposed to double the line throughout and extend it. The atmospheric brake, which was in use upon all trains on the line, placed them entirely under the control of the engineman, and was so effective that a train moving at a maximum speed could be brought to a full stop in a distance barely exceeding its length. The cost of this elevated railway for a double line was estimated at about 55,600*l*. It was simple in construction, and did not much interfere with street traffic in erection; it was very economical as compared with underground railways; it was pleasant to travel on; and it was comparatively free from risks of accident from collision; it was easy of access; the form of locomotive adopted was free from objection, as it was comparatively noiseless and did not appear to frighten horses when passing above them, and on the whole was more free from objection than any other form of road for rapid transit in towns.

Mr. G. Stephenson followed with a similar paper *On a New Safety Suspension Tramway or Light Railway.*

The Importance of giving a Distinctive Character to the Needles Light, by Sir Wm. Thomson, F.R.S.—He urged the necessity of giving a distinctive character to different classes of lighthouses, referring more particularly to the Needles light. He contended that the period of no revolving light ought to be more than half a minute, and stated that the three minutes revolving lights on the Irish coast had been done away with, and periods of a minute and half a minute substituted. To every fixed light a distinctive character should be given. Nine-tenths of our lighthouses had fixed lights, which had the advantage of being continuously visible, but lost the advantage of the great intensity of the revolving light. The distinctive character which he suggested should be given to the Needles and similar lighthouses was similar to the signals invented by Capt. Colomb, but instead of short and long flashes, he proposed to substitute short and long eclipses.

Sir Wm. Thomson read a paper *On an Improved Method of Recording the Depth in Flying Soundings, by substituting Chromate of Silver laid on by Albumen instead of Green Vitriol Solution.*

On the Eddystone Lighthouse, by J. N. Douglas.—He stated that the rock upon which this lighthouse was built had been so undermined by the sea that it had been determined to build another of larger dimensions at a distance of 120 feet from the present structure. He expressed a hope, however, that if Smeaton's wonderful handiwork were taken down, it would be considered worthy of another site on English soil.

On Recent Experiments in Telephones, by Prof. Graham Bell, of Boston.—He stated at the outset that after the lecture delivered by Mr. Preece it would be scarcely necessary for him to put before them a description of the construction and the operation of the telephone in its present form, but he thought it would be interesting if he took up the subject in another light, and showed them the evolution of the telephone, and described to them the process by which the instrument had been brought up to its present state. Having alluded to the fact that it was now some years since his attention was first directed to the form of the vibrations of the air during the production of speech, and having pointed out that he was not aware how the idea of using electricity as a means of conveying these vibrations from one place to another suggested itself to him, Mr. Bell gave an interesting account of the time and labour which he, assisted by Dr. Clarence, J. Blake, Prof. Pearce, and other friends on the other side of the Atlantic, had devoted in endeavouring to discover some means by which the sound of the human voice could be successfully conveyed to whatever place was desired. He gradually traced the progress of these researches, and enumerated the different forms of instruments which had been invented for the purpose of accomplishing the object desired, several of which instruments he exhibited, at the same time explaining that experiments were still being made in Boston with a view to further improvements in, and in the further development of, the telephone. He confessed that he did not yet know which was the best form of instrument that could be used, reminded his audience that he did not bring the invention before them as a perfected one, that it was still in embryo, but expressed a hope that at the next meeting of the British Association he might have the opportunity of producing before them still more perfect forms of the instrument. Prof. Bell then announced that he had brought with him his telephonic organ, and that he should presently attempt to produce a little "bad" music for the benefit of the Association. This organ, he explained, resembled a harmonium or parlour organ. The reeds were all connected with a battery, and in front of each reed there was a little screw with a platinum point. When the instrument was blown the reeds vibrated against the screws, which were all connected with a telegraph wire, which had been brought into that room, and contact being made, the music was thus conveyed. He also explained that the organ was in the Guildhall, and that telegraphic communication had been made between that building and the Post-office, and between the latter place and the room in which they were then assembled. Experiments with the instrument were then proceeded with, the telegraph wire being attached by Mr. Preece to a telephone with a powerful battery and with a somewhat capacious "mouth." Harris, Mr. Preece's assistant, who was stationed at the Post-office, was then communicated with by that gentleman, and told to request the organist to "strike up," and almost immediately the audience were astounded by hearing with perfect distinctness the well-known air, "God save the Queen." The organist was then ordered, through Harris, to play "something with chords," and again the sounds of music were clearly heard, although this time the tune could not be recognised. Another instrument without a battery, was then connected with the wire, but as Mr. Bell had prophesied, the sounds of music conveyed to the audience by means of this instrument were very faint, being audible only to those at the top of the room. The first instrument was now again used, Harris being requested by the professor to sing as loudly as possible. In a second or two the favourite song "Auld Lang Syne" was heard with remarkable clearness, although many of the notes were somewhat "shaky." Harris next read a newspaper paragraph, and although the sound of his voice was distinctly heard, no one was able to ascertain the subject of what he was reading. Prof. Bell explained the reason of this, and informed the audience that the louder the voice was at the transmitting end the more indistinctly it was heard at the other end.

THE FRENCH ASSOCIATION AT HAVRE

THE French Association commenced its Session this year at Havre on August 23. M. Broca, the well-known anthropologist, is president this year, and after a few remarks on the rapid and steady progress of the Association, he announced as the subject of his presidential address, "The Fossil Human Races of Western Europe."

M. Broca spoke of the antiquity of historical nations, showing that it has been very much exaggerated, mainly by the nations themselves, and that even in the case of Egypt the historical epoch cannot be pushed back beyond 6,000 or 7,000 years. M. Broca then showed that up to a very late period man's advent on the earth was universally accepted as very recent, long posterior to the last geological phenomena which have modified the conditions of life and produced changes in climate, and with these in the flora and fauna. The president then gave a brief sketch of the change of opinion which has taken place during the past thirty or forty years, on the question of the antiquity of man; pointed out with what incredulity the accounts of the first finds of human remains under conditions showing their antiquity were received, and that it was only after long years of labour, 1840 to 1858, that Boucher de Perthes at last managed to obtain a serious hearing for the argument in favour of the genuineness of his discoveries and of the antiquity of man. The English palæontologist, Falconer, went to Abbeville, in 1858, in order to examine at once the beds explored by Perthes, and the rich collection of cut-flints and bones which had been exhumed. M. Broca refers also to the early work in the same direction of Prestwich, Evans, Flower, and Lyell, stimulated by whose example, French men of science at last came forward in earnest to examine for themselves. The French Anthropological Society took the matter up, and the prudent and straightforward Isidore Geoffroy St. Hilaire at last declared that the last objections to the antiquity of man had vanished. Fossil man had proved his right to be received on the platform of positive science. The year 1859, which saw the doctrine of the antiquity of man make its way into science with irresistible force, was the beginning of one of the most fruitful of eras. New and boundless horizons were opened to the view of men of science; over all Europe geologists, archæologists, anthropologists, set themselves to work with astonishing activity. Only eighteen years have passed since then, and never, perhaps, in any past time, have we seen so rich a harvest.

Boucher de Perthes raised only a corner of the veil which conceals early humanity. He proved that man lived during all the quaternary epoch, that he was the contemporary of the reindeer and other animals which have since migrated, of the mammoth, and other extinct animals. But was this all? and is humanity not older still? This latter question, still more grave than the former, was soon asked; more grave, for the duration of each of the three periods of the tertiary epoch was incomparably longer than the quaternary epoch. But M. Broca did not intend to discuss the researches concerning tertiary man; the discoveries made by M. Desnoyers at St. Prest, near Chartres, and by Prof. Capellini in several tertiary beds of Tuscany, tend to establish the existence of man during the pliocene period; those of the Abbé Bourgeois in the commune of Thenay (Loir-et-Cher) carry back even to the miocene, *i.e.*, to the middle-tertiary, the existence of an intelligent being who knew how to cut flint, and who could be nothing else than man. But these facts, although collected by thoroughly competent observers, and although accepted after keen discussion by many eminent *savants*, are not yet sufficiently numerous nor incontestable to constitute a definitive proof. Tertiary man is not yet on the platform of science; he holds the place occupied by quaternary man twenty years ago. Will it be given to another Boucher de Perthes to demonstrate with irresistible evidence the existence of tertiary man? That is the secret of the future.

After referring to the vast amount of evidence for quaternary man obtained both in the Old and New World, M. Broca said that he is better known now than many peoples mentioned in history. We know enough to establish with certainty the multiplicity and the great diversity of quaternary races, and although the regions hitherto explored include only Western and a part of Central Europe, we can now, on this little corner of the globe, recognise and distinguish at least three fossil human races connected with two essentially different types. The two types are the dolichocephalic and the brachycephalic—the long-heads and the short or round-heads. Between these are the mesatocephalic.

By dividing the one diameter of the head by the other we obtain what is known as the cephalic index. The dolichocephalic are those whose cephalic index is less than $\frac{7}{9}$ or 77·7 to 100; the brachycephalic are those whose index is greater than the fraction $\frac{4}{5}$ or 80 to 100; the index of the mesatocephalic is between the two. But the variations of the cephalic index are so extended that it appears useful to distinguish two degrees in the dolichocephalic type; the dolichocephalic properly so called are those whose index descends below 75 to 100, the index of the subdolichocephalic being above that limit. So among the brachycephalic, we distinguish these properly so called from the subbrachycephalic, according as the index is above or below the fraction $\frac{5}{6}$ or 83·3 to 100. In consequence of many mixtures of races which have been produced before or during the historic period, these diverse cranial forms exist to-day with a varying degree of frequency among all the populations of Europe. We may conclude with certainty that the peoples of Europe are the issue of several races characterised by very different cranial forms.

After referring to the simple theory of Retzius, M. Broca said that the diversity of the races of Europe does not date from the almost recent era of the Asiatic invasions; it does not date from that long period of polished stone which preceded the introduction of metals, and which succeeded the age of the reindeer; it goes back to quaternary times. More, the dolichocephalic type, far from being the latest comer among us, is the most ancient of all; the migrations and the mixture of races, far from developing it, have only attenuated it; and these brachycephalics, who were formerly considered an autochthonous race, conquered and dispossessed by stronger and more civilised races, have been, on the contrary, foreign invaders, whose slow and progressive immigration modified in a manner as profound as durable the ethnology of Western Europe. They only appeared in the later times of the quaternary epoch. Before them two other races of dolichocephalic type had successively occupied the ground. M. Broca then proceeded to describe the chief distinctive characteristics of these three races, discovered by science after so many ages of oblivion. Names have been given to these three races after the places where their remains have been found, viz., Canstadt, Cromagnon, and Furfooz.

The race of Canstadt is the oldest. Its remains were exhumed so long ago as 1700 by Duke Eberhard, of Würtemberg, at Canstadt, near Stuttgard. These remains were found in the Würtemberg collections only in 1835 by Fred. Joeger, who recognised their importance. But the Canstadt cranium has not been universally accepted as genuine. Six or seven other incomplete crania, some fragments of jaws and long bones, are all that have hitherto been found of the Canstadt race. To these belong the Neanderthal skull and the lower jaw, found by M. Dupont in 1865 in the Naulette Cavern, in the valley of the Lesse, Belgium. The jaw has some very peculiar characteristics, and the Neanderthal skull recalls the form of that of the anthropoid apes. The Canstadt race seems to have been robust, of short stature, probably not exceeding from 1·68 m. to 1·70 m. The crania, though incomplete, show that the Canstadt race was "dolichoplatycephalic," *i.e*, long-headed, but with the top of the head much flattened. The marked dolichocephaly of the Canstadt race is to be found now only among the Australian and the Esquimaux. The platycephaly was due greatly to the obliquity of the forehead, which rapidly retreated. Although the occipital region was also prominent, yet the cranial capacity was small, and appears to have been smaller than that of the Hottentots and Australians. Other characteristics of inferiority were the prominence of the incisors, the great size of the jaws, the total absence of chin, and the total absence of the alveolar arch. If the skull found in the Forbes Quarry at Gibraltar be of the Canstadt type, as M. Broca is inclined to think, it shows still more marked characteristics of inferiority. The Canstadt race, he concludes, was certainly very savage, more savage than any existing race; its instruments were of the rudest kind, and it had to carry on a painful struggle for existence with the powerful mammals that then disputed the ground with them. Nevertheless its geographical extension was very great; it is found at Brux, in Bohemia; at Canstadt, in Würtemberg; at Neanderthal, in the Rhine provinces; Naulette, in Belgium; Eguisheim, in Alsace, at Paris, Arcy-sur-Cure, in Yonne, Mount Denise, in Haute-Loire, Olmo near Arezzo, Tuscany, and probably at Gibraltar. In Central and Western Europe then it maintained its place from the beginning to the middle of the quaternary epoch, when appeared another stronger and more perfect race which took the place of the former only probably after having nearly exterminated it.

This second fossil race was that of Cromagnon. It takes its name from a cave discovered in 1868, near the village of Eyzies, in the valley of the Vézère, Dordogne. This race, now represented by a score of crania, some almost complete skeletons, and a large number of bones, is comparatively well known. Though dolichocephalic like the Canstadt race, it otherwise differs completely from it. Its mean height was 1·78 m., and one skeleton measures 1·85. Its crania was equal if not superior to that of modern Parisians; forehead straight and high; vertical diameter well developed, and the cranial arch elevated; chin pronounced, and lower incisors vertical. The type as a whole approaches the Caucasian, though the upper incisors project somewhat, and the cheek-bones are high. The Cromagnon race is also characterised by its peculiar bones, its elastic-like femur, its platycnemic tibia, its channelled fibula, its arched cubitus; these characteristics, found now only in scattered individuals and much subdued, are normal to the Cromagnon race, and distinguish it from all modern races. With regard to the great capacity of the Cromagnon skull it should be remembered that among them the weak in intellect as in body would not survive as they do with us; still it shows a highly intellectual race, as is evidenced besides by the highly finished remains of their work which have been found. This race did not extend so far east as that of Canstadt. It has been found in Italy and probably in Britain; but it occupied especially France and Belgium. Its chronology coincides almost with the second half of the quaternary epoch, the age of the reindeer being that of its greatest prosperity. Its decline came with the departure of that animal. Still the race survived in some parts, and mixed with new races, and they have left behind them a lasting anthropological characteristic; even now their peculiarities occasionally appear in obedience to the law of atavism.

The Cromagnon race takes us down to the neolithic period; the Furfooz race leads us back to the reindeer. The latter race was discovered in 1866 and 1867, by M. Dupont, in several caverns on the right bank of the Lesse, near the village of Furfooz, Belgium. Crania, bones, and cave-dwellings have furnished materials for its study. The height of the Furfooz race was only 1·53m. to 1·62m., and descends even to the level of the Lapps. The bones are exactly similar to our own. Its only peculiarity was the elbow-perforation of the humerus, which, however, cannot be regarded as any mark of inferiority. With this race appears for the first time a rounded cranial type, which is not yet true brachycephaly, but which announces the arrival of the brachycephalics. The cranium as a whole is small, especially in its anterior parts; the forehead is narrow, low, and retreating, the vault little elevated, thus placing the race below that of Cromagnon, and nearer that of Canstadt. The face is smaller than the Cromagnon one, cheek-bones less prominent, the orbits narrower and higher, the nasal opening less extended compared to its breadth, the lower jaws smaller and thinner. The Furfooz race arrived in Belgium only at the end of the reindeer age. They lived in caverns and by the chase, but were inferior to the Cromagnons, their art and their weapons and implements being of a much ruder type. But they manufactured pottery, which is not found among the remains of the Cromagnon race, and which would indicate a date a little before the epoch of polished stone. This race was mesatocephalic or subdolichocephalic, and while they lived in Belgium, the true brachycephalics, with indices of eighty-three, eighty-five, and beyond, entered France by the eastern frontier. Their remains have been found at Solutré, in the Mâconnais. The discovery made in the loess at Nagy-Safi, near Gran, in Hungary, proves that the true brachycephalics lived on the Danube at the height of the quaternary epoch. Their immigration, however, belongs to subsequent geological periods belonging to the present geological epoch, and therefore not entering into M. Broca's subject; they may possibly have been modifications of the Furfooz race, by crossing and otherwise.

It is the problem of anthropologists to unravel these different elements as they appear in modern European races; and altogether we have no reason to be ashamed of our remote ancestors.

The Mayor of Havre followed M. Broca with a few warm words of welcome, when M. Deherain, the general secretary, gave a sketch of the work of the Association in 1876. M. G. Masson, the treasurer, made a statement as to the funds of the Association. For the past year its income has been 48,764 francs, and its expenditure 44,181 francs, of which 6,361 francs were given as grants for research. The capital of the Association at the end of 1876 was 210,307 francs.

The bad weather, our correspondent writes, has told upon the success of the various excursions organised. The uncertainty of public affairs, moreover, has caused the French papers to give the most meagre reports of the proceedings; indeed only the titles of a number of communications are given without any attempt at a report.

On Friday M. Fremy was elected president for 1878 by a full house, and almost unanimously. The place of the next meeting will be Paris in all probability. Consequently an opportunity will be afforded to influential members of the French Institute to give a new impulse to the organisation of the French Association, and to remodel it more fully according to the pattern of its English sister.

A number of members of the British Association arrived at Havre at the beginning of the session, including Dr. Huggins, Prof. Sylvester, Messrs. Glaisher, sen. and jun. It has been regretted that no formal delegation from France was sent to Plymouth, as contemplated, and that no direct request was sent to Mr. Bell to bring over his wonderful telephone.

A committee was appointed at Clermont-Ferrand to report on the position of French meteorology. A report was drawn up pointing out the necessity of memorialising the French Government to establish a special meteorological institute. The report was not adopted by the Meteorological Section, and a new report will be drawn up, and was to be proposed on Monday. But the discussion will offer little interest, owing to the absence of the leading French meteorologists.

The mathematical and astronomical section has been well attended under the presidency of Professors Catalon (Liége University) and Sylvester, the former being acting president and the second honorary.

The Geological Society of Normandy has organised an exhibition of local geology in the old Palais de Justice, which may be considered as a model of care, order, and completeness. A number of large oil pictures have been executed to show the different stages of the evolution of life before the appearance of man on earth, from the age of coal-measures.

M. Gabriel de Mortillet, general secretary to the section of anthropology of the International Exhibition, has delivered a lecture on the organisation of that section. The Trocadero Palace will be devoted entirely to "Histoire de l'Homme." One of the aisles will be devoted to the ethnography of living savage nations, and will be considered as affording a fair representation of primordial ages. The other aisle will be entirely devoted to the history of the arts, which are supposed to represent civilisation in its highest state of development. The central part of the building is devoted to anthropological science, viz., European anthropology, prehistoric anthropology, demography, comparative linguistic, &c. Exhibitors of all nations will be admitted, and all the expenses of the exhibition will be supported by the French administration. The space allotted to foreigners for this exceptional exhibition will not be reckoned as part of the total space granted to their own nation in the Champ de Mars. M. Gabriel de Mortillet, Chateau de Saint Germain, Seine-et-Oise, will answer any letters addressed to him, and give practical directions to intending exhibitors.

The scheme, of which we gave details some time since, has been conceived by M. Krantz himself, who was desirous to see the science of man utilised as an introduction to the exhibition of the works of man.

ENGLISH NAMES OF WILD FLOWERS AND PLANTS[1]

EIGHT years ago I was piloting a famous botanist from the east of England among the fields and lanes round Taunton, when he asked me the name of a plant which he did not at the moment recognise. I answered that it was the gipsy-wort, and received a prompt rebuke. "This is the third time," he said, "that I have inquired the name of a flower, and you have answered me in English. The Latin names are universal, the English at best are local. It is to be wished that all English names of plants could be forgotten, and their scientific names become popularised instead." Unquestionably a foolish utterance, it was of great service to myself, for it set me to consider the real value of these names which my pedantic guest despised, and from that time to this I have never encountered the popular name of any English wild flower without questioning it closely as to its etymological history and meaning, and noting the passages in our literature where it occurs. It would be a great pleasure to me to believe that the knowledge gained by these inquiries, put together to the best of my power, could interest you to-night as much as it has interested myself.

It is no new thing to infer from the terms in use at the beginning of a nation's history the arts and customs of the nation using them. Thus the fact that in all or nearly all the Aryan languages the words for the Supreme Being, for the king, for brother and sister, for ploughing, grinding, building, closely resemble one another, is admitted to show that our common forefathers in times when they were still one people, and had not yet scattered into India, Persia, Europe, had the beginnings of religion and government, possessed the family life, knew the simple arts which are most needed for the comfort of home life. Let us see what light will be thrown upon the habits of our Teutonic forefathers if we apply their method of investigation to the popular names of plants.

The following words are common to all the Teutonic languages; must have been known, that is, to the race from which we ourselves, with the Germans, Danes, Swedes, and Norwegians, are descended, on their first settlement in Europe, and before they broke up into sub-divided nations. The first I will take is *birch*, the rind of which must, we find, have been used for boat-building and for roofing houses; for boat-building, since the word *bark*, from the same root as *birch*, stands for ship in English, Dutch, Icelandic, Danish; for roofing houses, since the Old English *beorgan* and the German *bergen*, also from the same root, mean to cover, protect, or shelter. From this simple word, then, we gather that our ancestors possessed the arts of building boats and of roofing or thatching houses. Houses could not be built without timber; and we find the word *tree* in almost every Aryan language standing for three things—for a tree, for timber, and for an oak, extending the use of oak wood for building purposes back to the first formation in Asia of our mother language, and presenting us with the additional facts that our European ancestors built of oak timber the houses which they roofed with birch. In *hazel* a fresh fact lies buried. It is in all Germanic dialects the instrumental form of *hæs*, command or *behest*, a hazel stick having been used, as Jacob Grimm informs us, in the earliest times as a sceptre or baton to keep order among slaves and cattle. Without dwelling on the fact that the old word *hælsian*, to foretell, indicates the use of the hazel rod for purposes of divination, we have the additional probability revealed in a single word that our remote ancestors possessed slaves and cattle. In *hawthorn*, common to Swedish, German, and English, we have testimony to the use of a haw, *hæg*, *hedge*, or fence, "honouring the holy bounds of property," and consequently to the division and appropriation of land, in the earliest Teutonic time. My next word makes some demand upon your etymological credulity. Without tracing particulars, I will ask you to believe that the Sanskrit *Kshi*, to dwell, passes through various forms in one direction to the English *home*, in another to the word *heath*; now meaning the plant which grows wild on open land, standing originally for the land itself. "My foot," says Rob Roy, "is on my native heath;" and the same idea was enshrined in the same word to the first Teuton settlers. In the forest he fought his enemies, hunted his prey, hewed timber for his fences, and peeled bark for his roofs; his home was in the open land, or *heath*, from which, again, when ages had passed away and Christianity possessed the towns, he still worshipped his father's gods upon his father's heath, and gained, as Trench thinks, his ancient name of *heathen*. A sixth word lifts him higher than all the rest. The word *beech*, in Gothic, Old-High-German, modern German, Norse, Danish, Dutch, English, is identical with *book*, the Runic tablets of our ancestors having been carved upon this wood. In *sloe*, the wild plum, we have the root of *slay*, its tough wood having been used for bludgeons; *dog-wood* is *dagger*-wood, from *dag*, to strike; from *ash*, whose wood was therefore used for spear-shafts, came the Old English *æsc*, a spear; *sedge* is allied to *sæcg*, a sharp small iron sword. And let us observe that while all these plants, bearing purely Teutonic names, extend far into Northern Asia, trees which stop short at a more southern limit—the elm, chestnut, holly, sycamore, plum, pear, peach, cherry—all have Latin names, showing that the Teuton squatters came from a colder country than that in which they are supposed to have settled near the Roman Provincials on the Lower Rhine. The knowledge that wheat, barley, oats, corn, rye, are all Teutonic words, completes the historical picture given by the first list of names. They show us a race of men coming from a northern to a southern region, dwelling in timber houses, roofed and thatched, launching boats upon the rivers, possessing cattle and slaves, recognising the rights of property and the sacredness of home, fighting with cudgels, swords, and spears, familiar with cereal agriculture, in some way not ignorant of letters. All these facts, just hinted at here, but challenging minute investigation, we owe to a dozen common names of English plants, whose Latin equivalents teach and commemorate nothing of any national interest to ourselves.

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[1] Lecture by Rev. W. Tuckwell before the Somersetshire Archæological and Natural History Society.

These names, and a few more, are as old as the English language; but from the Conquest to the sixteenth century botanical inquiry ceased in England, and the rest of our popular names are little more than three hundred years old. Most of these come to us from the Greek and Latin; but some of them are so corrupted as not to be easily recognisable. Any scholar will detect in *acacia* the Greek word for *guilelessness;* in the *amaranth*, with which Milton's worshipping archangels wreathed their brows, the Greek for *unfading*; in the *periwinkle* the *pervinca* used to *bind about* the head; in *lettuce*, the meaning of *milky;* in *geranium*, the descriptive name *crane's bill.* In the *plane* he will see the *Platanus* of the poets; in the *rose*, the *Rhodon* of Homer and the *Rosa* of Virgil; in the *sycamore*, the wild fig of the Bible, transferred in mediæval miracle plays to the tree which now bears the name; in the *vine*, the *oinon* and *vinum*, whose Sanskrit root is still present in our words *twine* and *twist.* He will understand that the *basil* which poor simple Isabel planted in the pot which held her murdered lover's head was the *regal* plant, used perhaps of old in some royal bath or unguent; that the *angelica*, which now flavours our soups, and was once a specific against the plague, was given to mankind by angels; that the *belladonna* was applied as a cosmetic to make ladies beautiful for ever; that the *cyclamen*, which still grows wild in Devonshire, owes its name to its prominent circular tuber. He will not so readily discover that the *tansy* of our cottage gardens is the Greek *athanasia*, immortality, administered to Ganymede that he might become fit for his life in heaven; that the common milfoil *yarrow* is the *hiera*, or holy herb, pledged to heal all herbs with its fragrant leaves; that *nasturtium* means *nose-twister*, from its pungent smell; that our Quantock *whortle-berry* is a corruption of *myrtillus*, *myrtle-berry;* that *eglantine* is *aculenta*, the prickly rose, or sweet-briar; that the herb *Bennett* or *avens*, is the *benedicta*, *blessed* herb, kept in houses to prevent the entrance of the devil; that the *hip* of the dog-rose is a form of the Greek and Latin words which people afflicted with sore throats know as *jujubes;* that *liquorice* is an Anglicism of the Greek *Glycyrriza*, *sweet-root;* that the *larch* is from the Latin *lar*, a *house*, in consequence of its use in building; that *lavender*, from the Latin *lavare*, to wash, was in the twelfth century Scotch and northern English for *washerwoman*, because then as now its sweet spikes were laid amongst fresh linen; that the *service-tree* is the Latin *cerevisium*, *beer*, its leaves having been used to flavour ale before the virtues of the hop were known; that the little *sqainancy-wort* was the ancient remedy for the disease *Kynanche* or *dogchoker*, which we know in its modern sound as *quinsy;* that the *mushroom* is the *muscarius* or *fly-bane*, because a particular agaricus, pulverised and mixed with milk, was used in Southern Europe as we now use the poison called "Keating's Insect Powder." Least of all will our scholar be quick to admit that the *narcissus* owes nothing to the love-sick youth over whom Ovid sung and Bacon moralised, but is connected with the Greek *narkodes*, sluggish, a derivative from *narke*, the torpedo, itself sprung from the Sanskrit *nark*, *hell;* cited by Sophocles (Œd., Col., 682), as crowning the goddesses of Hades; gathered by Proserpine before her wedding tour into the same dark region, because its heavy odour (for by it the ancients meant the hyacinth) blunts the nerves and makes men sleepy and torpid. I can find comparatively few names which we have borrowed from the French. *Dandelion* is, of course, the lion's tooth; why, botanists are not agreed. *Mignonette* is applied by us to a very different plant from that which bears the name in France. *Woodruffe*, known to travellers in Germany as flavouring the pleasant drink called *Maitrank*, takes its last syllable from *roue*, a *wheel*, its verticillate leaves being set like a wheel or rowell on the stone. *Pansy* is *pensée*, thought, from its significance in the language of flowers: "There's pansy," says Ophelia, "that's for thoughts." *Gilliflower* is *giroflée*, from *caryophyllum*, a clove, a name originally given to the carnation, but now transferred to the wall-flower. *Tutsan* is *toute-saine*, the oil in its leaves having made it a remedy for wounds. Most curious of all is *Apricot*, from *abricot*, which at one time I contentedly referred to the Latin *apricus*, *sunny*, ripening as it does on sunny walls. It is, in fact, traceable to the Latin *præcoqua*, *early*, the fruit being supposed by the Romans to be an early peach. The Arabs took the Latin name and twisted it into *al burquq;* the Spaniards altered its Moorish name into *albaricoque;* the Italians reproduced it as *albicocco*, the French as *abricot*, and we get it next in England, curiously enough, as *apricock*, so spelt in Shakspeare's time, and finally as *apricot.*

Many curious bits of myth and history reveal themselves as we excavate down to these old meanings. The *pæony*, or healing-plant, commemorates the Homeric god Pæon, the first physician of the gods, who tended the bellowing Ares when smarting from the spear of Diomed. The *centaury* is the plant with which the centaur Chiron salved the wound inflicted by the poisoned arrow of Hercules. The *ambrose*, or wormwood, is the immortal food which Venus gave to Æneas, and Jupiter to Psyche; the Sanskrit *amrîta* which Kehama and Kailyal quaff in Southey's splendid poem. The *anemone*, or wind-flower, sprang from the tears wept by Venus over the body of Adonis, as the rose sprang from his blood—

αἷμα ῥοδὸν τίκτει, τὰ δὲ δακρυα τὰν ἀνεμώναν.

The *daphne*, *syringa*, and *andromeda* tell their own tales: the last, which you may find in the peat-bogs round Shapwick station, is due to the delicate fancy of Linnæus, who first discovered and named it, blooming lonely on a barren, rocky isle, like the daughter of Cepheus, chained to her sea-washed cliff. The *Juno rose*, or tall white lily, was blanched by milk which fell from the bosom of Juno, the tale being transferred in Roman Catholic mythology to the Virgin Mary and the milk-thistle. The yellow *carline thistle* is named after Carl the Great (in Mr. Freeman's county I must not call him Charlemagne), who, praying earnestly for the removal of a pestilence which had broken out in his army, saw in vision an angel pointing out this plant as a heaven-sent cure. The *herb Robert* healed a disease endured by Robert, Duke of Normandy, still known in Germany as *Rupprecht's-plage.* The *filbert*, though this is disputed, commemorates the horticultural skill of one king Philibert. The *treacle mustard*, a showy crucifer resembling wallflower, was an ingredient in the famous Venice treacle, compounded, as you will remember, by Wayland Smith to treat the poison sickness of the Duke of Sussex. The word *treacle* is corrupted from the Greek *theriacum*, connected with wild beasts, whose blood formed part of the antidote. It was at first made up by the physician to Mithridates, King of Pontus; and is still in many parts of England known as *mithridate mustard.* The *flower-de-luce*, or fleur-de-lys, is the flower of King Louis, having been assumed as a royal device by Louis VII. of France, though legend figures it on a shield brought down from heaven to Clovis, when fighting against the Saracens. It is probably a white iris.

Not a few strange superstitions and beliefs are embalmed in well-known names. The *celandine*, from *chelidon*, the swallow, exudes a yellow juice, which, applied by the old birds to the eyes of young swallows, who are born blind, or have lost their sight, at once restores it. The *hawk-weed* has the same virtue in the case of hawks. The *fumitory*, *fume-terre*, was produced without seed by smoke or vapour rising from the ground. The *devil's-bit* is a common scabious, with a premorse or shortened root, which was used so successfully for all manner of diseases, that the devil spitefully bit it off, and for ever checked its growth. The *eyebright*, or *euphrasy*, was given to cure ophthalmia.

> "Michael from Adam's eyes the film removed,
> . . . Then purged with euphrasy and rue
> The visual nerve, for he had much to see."

The *Judas-tree*, with its thorns and pink blossoms, was the tree on which Judas hanged himself. The *mandrake* gathered round itself a host of wild credulities. It was the *atropa mandragora*, a plant nearly allied to the deadly nightshade, but with a large forked tuber resembling the human form. Hence it was held to remove sterility, a belief shared by Rachel in the Book of Genesis, and was sold for high prices in the middle ages with this idea. In fact, the demand being greater than the supply, the dealer used to cut the large roots of the white bryony into the figure of a man, and insert grains of wheat or millet in the head and face, which soon sprouted and grew, producing the semblance of hair and beard. These monstrosities fetched in Italy as much as thirty gold ducats, and were sold largely, as Sir T. Brown tells us, in our own country. It was thought that the plant would only grow beneath a murderer's gibbet, being nursed by the fat which fell from his decaying body: hence it formed an ingredient in the love-philtres and other hell-broths of witches; and, as it was believed that the root, when torn from the earth, emitted a shriek which brought death to those who heard it, all manner of terrible devices were invented to obtain it. The readers of Thalaba will remember the fine scene in which the witch Khawla procures the plant to form part of the waxen figure of the Destroyer. I have seen the plant growing in the Cambridge Botanical Gardens ; it is not uncommon in Crete and Southern Italy ; its fruit is narcotic, and its name is probably derived from *mandra*, an inclosed, over-grown place, such as forms its usual home.

The medical beliefs revealed by many names are not less curious than their legendary associations. It was the opinion of the old herbalists or simplers that God had not only provided special plants as a cure for every disease, but had made their curative power evident by stamping them with some resemblance to the malady they were meant to heal ; and this faith, known to students of our older botany as the "Doctrine of Signatures," lurks or reveals itself in many an English name. The *lung-wort*, spotted with tubercular scars, was a heal for consumption ; the *liver-wort*, liver-shaped in its green fructification, was a specific for bilious maladies ; the scaly pappus of the *scabious* for cutaneous eruptions ; the throat-like corolla of the *throat-wort*, or Canterbury bell, caused it to be administered for bronchitis ; the saxifrage, cleaving the hard stones with its penetrating fibres, was efficient against calculus ; the *scorpion-grass*, now known as the forget-me-not, whose flower-spike dimly resembles a scorpion's tail, was an antidote to the sting of that or of other venomous creatures ; the *moon-daisy* averted lunacy ; the *birth-wort*, *kidney-vetch*, *nipple-wort*, *spleen-wort*, were all appropriated, as their names suggest, according to resemblances, real or fancied. The pretty toad-flax of our walls and hedges owes its name to a strange mistake. Believed to be the cure for a complaint called *buboes*, it received the Latin name *bubonium*. A confusion between *bubo* and *bufo*, which is Latin for a *toad*, gave birth to its present name ; and stories were not long wanting that sick or wounded toads had been seen to eat of it and to recover health.

Similar distortions occur in non-medical names ; and it is most curious to notice how soon a story springs up or a belief asserts itself in confirmation of the mistaken identity. The common *fumitory*, which we have already noticed, received its name of *fume-terre*, earth smoke, from its causing the eyes to smart and water when applied to them, as smoke does. The meaning was lost as time went on, and was supplied by the belief that it was produced without seed by smoke or vapour rising from the earth. *Buttercup* was said to give colour and flavour to butter, as being eaten by cows, when in blossom, the facts being that it is a corruption of *bouton-cop*, button-head, and that cows eat the grass all round it, but always, if possible, avoid it. *Meadow-sweet* is a corruption of *mead-wort*, *honey-wine plant*, a beverage being still extracted from it by cottagers. *Bullrush* is *pool-rush*, as growing in pools, not in mud ; *snapdragon* is *snout-dragon*, from its shape ; *marigold* is *marsh-gold ; sweet-william* is *œillet*, a little eye ; *pink* is the low German *pinksten*, Pentecost, from its flowering at Whitsuntide, the name being transferred first to the colour of the flower, then to a method of working flowers on muslin, called pinking ; and so to the sword-stab in a duel, piercing or pinking an adversary as the needle pierced the cambric. *Nightshade* is *night-scada*, soother, or anodyne ; *samphire* is St. Pierre, from its love of rocks ; *sanicle* is St. Nicholas, the restorer of the three murdered children, from its healing powers ; *poplar* comes from the Indian *pepul*, whose leaves when varnished and painted closely resemble those of the large Spanish poplar ; *primrose* was anciently the *daisy*, and is called by Chaucer *primerole*, from the old French *primeverole*, the first spring flower ; *primerole* was changed to *primrolles*, that to *primrose*, the first rose of spring ; and it was not till the sixteenth century that it attached itself to the familiar flower which now bears its name. *Cowslip* is more strange still. It was originally *hose-flap*, and belonged to the mullein, whose great flannelly leaf might well be likened to the flap or skirt of a woollen under-garment. Later on it was transferred to the wild primula of our meadows, and the mistake was stereotyped by the unlucky botanist, who in ignorance of its origin gave the name of *oxlip* to its pretty congener, the *Primula elatior*. The *Jerusalem artichoke* is a sun-flower, not an artichoke ; but the tubers resemble the artichoke in flavour. From its Italian name *girasole*, turn to the sun, came Jerusalem ; and by a further quibble the soup made of it is called Palestine soup. The *forget-me-not* was originally the *germander speedwell*, whose blossoms, falling off and flying away as soon as it is plucked, gave emblematic force to the name. It was known in the days of chivalry as the "flower of souvenance," and was embroidered into the collars of the knights, a fact still recalled by its German name *Ehrenpreis*, prize of honour. About 200 years ago we find the name given to the ground-pine, *Ajuga chamæpitys*, whose nauseous taste once realised can never be forgotten. Finally it was seized upon by the river-side Myosotis, and forthwith sprung up a charming legend, created obviously to suit its latest identification, how that while two lovers loitered by a lake, the maiden saw and longed for the bright blue flowers, the knight plunged in to get them, but, unable to regain the shore, had yet agility enough to fling them into his lady's lap, and then with a last devoted look and the words "forget me not," sank below the waves for ever.

Many names of plants contain the geography of their origin. The *Canterbury bell* is obvious, so is the *Guelder rose*. The *Alexanders*, a rare plant round Taunton, but growing in great quantities at Blue Anchor, comes from Alexandria ; the *candy-tuft*, from Candia ; the *elecampane*, from Campania ; the *medick*, from Media ; the *carraway*, from Caria ; the *walnut* or *Welsh nut* from the north of Italy, called Wälsh by the Germans. *Peach* is *Persicus ; shalot*, *Ascalonicus ; spinach*, *Hispanicus ;* the *damson*, rightly spelt as *Damascene*, tells its own tale, which is less clear in the case of the *Dame's* or *Damascene* violet, a corruption extended and perpetuated, as often happens, by its Latin equivalent, *matronalis*.

All first attempts at classification, etymological or other, leave a large margin of miscellaneous items refusing to be ticketed or systematised ; and there remain a few names falling under none of the categories which I have cited, yet too interesting to be omitted. Such is *apple*, retaining its form in the Teutonic, Celtic, Sclavonic, and Lettish languages, and springing apparently from the Sanscrit *ap*, water, which reappears inverted in the Latin *pa* of *Padus*, *po* of *Poto* and *Pomum*, meaning therefore the water fruit or juice fruit. Such again is *daffodil*, the *daffadowndilly* of Spenser and other poets. It is a combination of *sapharoun*, or saffron lily, with *asphodelus*, the old English *affodilly*. With the taste for alliteration often shown in popular names the sapharoun lily blending with the affodilly became by a mutual compromise daffadowndilly, whence daffodilly and daffodil. *Foxglove* is the *fox's glew*, or tintinnabulum, a ring of bells hung on an arched support. *Bedstraw* was a plant much used for couches before mattresses were invented, and a species which when dry yields a pleasant scent is still called *lady's bedstraw*. *Carnation* is *coronation*, its flowers being used as crowns or chaplets, just as *campion* is *champion*, gathered to crown the champions in a tournament. *Cress* is possibly from *cross*, its petals being cruciate ; possibly from *crescere*, to grow, in token of its rapid increase. It was used in Chaucer's time under the form of *kers* to express any insignificant quantity.

> "Of paramours ne raught he not a *kers*,"

from which comes, perhaps, our vulgar phrase, "I do not care a curse," though a yet ruder parallelism has since been manufactured to confuse its spelling and its etymology. *Nettle* is from *ne*, to spin, indicating that its coarse fibres were used for thread in early times, an idea borne out by Hans Andersen's beautiful tale of the wild swans, in which you remember that the princess was permitted to redeem her brothers from their transformation by weaving them shirts of nettles. *Shamrock* is from an Erse word signifying the *little trefoil*. The story of its theological use by St. Patrick is of modern date, and it has been taken by various writers to represent the *water cress*, the *wood sorrel*, the Dutch *clover*, and the *black medick*. Irishmen are divided in the present day between the two last, which are sold on St. Patrick's day both in London and Dublin. The *snowdrop* is so-called from its resemblance to the large eardrops worn by ladies in the sixteenth century, and represented often by painters of that period. The *tobacco* was the Indian name for the pipe in which the weed was smoked, not of the weed itself ; and *potato* belonged at first to a tropical convolvulus, and was transferred by mistake to the well-known esculent. The *gooseberry* was the *cross-berry*, from its triple spine, which frequently takes the form of a cross. The *hollihock* is the *cauli-hock*, *hock* being an old name for the mallow, to whose order it belongs, and *cauli*, meaning cabbage, either from its lofty cabbage-like stalk, or, as in cabbage-rose, with reference to its rich double bloom. The *laburnum* closes its petals at night-fall like a tired *labourer*, and the *ozier* is named from the *oozy* beds which suits its growth.

I bring my list to an end, not because it is exhausted, but for fear my hearers should become so. I have picked only the most suggestive and curious of our many floral names, leaving an abundant gathering to many gleaners. One branch of the subject I have barely touched, the superstitious practices attaching to many of our wild plants, though not surviving in their names. I have left alone the interesting question of Bible plants, of the hyssop, the juniper, the mustard-seed, the lilies of the field, the burning bush, the shittah, the almug, the gopher, the curiously mistranslated cab of dove's dung, with the light thrown upon their identity by the names given to them in the commentaries in our older translations. Nor can I do more than hint at the rich store of literary allusion to our wild flowers which abounds in all English poets, and the beautiful thoughts suggested to many of them by some particular plant. I should have liked to read you Chaucer's lines upon the daisy, Herrick's on the daffodil, Burns's on the dog-rose, Shelley's on the sensitive plant, Southey's on the holly, Wordsworth's on the lesser celandine, Longfellow's on the compas-plant. I should like to open volume after volume of Elizabethan and of later days; to enumerate and discuss the flowers with which Ben Jonson bids us "Strew, strew the smiling ground;" the "pretty paunce and chevisaunce," of Spenser; the "quaint enamelled eyes" that decked the laureate hearse of Lycidas;" the silver globes of guelder rose" which won the heart of Cowper; the "hawthorn bush beneath the shade" of Goldsmith's lovers; the "slight hairbell" which raised its head, uncrushed by the airy tread of Ellen Douglas. I should like to remind you of the lessons in natural theology which Paley drew from the "little spiral body" of the dodder seed; of the star-shaped shadow of the daisy which Archer Butler showed to Wordsworth, or how Linnæus, when he first saw the wild broom in flower,

> "Knelt before it on the sod,
> For its beauty thanking God."

Above all I should love to turn with you the page of Shakespeare; to read of the grey discrowned head of Lear wreathed with "rank fumiters and furrow weeds;" of Perdita at the shearing feast disparaging the streaked gilliflowers as Nature's bastards; of poor distraught Ophelia distributing her rosemary and herb of grace; of Puck telling how love in idleness was purpled with love's wound; of Titania gently entwining the "female ivy and sweet honeysuckle" round the sleek smooth ass's head of Bottom; of Helena and Hermia, "a double cherry seeming parted, two lovely berries moulded on one stem." For I should lay on you a spell mightier than I can forge myself; I should invoke allies before whom we all bow as the source of our intellectual happiness and growth; I should remind you how the most creative minds have drawn nutriment from these tenants of our hedgerows and hill-sides, and how the knowledge of their lore helps us in its turn to interpret the sweet thoughts and apt illustrations of the poets they inspired and delighted: how, if the aspirations of my Cambridge botanist were fulfilled; if the daisy could become the *bellis*, the strawberry the *fragaria*, the honeysuckle the *caprifolium*, the heather the *calluna*, the parting genius of romance and myth and association and folklore would be sent sighing from the domain of botany; and the richest and most attractive of the natural sciences would become the dullest and the most neutral.

In conclusion, let me disclaim all merit of originality in the ideas which have been put before you to-night. I have but attempted to bring together, with the interest attaching to cumulative illustration, conjectures which have been started and discoveries which have been worked out by others. Scattered through the old-fashioned tomes of Coles, Lyte, Parkinson; through the pleasant pages of Loudon, Pratt, Johns; above all in that most valuable work on popular botany which we owe to our Somersetshire naturalist, Dr. Prior, you will find all or nearly all that I have advanced. The flowers were plucked by other hands; mine has been only the *pia dextera* to sort and wreathe them.

NOTES

We greatly regret to record the death of Mr. J. P. Gassiot, D.C.L., F.R.S., which took place on the 15th inst., the opening day of the Plymouth meeting of the British Association, at the age of upwards of eighty years. Sir Wm. Thomson referred to Mr. Gassiot at the concluding meeting of the Physical Section in terms of the highest appreciation. His experiments with the vacuum tubes, an account of which will be found in the Royal Society's publications, extended over many years, and he varied them in very many ways, in order to throw light on the theory of the stratified discharge. Mr. Justice Grove worked a great deal with Mr. Gassiot, who continuously for many years experimented with a battery of high potential, beginning with a battery of 500 water, and ending with 3,500 *Leclanché* cells. He spared no expense or trouble in his own researches, and in making known to Englishmen the researches of continental physicists by the purchase of similar apparatus to that they had employed. At his scientific gatherings one met the eminent men of all nations, and in the early days of the British Association they generally assembled after the meetings at Clapham Common. Before his death he distributed the greater part of his apparatus; much of it was given to the Cowper Street Middle Class School, and his vacuum tubes (in very great numbers) to Mr. Spottiswoode. He was a generous patron of science, and a helper of scientific men. He has munificiently endowed the Kew Observatory and the Cowper Street Middle Class School, and was the founder of the Royal Society Scientific Relief Fund. His untiring activity enabled him to take an active part in the administration of some of the largest public companies, and though in years he lived a very long life, by his activity he may be said to have lived twice as long. He was the intimate friend of Faraday, and most men of eminence in England and abroad; those living will recall, when they hear of his death, the many pleasant and profitable hours spent at Clapham Common.

We learn from a correspondent in Alexandria, under date August 12, that the obelisk is now nearly quite inclosed in its iron casing, and its launch may take place in another fortnight or so. "It is now receiving an outer skin of strong thick planks, to protect the casing from injury when it is rolled down the inclined plane into the sea. Two delicate engineering operations have to be got over before it is ready for the launch. The first is to let down the obelisk on to its bed in the cylinder, and, that accomplished, to complete the riveting of the lower plates, and then let the whole down on to the ground; for at present the obelisk and cylinders are supported above the ground independent of one another. There will be no ceremony at the launch as the state of the sea may prevent the operation at any fixed time; and a calm day will have to be selected. The *fête* will take place when it is ready for sea after being docked in the Great Harbour. It has yet to have a rudder and bilge keels fitted, besides the cabin, wooden deck, mast, sail, &c. It will be painted bright-red and bear the name of "Cleopatra." If met at sea, it may be taken for a torpedo boat, and avoided accordingly. One side, the part which remained undermost, is in beautiful preservation, but other sides are more or less eroded; still, when erected and seen at a distance the hieroglyphs will probably appear more sharply defined than when seen close and in a prostrate position."

Prof. E. S. Morse, of Salem, Mass., is now busy with dredge and microscope in Japan, having fixed his headquarters at Inoshima, seventeen miles south of Yokohama. Recently he ascended one of the highest of the Japanese mountains, about 100 miles from the coast, and found opportunity there for dredging Lake Chiusenji, a body of water 4,000 feet above sea level. Its fauna was ascertained to be quite peculiar. Prof. Morse will return to the United States in time for his usual courses of lectures during the coming autumn and winter; but afterwards, in 1878, he expects to go back to Japan, having accepted an engagement in the Imperial University of Tokio, as professor of biology. He has also projected a summer school of natural history, to be conducted on the coast near the university; his text-book for beginners in zoology is to be translated into the language of Japan, and animals native to that country are to be substituted for the American ones referred to throughout the volume.

THE *New York Tribune* tells us of a practical application of the telephone. Mr. J. L. Haigh, the contractor for manufacturing the wire for the Brooklyn Bridge, put up a telephone a short time ago, connecting his establishment with the Bridge Superintendent's office. Mr. Cheever, the agent for the telephone in the United States, has lately placed in New York several telephone instruments and wires. One of these connects his office with the Champion Burglar Alarm Company's office at Thirteenth Street and Broadway, using one of their old telegraph-wires, between three and four miles in length, as the medium of communication. Mr. Cheever has another wire running to Broad Street, in communication with an establishment engaged in the construction of telegraph lines. Mr. Cheever is erecting a line for the Clyde Steamship Company from its office in Bowling Green to Pier No. 2, North River, from which its steamships sail. This is a circuitous line, about five miles in length. The piers of the Brooklyn Bridge are also being connected by telephones with the superintendent's office, so that all the movements of the "travellers" in carrying the wires across from pier to pier can be communicated and directed without the use of signal-flags as heretofore. The current of sound in these telephones is carried by a single wire in either direction. All that it is possible to do in ordinary conversation between two people sitting within two feet of each other in a room can be done at the distance of five or ten miles, or even a greater distance, by simply raising the voice and speaking a little slower than naturally. The telephone instruments themselves are very simple, consisting of two wooden tubes, one of which is placed at the mouth, the other at the ear. The extension of these telephones all over the city in place of the electric telegraph is probblay only a question of time.

FROM the *New York Tribune* we learn that the first act of the new College Administration at Amherst has been the purchase of all the Shepard Scientific Collections located in the college cabinets. These collections, the private property of Prof. C. U. Shepard, were removed from New Haven to Amherst in 1847, through the influence of Prof. Hitchcock, and have remained there ever since, receiving constant personal attention from the Professor, and from the College such enlarged accommodations as their growth required. The purchase has been made for 40,000 dollars, or less than half the appraised value of the minerals, the College thereby securing for itself not only all the material necessary for study in this department, but also, to use the Professor's own words, a collection "which, besides being the largest ever formed by one individual, is actually the best now possessed by any college or university in this country or in Europe." The collections are three in number: viz., a mineralogical, a geological, and a meteoric. Of these the first is the most important and perfect, containing specimens illustrative of almost all the species of the twenty-two orders, selected with the utmost skill and appreciation of scientific beauty. The meteoric collection is the fourth for size and value in the world, the three others which outrank it being those of the national museums at London, Vienna, and Paris.

PROF. PIAZZI SMYTH sends to the *Edinburgh Courant* a memorandum from the Edinburgh Royal Observatory, dated at 2 P.M. August 21, in which he states that the 24-hour period then closed had witnessed by far the heaviest rainfall ever recorded there within an equal interval of time, having amounted to no less than 1·940 inches. "This particular storm," the memorandum states," which has been marked throughout by a heavy rain-band in the prismatic spectrum of the daylight, though by no particular fall of the barometer, commenced on August 17 with a veering of the wind from the west towards the east by way of the north, in which easterly position it settled for the four following days, or up to this time, when it has now gone back by the north to the west, and the depths of rain found in the rain-gauge each day have been thus :—

	in.
August 18	0·177
August 19	1·349
August 20	0·794
August 21	1·940

making a total of 4·260 inches in four days; in a climatic position, too, viz., the roof of this observatory, where the mean *monthly* fall throughout the year is only 2·091 inches."

IT is worthy of note in connection with the present exceptionally wet weather in this country that the rainfall in Victoria this season has been below the average, and the weather cold.

A SEVERE shock of earthquake was felt in Melbourne on June 25, at 3.30 A.M.

AS already announced, the fiftieth meeting of German naturalists and physicians will take place at Munich on September 17–22. Among the visitors who have announced their intention to read papers, are Professors Waldeyer (Strassburg), Ernst Haeckel (Jena), Tschermak (Vienna), Klebs (Prague), Dr. G. Neumayer (Director of the German Observatory, Hamburg), Virchow (Berlin), Dr. Avé Lallemant (Lübeck), and Günther (Anspach).

THE British Archæological Association commenced its annual meeting at Llangollen on Monday, under the presidency of Sir Watkins Williams Wynn, M.P.

THE first annual meeting held in Scotland of the Institute of Naval Architects was opened at Glasgow on Tuesday morning, when the members were received in the Corporation Galleries by the Lord Provost. Lord Hampton, the president, spoke of the immense amount of shipbuilding carried on in Glasgow. Papers were read "On Transverse Strains in Ships," by Mr. W. John, and on "Abnormal Influences in the Direct Motion of Steam-Vessels," by Mr. Robert Mansel. The members in the afternoon visited several of the shipbuilding yards in the neighbourhood.

THERE have been just added to the South Kensington Museum six models illustrating the cliff houses, cave dwellings, and lowland settlements met with through the district where the States of Utah, Colorado, Arizona, and New Mexico join. A series of models of the same kind was shown at the Philadelphia Exhibition, and through Sir Herbert Sandford these six have been generously presented by the United States' Government to this country. They are reduced to different scales, the cave dwellings being of smaller scale than the lowland dwellings, since with the former the surroundings are given, while with the latter they are not.

THE Archæological Congress at Kazan was opened on August 12, and will continue its sittings during three weeks. The Congress is divided into seven sections, one of which deals with pre-historic man. The questions to be discussed are of great interest, as well as the very varied archæological exhibition opened in connection with the Congress. Several excursions will be made by the archæologists along the banks of the Volga, and during one of these a *koorgan* (mound) will be excavated

AS we have already announced the Sanitary Institute of Great Britain will hold its Autumn Congress at Leamington from October 3 to 6. The president will be Dr. B. W. Richardson, who will open the Congress with an address on the evening of the 3rd. Other addresses will be given on the 4th and 5th, and among other papers to be read will be one by Surgeon-Major de Chaumont on "The Effects of Climate upon Health," by Mr.

W. Eassie, C.E., on "The Influence of Vegetation on Human Health," and by Mr. A. Haviland on "Geography of Disease in Relation to Sanitary Science." There will be an exhibition of sanitary apparatus, appliances, and articles of domestic use and economy, in the Drill Hall, Leamington, from October 3 to 18, in connection with the Congress.

RUSSIAN papers announce the return to Kuldja of Col. Prshevalsky. He has brought with him very interesting zoological collections, the most important of them being the skins of three wild camels.

PROF. WAGNER is engaged now in the organisation of zoological stations on the White Sea. One of these will be organised on the shore of the Anzerski Strait, the other on Cape Orlovsky, and the third on the Sviatoi Nos.

MESSRS. W. AND A. K. JOHNSTON have published a small pamphlet from the pen of Dr. Andrew Wilson, on the Colorado beetle (*Doryphora decemlineata*), with an excellent and much magnified coloured representation of the insect, and another of *Doryphora juncta*, "the Bogus potato-bug" of the colonists. An account is given of the structure and habits of the species, together with the most successful methods that have been employed for its destruction. Why *D. juncta* is depicted on the outside of the paper with the heading "The Colorado Potato Beetle" just above it, we are at a loss to comprehend. It is misleading, to say the least. We have also received from Messrs. Routledge a reprint from one of the Reports of Mr. C. V. Riley, the State Entomologist of Missouri, on the Colorado beetle, and from Mr. Stollwerck, of Cannon Street, a very successful model of the beetle at its various stages in a neat little case. The model has been made by Stollwerck Brothers, of Cologne, by order of the German Government, and has been widely distributed all over the country, in schools, &c.

WE notice an interesting report, by M. Kamensky, on the cotton-tree culture in Turfan, read at the last meeting of the St. Petersburg Society for the Protection of Trade.

FROM the *Twenty-third Annual Report* of the Brighton and Sussex Natural History Society we are pleased to see that the society is in a state of continued prosperity. The report contains many papers read at the society's meeting, most of them scientific, and many of them interesting and valuable. An equally satisfactory account of progress is given in the *Ninth Annual Report* of the Eastbourne Natural History Society, which also contains a number of interesting papers read at the meetings.

WITH reference to Galileo's claim to be the inventor of the telescope, M. Wolf quotes (*Annalen der Physik und Chemie*) from a manuscript of Scheiner (1616) in a library in Zurich, a curious passage, of which the following is part: "It must be allowed first, considering what the telescope does, that Baptista Porta has better right to be thought the inventor, because he describes, after his own way, in obscure words and puzzling expressions, an instrument like the telescope. But secondly, if we speak of the telescope, as it is now used after general perfection, we must say that neither Porta nor Galileo is the first discoverer of it, but the telescope in this sense was discovered in Germany, among the Belgians, and that accidentally by one Krämer, who sold spectacles, and either for amusement, or experimentation, combined concave and convex glasses, so that with both glasses he could see a quite small and distant object large and near; at which success being rejoiced, he united several similar pairs of glasses in a tube, and offered the combination at a high price to wealthy people. Thereafter they (the telescopes) became gradually more common among the people, and spread to other countries. In this way two of them were brought for the first time by a Belgian merchant to Italy; of these, one remained long in the college at Rome; the other went first to Venice, later to Naples; and here the Italians, and especially Galileo, at that time Professor of Mathematics in Padua, took the opportunity of improving it, in order to apply it to astronomical purposes, and extend its use further. Thus the telescope, as we have it to-day, was discovered by Germany, and perfected by Italy; the whole world now rejoices in it."

EXPERIMENTS have recently been made at Dortmund, on the Cologne-Minden Railway, with a newly-invented steam-brake, and on the whole they were crowned with success. A railway train travelling at full speed was brought to a standstill in the remarkably short time of twenty seconds, and the inventor is confident to be able to reduce this time to eighteen seconds.

WE have received the programme of the St. Thomas Charterhouse School of Science for session 1877-8, which commences on September 29. It is as well arranged as before, and we notice that another series of Gilchrist Lectures will be given this winter, by Dr. B. W. Richardson, on physiology. One of the subjects to be taught this session is "advanced and elementary physiography."

THE *New York Tribune* of August 10 and 11 devotes about six columns, with illustrations, to a description of the contents of the Peabody Museum, Yale College. Why does not some enterprising English "daily" try, by a similar experiment, whether the English public is ripe for such reading?

THE property of certain salts of cobalt (such as the chloride) to assume a blue colour in dry air and to change to pink during moist weather, has lately been utilised for ladies' hats and bonnets. An enterprising *marchand de toilettes* at Paris has added to his "nouveautés" artificial flowers covered with the salts in question, and christened them "barometers." "Hygrometers" would perhaps be more correct, but then the barometer is the old established weather prophet of the enlightened millions.

THE law deduced by Baer from observation on Russian rivers, regarding influence of the earth's rotation on the form of river banks and beds, has received confirmation by various observers since. The attention has been almost exclusively directed, however, to rivers flowing in meridian direction. And a like remark applies to investigations of the pressure arising from the earth's rotation on one of the rails in railways. In a recent paper to the Vienna Academy, M. Finger enlarges the problem beyond this and other limitations, studying the influence of the earth's rotation on movements (especially of rivers and winds) in any paths parallel to the *spheroidal* (not *sperical* surface of the earth. One surprising result is, that even when the azimuth of the direction does not vary, the lateral pressure to the right is not (as the adherents of Baer's law suppose) greatest for a motion along the meridian, nor has it the value indicated by the law for all azimuths, but it depends on the value of the azimuth, and, with conditions otherwise equal, it is greatest for a motion towards the east, and least for a motion towards the west. With regard to vertical pressure of a body moving along the earth's surface, M. Finger finds that in consequence of the earth's rotation alone, even if the temperature and vapour conditions did not vary, there would be an influence of wind-direction on the state of the barometer, small, indeed, but in the case of strong winds by no means to be negleted, so that a higher barometer would correspond to the east winds, a lower to the west.

A REGISTERING "physiological balance" has recently been devised by M. Redier, at the instance of M. Grandeau, for an agronomic station, its object being to represent in curves the gains or losses of weight of any matter (soil, plant, animal, &c.) placed in one of its scales. With three of these instruments (one carrying bare soil, another a plant in the same soil exposed to air, and the third a similar plant, but with its stem passing up through a covering obturator), and with a dry and a wet registering thermometer, M. Grandeau hopes to be able to settle some important questions relating to quantity of water required by a given species, transpiration, quantity of evaporation from ground under various conditions, &c.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Miss Gover; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. G. B. Southern; a Sœmmerring's Antelope (*Gazella sœmmerringi*) from Abyssinia an Arabian Gazelle (*Gazella arabica*) from Arabia, presented by Capt. F. Cotton; an Emu (*Dromæus novæ-hollandiæ*) from New South Wales, presented by Mr. F. Green; a Slender-billed Cockatoo (*Cacatua tenuirostris*) from South Australia, presented by Major M. Pasley, R.A.; Crested Ground Parrakeet (*Calopsitta novæ-hollandiæ*) from Australia, presented by Mr. Salisbury Baxendale; a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, deposited.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale des Sciences de Belgique, No. 3. On the pension-fund of widows of officers of the Belgian army, by M. Liagre.—Some curious examples of discontinuity in analysis (continued), by M. Plateau.—Reply to M. Terby's criticism on the map of Mars, published in the *Terres du Ciel*, by M. Flammarion.—Theorem on the Arquesians, by M. Saltel.—Applications of the method of analytical correspondence and of the law of decompositions to certain left curves, by the same.—Observations at Rome on the magnetic needle and the solar spots during 1875, by Abbé Spée.—Microscopic researches on the anatomy of the cochlea of mammalia, by M. Nuel.

No. 4.—On the theory of continuous periodic fractions, by M. Le Paige.—Studies on the planet Mars (10th notice) by M. Terbe.—Continuation of theorems on regular polygons, by M. Reynemund.—Fragment of tourmaliniferous rock from puddingstone of Bonsalle, by MM. Poussin and Renard.

No. 5.—Application of the rhe-electrometer to the lightning-conductors of telegraphs, by M. Melsens.—Some remarks on the winter of 1876–77; periodicity of mild winters and hot summers, by M. Lancaster.—On subnormal polars and radii of curvature of plane lines, by M. Ghysens.—Morphology of the dental system of human races, by M. Lambert.—Stratigraphic arrangement of fossil seals collected in the strata of Antwerp, by M. Mourlon.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, August 1.—Mr. J. W. Dunning, F.L.S., vice-president, in the chair.—Mr. Stevens exhibited specimens of *Teretrius picipes*, Fab., one of the *Histeridæ* taken on a fence at Norwood. He also remarked on the appearance of a second brood of *Colias Edusa*, of which he had observed several males.—Mr. F. Smith exhibited (on behalf of Dr. Bennett of Sydney, who was present at the meeting), a fine pair of the beautiful and rare *Eupholus Bennettii*, Gestro., from Yule Island, New Guinea. It had been described under that name in the *Annali di Mus. Civ. di Genova*, viii. 1876.—The secretary exhibited a specimen of an insect forwarded to him by Mr. Bewicke Blackburn, who stated that a large field of mangolds belonging to the Knight of Kerry, in the island of Valentia, had been totally destroyed by it. It was believed to be the larva of some Coleopterous insect, but in consequence of the imperfect condition of the specimen, it could not be determined.—Mr. R. A. Ogilvie forwarded (through Mr. Douglas), specimens of an insect found in great quantities in a jar of pickles (piccalilly), devouring the pieces of cauliflower in the jar. Prof. Westwood had pronounced them to be the dipterous *Drosophila cellaris*, an insect commonly found in cellars and cupboards, delighting in stale beer, wine, &c. In answer to a question asked by Mr. Ogilvie, he said that the eggs were laid in the pickle-jar, and not in the cauliflowers before they were pickled.—Mr. Douglas also forwarded a letter from Mr. A. H. Swinton, of Guildford, inclosing a specimen of *Myrmica ruginodis*, which, on being placed under a wine glass, stationed itself near the rim, head downwards, and rapidly vibrating the abdomen, continued "an intense noise," resembling the spiracular piping of the dipteron, *Syrilla pipiens*.—Mr. Enock remarked that a specimen of a spider taken by himself at Hampstead, and exhibited at a previous meeting by Sir Sydney Saunders as *Atypus sulzeri* had been since submitted to the Rev. O. Pickard, Cambridge, who stated that it was certainly not *A. sulzeri*, but probably *A. beckii*, Cambridge, which he believed to be the same as *A. piceus*, Thorell, though he was not certain as the only specimen he had examined of *A. beckii* was a female, and until he could obtain the other sex, he could not give a decided opinion. He added that he would be glad if collectors in the Hampstead locality would search for the males during the next autumn and winter, as it would help him to clear up the difficulty as to the species. A discussion then took place with reference to the exhibition by Mr. Jenner Weir, at the last meeting, of a specimen of *Cicada montana*, which was reported to have been distinctly heard to stridulate, notwithstanding that the insect was a female, and also that the species was one of which even the males were not known to stridulate. Mr. Weir stated that he had, since the last meeting, again visited the New Forest, and had seen in the possession of Mr. James Gulliver two specimens of *C. montana*, and he was assured by Mr. Gulliver that the fact of it stridulating was well known to him, and that he was guided by the sound so made, in effecting the capture. Mr. Champion said that he himself had captured the insect, and had distinctly heard a loud buzzing noise, but whether that sound was caused by the males or females he could not say. Mr. Dunning considered that farther evidence was wanting to prove stridulation in the females.—The following papers were communicated, viz.: Notes on the new and rare species of *Sphingidæ* in the Museum of the Royal Dublin Society, with remarks on Mr. Butler's recent revision of the family, by W. F. Kirby.—Descriptions of new genera and species of *Cryptocephalidæ*, by J. S. Baly.—Descriptions of new species of *Cleridæ*, by the Rev. H. S. Gorham.

GENEVA

Society of Physics and Natural History, May 3.—Prof. Plantamour gave the results of the determination of the difference of longitude between the observatory of Zurich and the geodetic stations of the Gäbris (Canton Appenzell), and of the Pfänder (Austrian Vorarlberg), at which he has worked with MM. R. Wolf and Oppolzer. The two last observers have had to guard against the influence of the electric register on the rate of their pendulum, which was sometimes affected to the extent of one-tenth of a second.—Prof. Plantamour also referred to a particular fact which has been manifested by the corresponding observations made by him at Geneva, and by Col. Orff at Munich, and where the instants marked are influenced by the inclination to right or left of the head of the observer, according to the position he must take to apply his eye to the telescope. There is here a physiological or psychological phenomenon which deserves attention.

VIENNA

Imperial Academy of Sciences, June 14.—Action of bromine on phloroglacin, by M. Benedikt.—On the means of acid formation in the animal system, and on some phenomena of blood-serum, by M. Maly.—A new proof of Pohlke's fundamental proposition, by M. Pelz.—On a proposition relating to the theory of the higher equations, and on development of the root expression of a quadratic equation, by M. Zimels.—Testing of a method for determination of the water in silicates, by M. Sipöcz.—On formation of pimelin acid in action of a mixture of hydroxide and cyanide of potassium on bromide of amylene, by M. Bauer. The Cœlenterata, Echinodermata, and Worms of the Austro-Hungarian North Polar Expedition, by M. Marenzell.—On the spots in the xylema of leafy and resinous trees, by M. Kreuz.

June 21.—Orthoptera of Senegal, by M. Krauss.—On the probable connection of the wind with the period of sun-spots, by M. Hornstein.—On the determination of the value of a circle by an immediate method, by M. Georgievicz.—Observations on the nerves of the cornea and their vessels, by M. Königstein.—On the influence of the earth's rotation on the movements of any kind parallel to the earth's spheroidal surface, especially the currents of rivers and winds, by M. Finger.

July 12.—On the fresh-water fishes of South-Eastern Brazil (4th part), by M. Steindachner.—On the recurrence of two different kinds of bundles of vessels in the kidneys, by M. Drasch.—On the compounds of the camphor group, by M. Kachler.—On the substances besides anthracene occurring in crude anthracene, on carbazol, and on the behaviour of camphor to hydrate of chloral, by M. Zeidler.—Theory of the functions $Cv_{n}(x)$, by M. Gegenbauer.—On intermediate cells in the large antheridium cell of the pollen grain of some Coniferæ, by M. Tomaschek.—On the properties of dialysed egg albumen, by M. Laptschinsky.—The volcano of Monteferru, in Sardinia, by M. Doelter.—On a relation corresponding to the linear differential equations of the second order, by M. Winckler.—On the light line in the prism-cells of some seed envelopes, by M. Junowicz.—The Salse of Sassuolo, the origin of aptychous lime, and the Mediterranean flora in its relation to the bottom deposits, by M. Fuchs.—The stand-aneroid barometer, by M. Schell.

July 19.—On the chemical reaction of the visual nerves and the retina, by M. Chodin.—The fossil flora of Parschlug in Steiermark, by M. v. Ettingshausen.—On the orbit of the Loreley (165), by M. Gruss.—The development of the embryo of *Asplenium Shepherdi*, Spr., by M. Vouk.—On idryl, by M. Goldschmidt.—On the behaviour of some resins and resinous acids in distillation over zinc powder, by M. Ciamician.—On derivatives of isatin, by M. v. Somaruga.—On cinchonin, by M. Skraup.—Action of water on haloid compounds of alcohol radicals, by M. Niederist.—Action of nitric acid on trimethyl carbinol, by M. Haitinger.—Action of weak affinities on aldehyde, by M. Lieben.—Researches on fluorescence, by M. Mach.—Tenacity and elasticity of vegetable textures and organs, by M. Wiesner.—Analysis of the sulphur springs at Baden, near Vienna, by M. Kretschy.—On the spectra of the chemical elements and their compounds, by M. Ciamician.—Contributions from chemical laboratory in Brunn.—Influence of temperature on galvanic conductivity of liquids, by MM. Exner and Goldschmiedt.—Behaviour of taurine in the system of birds, by M. Cech.—On peculiar products of mykotic keratitis with the reaction of amyloid, by M. Frisch.—On heat conductivity of cotton, wool, and silk, by M. Schuhmeister.—Anatomy of the optic thalami and neighbourhood, by M. Schnopfhagen.—On the laws of nerve-excitation, by M. Fleischl.

PARIS

Academy of Sciences, August 20.—M. Peligot in the chair.—The following papers were read:—Meridian observations of small planets at the Greenwich and Paris observatories, during the second three months of 1877, communicated by M. Leverrier.—Observations on a recent work of M. Hebert, relative to the exceptional winter of 1876-7, by M. Faye. The phenomena are attributed by M. Hebert to a succession of strokes of sirocco with descending whirling motion, which have communicated the heat and drought characteristic of them. Forty-one distinct gyrations were observed in December alone. The sirocco stroke, which caused the very mild dry weather in the beginning of the year, belonged to three great cyclones which came, like all the others, from the Atlantic.—Examination of documents relative to a scientific expedition to Peru in 1735 to 1743, by M. De la Gournerie. The documents contain what is perhaps the first reference to platina, also references to M. Bouguer's celebrated memoirs, on attraction of mountains, not known till ten years afterwards (1749).—On an example of reduction of Abelian integrals with elliptic functions, by Prof. Cayley.—Properties common to supply pipes, canals, and rivers, with uniform *régime* (continued), by M. Boileau. The influence of resistance of the walls on the decrease of velocity of the liquid sheets (starting with the principal liquid thread) is proportional to the square root of the intensity of this resistance.—The plague in 1877; third reappearance in Bagdad; two centres of origin in Persia; by Dr. Tholozan.—Results obtained by application of sulphide of carbon to vines attacked by phylloxera, by M. Allies.—Discovery of a new planet by Mr. Watson (telegram from Mr. Joseph Henry).—Discovery of two satellites of Mars by Mr. Hall, at Washington, by Mr. Henry.—On a stellar system in rapid proper motion, by M. Flammarion. The stars in question (which are considerably apart) are 7510 B.A.C. and 2810 Σ (the second is double). They move in the same direction and with nearly the same velocity, which exceeds much the ordinary average of proper motions. The direction of motion is nearly opposite to that of the sun's translation in space.—On the characters of flames charged with saline powder, by M. Gouy. The observations seem to show that there is at the base of the flame a very thin layer, where the temperature is much higher than in the flame itself.—Researches on the chromates, by M. Etard.—Cerebral anæmia and congestion produced mechanically in animals, by attitude or by a gyratory movement, by M. Salathé. Rabbits kept in a vertical position, with head up, showed, after some time, symptoms of syncope, also convulsions. Respiration and heart-beats finally ceased. Reversal of position quickly restored the animal. Centrifugal force (the animal being rotated on a board) gave much more rapid cerebral anæmia or congestion, according as the head or feet were towards the centre of the board's motion. While it took about ten minutes to produce death by cerebral anæmia thus, it generally took at least double to produce it by congestion.—On the coloration of the optic elements in the *Locusta viridissima*, by M. Chatin. There is a considerable similarity to the same parts in crustacea.—Phenomena which accompany metamorphosis in the *Libellula depressa*, by M. Jousset de Bellesme. It is by swallowing air and storing it in its alimentary canal that the Libellula obtains the force necessary to accomplish most of its transformations (displacement of the wing, &c.). The mechanism is probably general in this class of animals.—Observations on falling stars of the month of August, by M. Chapelas. The number observed is the smallest since 1837.—On the heat which may be liberated by movement of meteorites through the atmosphere, by M. Govi.—The upper Devonian limestones of the north of France, by M. Gosselet.—On the physiological balance and its applications, by M. Grandeau. This instrument is to represent by curves the gains or losses of weight of any matter (soil, plant, animal, &c.), placed in one of the scales. M. Gosselin submitted for inspection a new *densimeter*, consisting of a small wooden rule suspended by a wire connected to a non-central point in it. A certain weight is placed at the end of the longer arm, and a piece of the body to be examined is hung from the shorter arm so as to give horizontal equilibrium. Then this piece is immersed in water and the weight on the longer arm is displaced till equilibrium is restored. The weight then indicates the density by its position on the scale.

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THURSDAY, SEPTEMBER 6, 1877

NATURE AND CONSTITUTION OF MAN

A Philosophical Treatise on the Nature and Constitution of Man. By George Harris, LL.D., F.S.A. (London: George Bell and Sons, 1877.)

DR. HARRIS has long been known as one of those social scholars who combine with much modern learning a great deal of the learning of the ancients. He is essentially an antiquary in science, and he has obviously collected and brought under ready command a fine array of authorities of various schools of thought and of many centuries. In the two handsome volumes which now lie before us, Dr. Harris has collected a rich store of the historical work of which he is so fond, bearing on the history of man in relation to his life and his physical and mental constitution. The author tells us in the preface that the object of his work is "to afford a comprehensive and complete survey of the nature of man as regards his intelligent being; to exhibit the direct and immediate connexion of each department in his constitution, with its corresponding relation; and to demonstrate the uniform mechanism of the whole as one entire and consistent system."

In the first of these volumes we meet with a preliminary dissertation containing "certain collateral considerations and conclusions concerning the nature of man;" and touching on such details as "the origination and production of animated bodies," "the constitution of animated bodies," "man in relation to both substance and spirit," "essence of spiritual being and the nature of the soul," "operations of spiritual beings," and similar topics. A good way further on in this volume we reach the first "book," which treats on "the medial nature and constitution of man," and which, under the five heads of "sensation, emotion, appetite, passion, and affection," brings the volume to its close.

In the second volume we have first put before us, in "book the second" the subject of "the moral nature and constitution of man." This is discussed under three heads—"moral disposition and character," "the moral desires," "the conscience." Finally, in the same volume we have the third and last book treating on "the mental nature and constitution of man," and embracing under the different subdivisions "the intellectual faculties," "the faculty of understanding," "the faculty of reason," "the faculty of genius," "the memory," "the concurrent operation and reciprocal influence of the various medial, moral, and mental endowments and powers of the soul," and "mental discipline and cultivation."

We have given the outline of these volumes in the order in which they are set forth, because it affords the best account of the matter of the volumes. None but a steady reader will take the trouble to go through so many pages of two closely reasoning volumes on subjects abstruse and confessedly difficult, and we who have steadily gone through them may therefore venture to pilot others on the way.

As we lay down the volumes we find the difficulties of reviewing them very considerable. If we were dealing with the works of a less earnest man than George Harris we should have no difficulty in finding some faults in every page. If we did not understand, or did not think that we do understand what he means by all the toil he has expended on these two books, we might say that the toil was all labour lost, and that to the making of books there is no end. In a word there is scope in the volumes for the critic of all minds all intelligences and all sentiments. The scholar might question the history, the experimentalist the science, the *littérateur* the style, the metaphysician the metaphysic; and all, within common rules of criticism, might be severe and at the same time fair.

The truth as it seems to us is that Dr. Harris in the whole of his work has not really endeavoured to set forth any new and original idea of his own absolutely, while yet he has, at the same time, proceeded on an idea which is not destitute of originality. He has striven like a true antiquarian to focus in his own mind the learning of others old and new and best on the subjects upon which he is treating, and then he has tried to plant on his pages his own view as a compound of the complete study. The conception is erudite and laborious to a singular and almost painful labour. It is a work in character with the mental form of a man who has been engaged all his life in judicial pursuits (as by the way is the case with Dr. Harris), and while, therefore, it is free of all fancy, it gives no such indication of individual analysis as shall separate his idea of what he has read from his idea of any one person whom he has read. He tells us, in fact, that "during the progress of the work many hundreds of minds have been dissected by the author," but he does not tell us the further truth, because he is obviously unconscious of the fact, that he has tried to make one dissection out of the whole.

We have said that in the mode of constructing the chapters of these volumes there is an originality in dealing with the accumulated learning of previous authors. Another feature which is quite novel in literature is also introduced by the author. He has laid other living authors under contribution, and whether they agree with him or differ from him he has published their views as he received them, *totidem verbis.* In these cases he has submitted his text, in proof, to the writers whose views he solicited, and having obtained their opinions he has tacked them on to the text in notes. In this manner we have presented to us the views of a number of authorities on many of the most curious and important points in the natural histories of men and animals. Let us give one illustration.

In the chapter on the faculty of understanding in the second volume Dr. Harris discusses or rather considers the question whether inferior animals surpass the human animal in any particular faculty. He reasons that they do and adduces in proof of his opinion "the almost intuitive knowledge which certain animals seem to possess of the virtues of particular herbs, as also of other substances, earths and mineral waters, to which they resort, successfully, in cases of sickness or bodily injury. Their discernment in this respect," he adds, "is probably owing to the great acuteness and perfection of their sensorial organs, which also enable them to detect and avoid poisons."

So much for the opinion of the author himself on this nice point. But he is not content to let the reader rest on one opinion. He courts the views of nine other authorities who, he thinks, may throw some light on the subject. In this way we get opinions on one point of science rendered by men who are writing purely from their own knowledge without being aware that any one else is adding a word on the topic under consideration, viz., whether inferior animals have any special faculty which man has not. The result is very curious.

Darwin, one of the authorities consulted, doubts the opinion altogether. He knows of no facts making it probable that animals perceive any qualities that are not perceived by us. He does not believe that any animal knows what herbs are poisonous, except through experience during former generations, by which an inherited association or instinct has been acquired against any particular herb. Quatrefages admits the view of the author to a certain extent, and in some cases he believes it to be necessary to admit the intervention of instinct, but he is evidently very doubtful on the subject. Richardson doubts the assertion altogether that animals resort to earths or mineral waters as remedies. He also doubts whether animals avoid poisonous vegetables, except in instances where the substances they refuse are distinctly odorous. He adds that the evidence on the subject in favour of the animal over the man is very small when it is carefully analysed; and certainly in regard to the avoidance of poisons by the inferior animals, the faculty, "as he found by direct observation, is extremely limited." It extends only to the detection of odorous substances. Wallace believes that the statement as made by the author is "unfounded and erroneous." Lubbock doubts whether the word knowledge can be applied to animals, but agrees that their senses are "in some respects more acute; also, perhaps, very different from ours."

These are negative or opposing views to those expressed by Dr. Harris himself on the subject of the special faculties of the inferior animals. But he adds the opinions of other authors which go with his own on the point, and in some instances are more determinately expressed. The late Mr. Alfred Smee, Mr. Serjeant Cox, Dr. Carter Blake, and Mr. Wood are on the side of our author.

We have selected but one example of this incidental inflow of thought from other minds into Mr. Harris's pages. We could have found many more illustrations, some of which are of equal interest, and we have no doubt that in a future day, when all the writers are silent, as, alas! some already are, these footnotes will be quoted—as extracts from letters of past men are quoted now—as evidences of thought quite unpremeditated, but still as correct reflexes of the minds that gave them forth.

We turn to the chapter on "The Faculty of Genius," in the third book as a good illustrative type of the chapters generally. Dr. Harris here strives to fix a definition to the word genius. In accordance with his rule, he gives the definitions of many scholars and metaphysicians respecting genius, and thereupon he adds his own definition. "The faculty of genius," he says, "may be defined to be that power of the mind whereby it is able to produce results which cannot be attained by the common and ordinary faculties for receiving knowledge and reasoning upon it." Genius, therefore, produces results which no exercise of common or ordinary faculties, however energetically they may be employed, could produce—results which are quite beyond the sphere of such ordinary faculties, "and of a nature altogether different from anything produced by them." "Thus," he continues, "while by understanding and reason we receive ideas and compare them, by genius we are enabled to create them anew altogether, through the original combinations which we accomplish. While the former faculties only enable us to import and to select our wares, the latter enables us to make them ourselves."

From this definition the author proceeds to state that the faculty of genius, like that of understanding and reason, will be found to be constituted of certain independent capacities. These he classifies under different heads, viz., the capacity for wit; the capacity for taste; the capacity of organisation; and the illustration of the nature of these capacities, under the last of which heads there is appended a most interesting note by De Sainte Croix. Further on he writes under other heads, on the distinctive functions, operations and appliances of each of the different capacities of genius; the corresponding characters in the action of each of these capacities; on art as the especial province of the faculty of genius; on the extent and limit of the operations of genius.

Here, in summary, is the scope of this essay of our author on genius. From his point of view, that genius is a special faculty belonging to a particular class of men, it is an admirable treatise in itself. It ought to have been supplemented by a special chapter on genius in relation to families and races, without which chapter it may be considered, by some, to be diffuse, uncemented, and unsymmetrical; an edifice that may easily fall down and is not artistically laid out. For all that it is a commanding construction, wanting in genius but elaborate in labour. It is, in fact, a striking illustration of one of the author's own definitions. The best part of the essay is that in which the attempt is made to prove that the faculty of genius is especially connected with art. It will occur to all who think on this matter that there is in the idea a subject for careful consideration. If it should be true, the admission of its truth would lead to considerable modification in historical appreciations of work in science. It strikes us at once, as we glance back at the history of science, that the true men of genius in science have all been strongly imbued with artistic feeling and knowledge, and it strikes us also that some men who are known only as artists in literature or painting, or other true art, have made very singular and original contributions to science. But the theme is too fruitful of suggestion to be followed out here. We leave it for the study of those who have leisure as well as learning.

We replace Dr. Harris's volumes on our shelves in a convenient place for handy reference, and we commend others who have to think, write, and speak on the subjects submitted for study to do the same. They will often find the matter most useful as well as interesting, and although at times they may wish that the exposition had been less laboured, they will be grateful to an author who has spared neither time, nor labour, nor expense, to give them "the work of his life." We add, without hesitation, that Dr. Harris's work, though it be little read in this age of luxurious reading, will remain to be read as one of the solid and enduring additions to English learned literature.

SCIENCE IN THE ARGENTINE REPUBLIC

The Argentine Republic. Written in German by Richard Napp, assisted by several Fellow-writers, for the Central Argentine Commission on the Centenary Exhibition at Philadelphia. With several maps. (Buenos Ayres, 1876.)

Physikalische Beschreibung der argentinischen Republik, nach eigenen und den vorhandenen fremden Beobachtungen entworfen. Von Dr. Hermann Burmeister. Erster Band: die Geschichte der Entdeckung und die geographische Skizze des Landes enthaltend. (Buenos Aires, 1875.)

Acta de la Academia Nacional de Ciencias Exactas existente en la Universidad de Córdova. Tomo I. (Buenos Aires, 1875.)

Los Caballos Fósiles de la Pampa Argentina. Descriptos por Dr. Hermann Burmeister, Director del Museo Público de Buenos Aires. Obra ejecutada por órden del Superior Gobierno de la Provincia de Buenos Aires, para ser presentada en la Exposicion de Filadelfia. Con viii. Láminas Litografiadas. (Buenos Aires, 1875.)

ON several previous occasions [1] we have alluded to the excellent work in science accomplished by Prof. Burmeister, or under his supervision, since that eminent German naturalist took up his residence in the Argentine Republic. A batch of books, which has now, we regret to say, been lying before us for some time, testifies to his unabated activity in the good cause, and requires a few words of acknowledgment and explanation. The first of these, originally written in German by Richard Napp, but translated for the benefit of the Anglo-Americans, gives a general physical and commercial account of the Argentine Republic, prepared on the occasion of the Centenary Exhibition at Philadelphia. It is, of course, rather superficial, as is usually the case with such essays, but contains a good deal of information, and will be useful to the many Anglo-Saxons who are now settling in various parts of the country of which it treats.

Next we have the first volume of a complete physical description of the Argentine Republic, by Dr. Burmeister himself, which when finished will, as we understand from the introduction, contain a much more complete account of this extensive territory and its products than any work that has yet appeared. The present portion of it gives us the history of the discovery of the country and an account of its physical geography. Subsequent volumes will contain a general *résumé* of its natural history and geology. A French edition as well as a German will be issued, and a folio atlas will contain the necessary illustrations.

The third work on our list is the first volume of the "Acta" of the National Academy of Sciences of Cordova, which, as we have explained to our readers on former occasions, has been recently re-constituted under Dr. Burmeister's directorship. It contains contributions to science by some of the members of the new professoriate, the organisation of which has caused our excellent friend so much embarrassment. Dr. Stelzner and Dr. Brachebusch treat of various points in the geology and mineralogy of the Argentine Republic. Dr. D. C. Berg contributes an essay on the Lepidoptera of Patagonia, based upon collections made during an excursion to that country in 1874.

Lastly, we have an excellent memoir by Dr. Burmeister on the Fossil Horses of the Pampas formation written in Spanish and German, and prepared, as it appears, on the occasion of the International Exhibition of Philadelphia. Eight well-executed lithographic plates illustrate this important work, which is executed in the same style as other excellent essays of the indefatigable author that we have already noticed on former occasions. An Appendix contains a complete list of the mammals of the Quaternary Pampian formation, remains of which are contained in the Public Museum of Buenos Ayres. From this it appears that between fifty and sixty species are represented more or less perfectly in this unparalleled series, amongst which are many specimens that, in spite of the richness of our own palæontological collections, might well excite the envy of Prof. Owen and Mr. Waterhouse.

OUR BOOK SHELF

United States Commission of Fish and Fisheries. Part III.—Report of the Commission for 1873–74 *and* 1874–75. (Washington Government Printing-office, 1876.)

THIS volume is quite as interesting as any of those which have preceded it, whilst the amount of reliable information it brings to a focus, not only regarding the fish and fisheries of the United States, but of the fisheries of Great Britain, Sweden, Prussia, Holland, France, and Russia, is remarkable; nor are the historical observations on the condition of the fisheries among the ancient Greeks and Romans, and on their modes of salting and pickling fish less interesting. The volume is throughout so rich in statistics and details of piscicultural labour that we feel embarrassed as to what part of it to notice first; to give a detailed account of the contents is simply impossible in anything like the space we can afford. As readers of NATURE may be aware, the present volume is one of a series having for its object an exposition of the present state of the American fisheries, and in particulur showing the extent to which the seas and waters of the United States have been over-fished, and how far the systems of artificial fish culture at present in vogue provide a remedy for the reckless spoliation of the waters which has been going on for the last twenty years. Familiar as we are with the figures of fish-culture, so far as they are locally applicable to British enterprise, and whether in respect of oysters or salmon, any details we can give are utterly dwarfed by the fabulous-looking figures applicable to what has been achieved in America. The young salmon which have been thrown into the River Tay from the Stormontfield hatching-ponds since the beginning of the experiments in 1853 up to the present time, will not be equal to the operations of one season on the Upper Sacramento; in 1875 the salmon eggs collected numbered 11,000,000, making a bulk of eighty bushels, and weighing nearly ten tons! These eggs, so carefully packed that only a small percentage was wasted, have been largely distributed over America, and will doubtless ultimately add largely to the fish supply of the United States.

Another fish which has been subjected to the manipulations of the pisciculturists on a positively gigantic scale is the shad, *Alosa sapidissima*, and the fish locally known as the "alewife," *Pomolobus pseudo-harengus.* These fishes were at one time (forty years since) exceedingly abundant in the Potomac river, so much so that as many as 22,500,000 shad and 750,000,000 alewife were captured in six weeks' time. Only a small percentage of these numbers can now be taken. Enormous quantities of shad have been bred from the egg and sent into the waters in order to renew the supply; many millions of healthy young fish are thus annually added to the stock, and fresh rivers are now being populated with shad.

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[1] ATURE, vol. iii. p 282; vol. vii. p. 240; and vol. xii. p. 145.

Another fish to which the commission has of late directed its attention is the carp, *Cyprinus carpio*, and var. It is a fish which is thought to be eminently calculated for the warmer waters of America, and especially suited to the mill-ponds and sluggish rivers and ditches of the south. Some most interesting information is given about the carp and its numerous varieties, but it is too detailed for quotation. Another noteworthy feature of American fish-culture is the transport, in "an aquarium car," to the coast and inland waters of California, of various fishes and crustaceans of North America, in order to test the question of whether they could be acclimatised in the warmer latitudes of the Pacific. The first experiment failed through an accident on the railway, but on June 12, 1874, the car arrived safely with its interesting freight. Out of a lot of 150 lobsters which were placed in the aquarium car, only four, however, were left alive on reaching San Francisco Bay, and these were put into the sea at Oakland Wharf, nine days after they had been taken from the Atlantic Ocean. It is thought probable that the four specimens did not ultimately live, but as two of the four were big with spawn it is probable the eggs would come to maturity, as the death of the parent does not kill the spawn. Lobster eggs, unlike fish ova, are fructified before they leave the body of the animal.

An interesting account is given in the present volume of the American oyster fisheries.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Indian Rainfall and Sun-spots

SINCE my last communication to you on the above subject I have had some correspondence with Mr. Hill, the meteorological reporter to the North-West Provinces, in the course of which he has given me a more definite account of the results of his investigations. The outcome of our joint researches may be broadly stated as follows:—In years of maximum sun-spot the summer rainfall is *above* and the winter rainfall *below* the average, while in years of minimum sun-spot the reverse conditions hold, viz., the summer rainfall is *below* and the winter rainfall *above* the average.

Though the preceding hypothesis can only be considered at present in the light of a probability, it is considerably strengthened by the fact that, unknown to each other, Mr. Hill and myself have each taken it as the basis of our separate, and until recently, independent lines of inquiry. Mr. Hill has already sent in a short report to Government of the results of his own investigations, which he roughly states in the following words:—"I have examined the rainfall of Benares, Allahabad, Agra, Bareilly, Roorkee, Dehra, Mussoorie, and Naini Tal, since 1861, and I find that on the average the *total* annual rainfall of the maximum sun-spot years 1861, 1869, 1870, and 1871, is about 14 per cent. above the average for the whole period, and that of the minimum group 1866, 1867, and 1868, about 4 per cent. below it. On the other hand, when only the *winter* rainfall is considered, the defect in maximum sun-spot years is about 21 per cent. of the average winter fall, and the excess in minimum sun-spot years is above 20 per cent." Unfortunately no further data appear to be available in the North-West Provinces, as during the mutiny decade, 1851–1860, no register was kept, and before the mutiny the Schlagintweits took away to Germany most of the old rainfall records. The results, however, as far as they go, are in complete accordance with those I have obtained from a similar comparison of some of the rainfalls in Bengal.

The chief obstacles in the way of making a thoroughly complete and exhaustive comparison of the rainfalls throughout Northern India are (1) the paucity of registers, and (2) the limited periods over which they extend. In the single case of Calcutta the latter objection does not apply, since by dint of some trouble I have succeeded in obtaining the monthly as well as the annual falls for a period of forty years, from 1833 to 1876 inclusive. As this comprises four complete sun-spot cycles, the results are extremely valuable, especially as they tend to exhibit what Prof. Balfour Stewart considers to be the true test of a physical cycle, viz., its repetition.

I here append a comparison of the rainfall of Calcutta, for the months of January, February, and March, arranged in four groups of minimum and maximum sun-spot years, together with the years immediately preceding and following them (except in the case of the former, where the rise after the minimum is often very rapid).

Minimum sun-spot groups.		Maximum sun-spot groups.	
Years.	Total of each group.	Years.	Total of each group.
	inches.		inches.
1842 1843 1844	7·80	1836 1837 1838	3·54
1854 1855 1856	7·29	1847 1848 1849	6·68
1865 1866 1867	12·89	1859 1860 1861	6·42
1875 1876 1877	14·60	1869 1870 1871	9·76
Total of all the groups.	42·58	Total of all the groups.	26·40

In addition to this it may be noted that the total fall during the same three months of the four absolute minimum sun-spot years 1843, 1856, 1867, and 1877 (probable minimum) is 15·98 inches, while that of the four absolute maximum sun-spot years, 1837, 1848, 1860, and 1870, reaches only the insignificant amount of 2·48 inches. Similar results are obtained if the month of April and the months of November and December of the preceding years are respectively included, thus evidently showing that the relation is one connected with the seasonal distribution of the rainfall and not merely a coincidence, resulting from having taken the rainfalls of special months.

If we treat the rainfall of Dehra, from 1861 to 1877, in a somewhat similar manner, by taking the rainfalls of January, February, and the previous December, and consider 1861, 1862, and 1863 to approximately represent a maximum group, the results are as follows:—

Minimum sun-spot groups.		Maximum sun-spot groups.	
Years.	Total of each group.	Years.	Total of each group.
	inches.		inches.
1865 1866 1867	22·1	1861 1862 1863	2·6
1875 1876 1877	13·3	1869 1870 1871	10·5

I think an examination of the preceding comparisons of the winter rainfalls of Dehra and Calcutta (rough though they undoubtedly are) discloses distinct evidence of repetition in unison with the sunspot periods, at all events enough to warrant further and more complete investigations being made on the point. I will not trespass on your valuable space further than to add a similar rough indication of the inverse relation that holds in the case of the summer rainfalls. They will be seen to be greatest in years of maximum sunspot.

In the following groups the rainfall of Dehra is taken for the months of June, July, August, September, and October during which the south-west monsoon rains fall. I give the separate rainfall during these months for each year, as well as the totals and averages.

Maximum sun-spot groups.			Minimum sun-spot groups.		
Years.	Inches.	Totals.	Years.	Inches.	Totals.
1861 1862	91·4 90·5	181·9	1875 1876	67·2 76·1	143·3
Average for each year		90·9	Average for each year		71·6
1869 1870 1871	70·5 77·2 100·2	247·9	1865 1866 1867	58·9 67·0 60·6	186·5
Average for each year		82·6	Average for each year		62·1

The averages give a mean average excess in each year in the maximum sun-spot groups of 19·9 inches over each year in the minimum groups; a gigantic difference certainly, and apparently maintained pretty consistently throughout by the rainfall of each year. The defect in the winter rainfall of years of maximum sunspot and the corresponding excess in years of minimum sunspot together combine to render the excess in years of maximum sun-spot less apparent in the total annual falls, though it still exists to a certain extent. The present year (at present abnormally deficient in solar activity) appears destined to fulfil the preceding relations to an alarming extent. The winter rainfall was unusually plentiful throughout Northern India, while, up to the present time, when the monsoon should be in full swing, the rains, except in Eastern Bengal, have been so scanty that unless rain falls soon and abundantly, we shall have to face a famine as fearful as that which is just now devastating Madras.

E. D. ARCHIBALD

Greening of Oysters

IT has long been known that oysters, when removed from the sea and kept artificially in shallow pits filled with salt-water, assume a green colour which is or was much thought of by epicures. While this spring at Le Croisic, at the mouth of the Loire, my friend, Dr. Bornet, informed Prof. Lankester and myself that this singular change was particularly observable in the oyster preserves in the neighbourhood. He was at that time at a loss for an explanation, but I have just received a letter from him in which he gives the solution of the problem, and this will I think be so interesting to many of the readers of NATURE, that I have ventured, although without his permission, to communicate it to them.

"As a souvenir of our meeting at Croisic, I send you some *Corallinaceæ* from that locality. Several have not yet been detected on the English coast, where, however, they ought to occur. I have added a Diatom, *Navicula fusiformis*, Grunow, var. *ostrearia*. This species, whose contents are of a cobalt-blue colour during life, occurs in profusion in the oyster-preserves of Croisic, and it is because they feed on this Diatom that they become green. Nothing is easier than to demonstrate the fact by placing white oysters in a plate of sea-water containing nothing but *Navicula fusiformis*, and the "greening" takes place in thirty-six hours. As often as the experiment is repeated, the same result follows. But why should *Navicula fusiformis* be blue, while all other Diatoms are colourless?"

W. T. THISELTON DYER

P.S.—Since this note has been in type Prof. Oliver has called my attention to a paper in the "Mémoires de la Société Linnéene du Calvados," 1824, pp. 135-158, by Benjamin Gaillon, "Sur la cause de la coloration des Huîtres et sur les Animalcules qui servent à leur nutrition." These animalcules form masses which he compares to the green matter of Priestley, and as he refers them to the genus *Navicula* of Bory, they are no doubt identical with those which Dr. Bornet has studied. Gaillon refers to an earlier memoir of his own on the same subject published by the Academy of Sciences of Rouen, and in the *Annales des Sciences Physiques*, for 1821.

W. T. T. D.

Reproduction by Conjugation

THE phenomenon to which Mr. Bennett alludes is, I presume, well known; but it is not universal, though common. He will find illustrations in Hassall's "British Freshwater Algæ," where the zygospores are formed in both filaments simultaneously; *e.g.*, plate 19, *Zygnema* (Spirogyra); plate 38, several species of *Tyndaridea*. But in those genera in which the Zygospore is formed *between* the filaments it would seem impossible to decide which is male and which is female, *e.g.*, plate 39, *Tyndaridea conspicua*, *immersa*, *Ralfsii*, and *decussata*; or in the genera *Mesocarpus*, plates 42–47, and *Stenocarpus*, plates 47–49

GEORGE HENSLOW

Strange Dream Phenomenon

AFTER reading the interesting letter on a "Strange Dream Phenomenon" which appeared in NATURE (vol. xvi. p. 329) it occurred to me that it might be worth while to put on record the following experience which connects in a very striking manner the phenomena of dreaming and subjective vision. Some time ago, when rather tired by overwork, I dreamt during the night that some one had entered my bedroom and was approaching the pillow under my head with the intention of abstracting some valuable papers which I fancied were concealed beneath it. I noticed in every particular the dress, stature, and features of the intending robber, but just as he put forward his hand towards the bed I began to awake, slowly at first, but with great celerity as soon as I perceived the figure of my dream walking slowly down the side of the bed; wide awake now, I watched it reach the corner bedpost, turn round, and with measured noiseless step pass along the foot, till on coming between the window and myself it disappeared, as all the "ghosts" with which I was then afflicted were wont to do when shone through by the light.

I did not sleep any more for the rest of the night, and hence am perfectly certain that this was not "a dream within a dream," but a clear case of a subjective vision prolonged from the sleeping into the waking state, and thus affording evidence to prove the essential identity which underlies the phenomena of "dreaming dreams" and seeing "ghosts."

W. J. S.

OUR ASTRONOMICAL COLUMN

THE SATELLITES OF MARS.—The Paris correspondent of the *Times*, writing on August 27, states that at the meeting of the Academy of Sciences the same day, M. Faye had announced the independent discovery of the satellites of Mars by M. Borrelly at Marseilles, claiming for him even an earlier detection than was effected at Washington. Prof. Watson's name being introduced as the American discoverer instead of that of Prof. Asaph Hall, it is clear that the statement has arisen from a misconception on the part of the reporter at the sitting of the Academy, who has confounded the discovery of No. 174 of the minor-planet group, by Watson and Borrelly, with that of the satellites of Mars.

A letter from Rear-Admiral Rodgers, Superintendent of the Naval Observatory, Washington, to the Secretary of the Navy, dated August 21, furnishes particulars of the observations and calculations bearing upon both satellites, which had been made up to that date. The outer satellite was remarked on the 11th, but its true character was not certainly recognised until the 16th. On the following night Prof. Hall first observed the inner satellite. The discoveries were telegraphed to Messrs. Alvan Clark and Sons, at Cambridgeport, on the 18th, that confirmatory evidence of the existence of the satellites might be obtained by means of the 26-inch telescope of Mr. McCormick, at present in the hands of those eminent opticians, who succeeded in verifying Prof. Hall's discovery, as did also Prof. Pickering and his assistants at Cambridge, Mass. On the 19th the discovery was communicated to the Smithsonian Institution, and by it, to the European observatories.

We subjoin the principal results of Prof. Newcomb's calculations on the first ten days' measures, extracted from a circular for which we are indebted to the superintendent of the U.S. Naval Observatory.

For the outer satellite Prof. Newcomb finds:—

Major axis of apparent orbit at distance [9·5930]	82″·5
Minor axis ,, ,, ...	27″·7
Major axis of orbit at distance unity...	32″·3
Position angles of line of apsides of apparent orbit...	70°...250°
Passage through west apsis ($p=250°$)...	Aug. 19 at 16h·6 W.M.T.
Inclination of true orbit to the ecliptic	25°·4
Longitude of ascending node	82°·8
Period of revolution	30h. 14m.
Hourly motion in areocentric longitude	11°·91
These elements give for the mass of Mars	$\frac{1}{3,090,000}$

For the inner satellite Prof. Newcomb finds—

Major axis of apparent orbit at distance [9·5930]	33″·0
Passage through eastern apsis ($p=70°$)...	Aug. 20 at 13h·0 W.M.T.
Period of revolution	7h. 38m·5
Hourly motion in areocentric longitude	47°·11

If we refer the position of the orbit of the first satellite to the earth's equator, the ascending node will be found to be in 46°·4, and the inclination 36°·2. The real distances of the satellites from the centre of Mars are about 14,500 and 5,800 miles respectively.

Prof. Newcomb, writing to the *New York Tribune* on August 22, gives some particulars relating to the new satellites. He remarks that the first question which will naturally arise is, Why have these objects not been seen before? The answer is that Mars is now nearer to the earth than he has been at any time since 1845, when the great telescopes of the present day had hardly begun to be known. In 1862 when Mars was again pretty near to the earth, we may suppose that they were not looked for with the two or three telescopes which alone would have shown them. In 1875 Mars was too far south of the equator to be advantageously observed in these latitudes. The present opportunity of observing the planet is about the best that could possibly occur. At the next opposition in October, 1879, there is hope that the satellites may be again observed with the great telescope at Washington, but Prof. Newcomb thinks that during the following ten years, when owing to the great eccentricity of the orbit of Mars, he will be much further from the earth at opposition, the satellites may be invisible with all the telescopes of the world. In the present year it is hardly likely that they will be visible after October. The satellites may be considered as by far the smallest heavenly bodies yet known. "It is hardly possible to make anything like a numerical estimate of their diameters, because they are seen in the telescope only as faint points of light. But one might safely agree to ride round one of them in a railway car between two successive meals, or to walk round in easy stages during a very brief vacation. In fact, supposing the surface of the outer one to have the same reflecting power with that of Mars, its diameter cannot be much more than ten miles, and may be less. Altogether these objects must be regarded as among the most remarkable of the solar system." Prof. Newcomb further points out that we may regard M. Leverrier's mass of Mars as the product of a century of observation, and several years of laborious calculation by a corps of computers, while from the measures on four nights of the new satellites, ten minutes' computation gives a mass which is in striking agreement with that of the illustrious astronomer of Paris.

THE D'ANGOS COMET OF 1784.—In continuation of our remarks upon this object, so long considered apocryphal (NATURE, vol. xvi. p. 124), we proceed to notice an investigation by D'Arrest, undertaken in 1865. He refers to a somewhat confused note in Cooper's "Catalogue of Cometic Orbits," where the orbit given by D'Angos is introduced "with no slight misgivings," though the writer thought his note was "considerably opposed to Encke's statement," on what precise grounds, however, is not very apparent. And D'Arrest considered the essay in Zach's work may have received undue colouring from the editor, and a further proof of the non-existence of the comet is required.

Deducing the sun's places from the Tables of Hansen and Olufsen, D'Arrest compares the fourteen days' observations given by D'Angos in Bernouilli's and Hindenburg's Magazine, with the best of two orbits calculated by Olbers from them, and finds what he terms "horrenden Abweichungen." A similar comparison by means of elements selected from numerous sets computed by himself, exhibits also great differences, so that his first conclusion is identical with that arrived at by Olbers and Encke, the comet's places are irreconcilable with parabolic motion about the sun. On applying the general method of the *Theoria motus* he was led to extraordinarily small distances from the earth, in perfect agreement with the results obtained by Encke on a different method of procedure.

D'Arrest then introduces the consideration of the *geocentric hypothesis*. We are certain of cases where the orbits of comets have undergone an entire change through the action of the planet Jupiter, and there is now, he urges, no difficulty in accepting the idea of an insignificant and small vaporous mass, passing with a retrograde motion in such close proximity to the earth, that the case treated of in the "Mécanique Céleste," book ix. chap. ii., may enter. The question is a complicated one, and D'Arrest contents himself with examining whether in the series of observations of the comet, there is anything to contradict the hypothesis that for a time it was moving as a satellite of the earth. Necessarily omitting parallax, he finds on this geocentric assumption the following elements:—Time of passage by the perigee, April 15·13649 mean time at Paris; ascending node 90° 52′ 9″; inclination 43° 8′ 23″; distance of perigee from ascending node 128° 7′ 59″; eccentricity 0·97320, and least distance from the earth in units of the semi-axis major of the moon's orbit 3·82290; motion, retrograde. When now the comet's distances from the earth are found from the observed angular motions, and again direct from the elliptical elements, a comparison shows that the angular motion with respect to the earth corresponds well with the law of areas, and the latitudes from this approximate calculation are sensibly better represented, than on the supposition of heliocentric motion.

The general conclusions arrived at by D'Arrest from his investigation appear to be these: that the impossibility of the observations published by D'Angos and the obvious imposition previously attributed to him are not in accordance with evidence. The comet may have been moving for a time virtually as a satellite of the earth, or while passing very near to it may yet have been without the sphere of attraction, the definition of which is perhaps somewhat arbitrary. The difficulty and uncertainty attending further research, and the expense of time and labour necessary, induced D'Arrest to rest satisfied with having thrown a more satisfactory light on the observations of the second comet of 1784, or at least of proving that a small body moving under certain conditions might have occupied the positions in the heavens attributed to it. We have yet to give some account of a paper by Gauss on this subject.

THE FIRST COMET OF 1877 (BORRELLY, FEBRUARY 8)—The elements of this comet, so far as yet published, appear to depend upon an interval of observation of eight days only. The following orbit by Mr. Hind is founded on positions between February 8 and March 11 :—

Perihelion Passage, 1877, January 19·18369, G.M.T.

	°	′	″	
Longitude of Perihelion	200	4	18·0	M. Eq.
,, Ascending Node ...	187	15	7·0	1877·0
Inclination to Ecliptic...	27	5	24·1	
Log. Perihelion Distance	9·9071303			

Motion—retrograde.

There seems to be no sensible deviation from the parabola.

BIOLOGICAL NOTES

NEW WORK ON BIRDS.—We have received the prospectus of a new work by Dr. A. B. Mayer, Director of the Royal Zoological Museum of Dresden, to be entitled "Abbildungen von Vogel-Skeletten," in which he signifies his intention to publish, in parts, figures of the skeletons of rare or little-known birds. Each part is to contain ten plates of large quarto size, one of which, representing the skeleton of the extremely uncommon parrot from New Guinea, *Dasyptilus pecqueti* (Lesson) accompanies the prospectus. It is a photo-lithograph, and differs materially from any other which we have seen in one important particular, namely that the bones of one side only are depicted, which is a great advantage, as it prevents the confusion unavoidably associated with the representation of the whole structure The illumination of each bone and the focus of every part is most satisfactory, more so in many respects than any drawing could possibly be. Short commentaries, with measurements, will accompany each plate. It is proposed that Part I. shall contain figures of *Loriculus [illegible]*, *Charmosyna josephinæ*, *Meropogon forsteni*, *Paradisea papuana*, *Cicinurus regius*, *Manucodia chalybea*, *Ptilopus speciosus*, *Otidiphaps nobilis*, and *Gallus bachiva* (from Celebes). In the series is also to be included the skeletons of the several domestic pigeons and fowls. We hope that Dr. Mayer will have a large subscription to this valuable addition to ornithological literature.

THE BODY-CAVITY IN THE HEAD OF VERTEBRATES.—It has hitherto been regarded as a point of distinction between the mouth, throat, and gill region of vertebrates, and the rest of the trunk, that in the former no splitting of the body wall took place in early development, while in the trunk the body-wall becomes sharply separated from the contained viscera, and a cavity arises between them, part of which is the peritoneal cavity. Mr. Balfour (*Jour. Anat.*, April, 1877) has announced the discovery in sharks of a head-cavity on either side of the throat, dividing the growing tissue into an inner and an outer wall. When the visceral clefts (future gill-slits, &c.) appear, they subdivide these cavities into smaller ones. The head-cavity even grows forwards as far as the eye, and ultimately there is a series of cavities : (1) a premandibular, (2) a mandibular, (3) a hyoid, (4) a series in the branchial arches. These cavities ultimately atrophy, but their walls become developed into muscles, and they answer to the muscle-plates of the rest of the body. Thus this discovery gives information of a most valuable kind as to the segmental relations of the head to the rest of the body, besides furnishing a glimpse of a primordial condition in vertebrates which had till now remained unknown.

FISH-EATING BIRDS.—Mr. Joseph Willcox has recorded an interesting observation on the crow blackbirds of Florida (*Quiscalus purpureus*). Standing on the bank of a river in Florida, he noticed a commotion among a congregation of crow blackbirds, which were anxiously looking into the water. A large bass was pursuing its favourite food, the small fry, and the latter, in their frantic efforts to escape, jumped out of the water, and many of them fell on the land. The blackbirds, evidently experts at the game, immediately pounced upon the small fish, and swallowed them before they could get back into the water. (*Proc.* Acad. Nat. Sci., Philadelphia, 1877.)

ANTS' DOMESTIC ANIMALS.—Prof. Leidy (Philadelphia) has observed colonies of *F. flava* in possession of several kinds of insects at once. A comparatively small assemblage of them had three groups, an aphis, a coccus, and the larva of an insect, apparently coleopterous. The aphides were kept in two separate herds, and these were separated from a herd of cocci. In a larger colony of ants there was a collection of aphides occupying the under part of one margin of a stone, for ten inches long by three-quarters of an inch wide. A distinct group of cocci, closely crowded, filled a square inch. They all appeared to be carefully attended to by the ants.

A WHALE IN THE MEDITERRANEAN.—M. P. J. Van Beneden has made a short communication to the Académie Royale de Belgique, published in that Society's *Bulletin*, with reference to a letter by M. Capellini, on a true whale captured in the Mediterranean Sea, near Taranto. The Italian author suggests the new specific name *Balæna tarantina*, but M. Van Beneden much more reasonably thinks it most probable that it is a stray specimen of *B. biscayensis*.

THE LATE MR. GASSIOT

WE last week announced briefly the death of Dr. J P. Gassiot, and now give some account of the principal scientific results obtained by him. Mr. Gassiot, partner in the firm of Martinez Gassiot and Co., wine merchants, Mark Lane, first devoted his spare time to electrical experiments about the year 1838. An Electrical Society was formed about that time in which he took an active part. At one of the meetings it was observed that when the two copper wires forming the poles of a powerful voltaic battery were crossed and drawn asunder so that the voltaic arc passed between them, the positive terminal became heated to incandescence, while the negative remained comparatively cool. This excited great interest in Mr. Gassiot's mind and led him to make several experiments, but without thoroughly explaining the phenomenon. In the course of these experiments he procured powerful batteries, first of Daniell's construction, then of Grove's, and ultimately a large water battery.

It had been observed by many writers (principally Continental) that while the dynamic and chemical effects of the voltaic battery increased in intensity in proportion to the increasing chemical action in the cells of the battery, the static effects, such as the repulsive action in a gold leaf or pith ball electroscope, the spark, the power of charging a Leyden phial, &c., were more intense when the battery was charged with water and had consequently but a feeble chemical action in the cells. This anomaly puzzled electricians much, and though sought to be explained by various hypotheses, was a great stumbling-block in the way of the chemical theory of the voltaic battery.

Mr. Gassiot had been led to attach great value to good insulation between the cells of the battery, and he procured to be made a Grove battery (the most powerful chemical battery known) of 100 glass cells, all having long glass stems, and separated from each other. This battery gave very powerful chemical results, and a voltaic arc of great brilliancy ; but, what was of the greatest importance, he found that with this battery the static effects, or effects of tension, were greater than those of an equally-sized water battery. The puzzling anomaly was thus explained : the reason why the chemical battery had seemed inferior in tension to the water battery was that from the effervescing liquids, the close approximation of the cells, and their being moistened with good conducting liquids, insulation was destroyed, and no static effects at the terminal, or very feeble ones, were perceptible. This result, by far the most important of Mr. Gassiot's labours, was published in the *Phil. Trans.* of the Royal Society for 1844, p. 39. It got rid of the strongest objection to the chemical theory of the pile, and brought into harmony results which up to that time had appeared discordant.

In 1852 Mr. Grove had published in the *Phil. Trans.* of the Royal Society, in a paper "On the Electro-Chemical Polarity of Gases," an account of transverse dark bands or striæ, which he was the first to observe in the electric discharge. The discharges were obtained from a Ruhmkorf coil, and made to pass through attenuated gases, or what were commonly called *vacua*. Mr. Gassiot made a vast number of experiments on these striæ, the most important of which was that he obtained them in a Torricellian vacuum with the voltaic arc, showing that they did not depend on the intermittence of the discharge (occasioned by the contact-breaker), but accompanied all electric discharges *in vacuo*. There is, perhaps, some doubt whether the voltaic arc is absolutely continuous, or whether it does not produce, by its action on attenuated gas, something like waves (a stone thrown into water may be a rough simile), but at all events it is continuous in its inception, and in that respect quite different from the interrupted discharges of the contact-breaker apparatus, or the common electrical machine.

Mr. Gassiot devoted himself for a long time to procuring *vacua* as perfect as they could be obtained, for the examination of the electric or voltaic discharges, and proved distinctly that when the attenuation was pushed to a high degree of rarity, the electric discharge would not pass at all, a result which had been observed by Morgan (*Phil. Trans.*, vol. lxxv.), the accuracy of whose experiments was impugned by Davy.

Mr. Grove, as an answer to the contact theory of the voltaic pile, had shown that if two polished plates, one of zinc, the other of copper, were approximated, but kept from contact by a thin film of paper or mica, and then separated, the electric effects, alleged to be due to the contact of dissimilar metals, were produced; it was objected to this experiment, and not without reason, that these effects might be produced by friction of the paper or mica. Mr. Gassiot effectually got rid of this objection by bringing the plates into close proximity by a delicate micrometer apparatus and then quickly separating them; the same electrical results followed (*Phil. Mag.*, October, 1844).

The above are the principal of many curious results obtained by Mr. Gassiot. While thus giving up his leisure time to science, he was a diligent and successful man of business and a liberal promoter of, and contributor to, all useful scientific and benevolent objects, some of which we mentioned in our previous notice.

THE SPECTRUM OF NOVA CYGNI

IN the *Monatsbericht* of the Royal Academy of Sciences of Berlin (May, 1877), Herr Vogel, the eminent astronomer, publishes the details of his investigations of the spectrum of the new star in Cygnus, and whilst expressing his own views with regard to the physical condition of the star, enters upon a criticism of those of other observers. Herr Vogel observed the spectrum on sixteen different nights; the first observation was made on December 5, when the star was of 4·5 magnitude; the last on March 10, when the magnitude was only 8·3.

Herr Vogel's observations show that the spectrum of the new star was a continuous one, showing numerous dark lines and bands and several bright lines. The intensity of this continuous spectrum, which at first was very brilliant, decreased rapidly, so that three months after the discovery of the star it was only partly visible, and even that part was very faint. The decrease of intensity did not spread evenly over the whole spectrum; the blue and violet rays grew fainter far more rapidly than the green and yellow rays. The red part of the spectrum, which already during the first observations was very dim and crossed by broad absorption bands, soon disappeared altogether, so that a bright line in the red seemed to remain quite isolated. At first a dark band in the green, and, later on, a very broad dark band in the blue, were particularly conspicuous. With the exception of a bright line in the red, the other bright lines at first surpassed the continuous spectrum but very little in brilliancy; they could therefore be seen only with difficulty. During the rather rapid decrease of intensity of the continuous spectrum they, however, became more easily discernible, and, as results from the measurements made, the hydrogen lines Hα and Hβ were particularly bright, and, later on, a line of 499 mill. mm. wave-length. This latter line remained longest when the spectrum faded away, and finally surpassed the hydrogen lines in intensity; the red hydrogen line was the first to grow fainter. The weather not having been very favourable, the measurements which Herr Vogel made have no claim to very great accuracy, but they at least prove that the following *bright* lines have appeared in the spectrum:—

1. The hydrogen lines $\left.\begin{matrix}\mathrm{H}\alpha\\ \mathrm{H}\beta\end{matrix}\right\}$ beyond doubt.
   Hγ most probably.
2. A line of 499 mill. mm. wave-length (± 1 mill. mm.). This line coincides tolerably well with the brightest line in the nitrogen spectrum under ordinary pressure; it is the same line which is brightest in the spectra of nebulæ.
3. An indistinct line of 580 m. mm. wave-length.
4. An indistinct line of 497 m. mm. wave-length. This nearly coincides with a close group of lines in the atmospheric spectrum.
5. Some bright lines were seen in the neighbourhood of *b* and *E*, but their position could not be measured. On December 5 two lines were measured in the blue (of 474 and 470 wave-lengths), and were also observed on December 8, but, later on, only the second one has again been seen as an indistinct band of 467 wave-length.

In the accompanying illustration (Fig. 2) we reproduce Herr Vogel's drawings, which supplement his observations, and, as he points out, contain many a detail which could not well be described in words.

Herr Vogel, in discussing the views of other astronomers, first deals with M. Cornu's observations. M. Cornu made his first observations on December 2 and 5 (see NATURE, vol. xv. p. 158); he succeeded in measuring several bright lines in the spectrum, viz., wave-lengths, 661 (H α), 588, 531, 517, 500, 483 (H β), 451, 435 (H γ). He saw no dark bands distinctly in the continuous

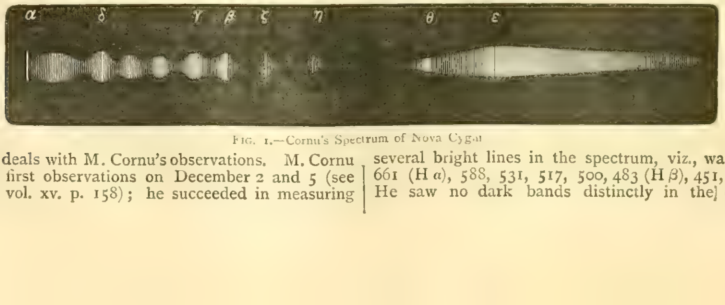

FIG. 1.—Cornu's Spectrum of Nova Cygni

spectrum, "because," says Herr Vogel, "he doubtless observed with a spectroscope of too great a power of dispersion, and therefore many details escaped his notice. This supposition is confirmed by the drawing, published in the *Comptes Rendus* (t. 83, p. 1,172), where M. Cornu represents the spectrum as consisting of two parts, and which contains no other details besides the bright lines.'

As the line 588 wave-length, measured in the star spectrum, corresponds closely with D_3, also the line 531 wave-length, with the well-known corona line (531,6 wave-lengths) and finally the line 517 wave-length, with the middle of the magnesium lines *b*, M. Cornu draws the conclusion that with regard to chemical composition the atmosphere of the star coincides completely with the chromosphere of our sun: "en résumé, la lumière de l'étoile parait posséder exactement la même composition que celle de l'enveloppe du soleil, nommée chromosphère." Herr Vogel, however, thinks that this conclusion is not altogether justified, since a line (500 wave-length), which does *not* occur in the chromosphere was distinctly visible with the other bright lines in the spectrum, and it eventually became the *brightest* line of the whole spectrum. Comparing his own observations with those of M. Cornu, Herr Vogel points out that they agree perfectly with regard to the presence of the three hydrogen lines, and that of the brightest line of the atmospheric spectrum, or the principal line of the nebulæ spectrum (500 wave-length). He could not determine the position of the bright green lines with sufficient exactness; on one day he found for them 527 and 514 wave-lengths respectively, but these figures differ considerably from M. Cornu's; this is still more the case with the lines in the blue, for which he finds 466 wave-length, while M. Cornu has 451 wave-length. The line 588 wave-length of M. Cornu was observed by Herr Vogel on one occasion, but was not again seen afterwards.

Father Secchi, in a short note in the *Astronomische Nachrichten* (No. 2,116), says that M. Cornu's description of the spectrum of the new star is correct, with the exception that the bright lines were not indistinct but well defined, like lines in spectra of nebulæ. Father Secchi observed on January 7 and 8, when Herr Vogel also saw the lines well defined. He is very positive that one of the bright lines is a hydrogen line, another a magnesium line, and a third a sodium line; this, in Herr Vogel's opinion, is decidedly a mistake, because on January 8 the lines in the vicinity of the magnesium group were quite faint, and no bright line near *D* was to be seen. According to Herr Vogel, the bright lines which Father Secchi saw were a considerable distance away from the sodium and magnesium groups respectively, their wave-lengths being 500 and 580.

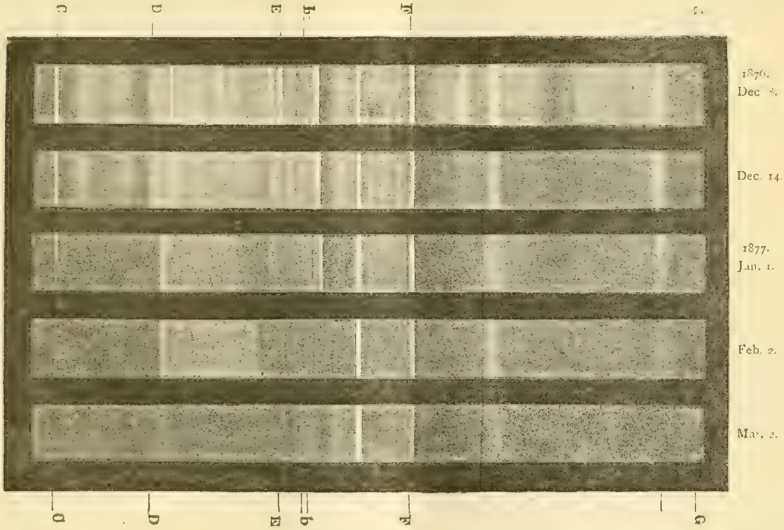

FIG. 2.—Vogel's Spectrum of Nova Cgyni.

Mr. Copeland, who worked with a star spectroscope of Herr Vogel's construction, which was connected with the 15-inch refractor of Lord Lindsay's observatory, made the first observation on January 2, when the star was of 7th magnitude. He found the spectrum to be remarkably bright and consisting of a faint, continuous spectrum, crossed by five bright lines, of which he determined the following wave-lengths:—(1) 655, intense bright red; (2) 581, middle of a rather bright band in the yellow, fading off rapidly on both sides; (3) 504, and (4) 486, bright, well-defined lines; (5) 456, faint line in the violet. Nos. 1 and 4 are the hydrogen lines; No. 3 is the brightest line of the nebulæ spectrum. On January 9, when the atmosphere was particularly favourable, Mr. Copeland observed two other lines, of 594 and 414 wave-lengths respectively. The first was a "very narrow line," the second "excessively faint, but still certainly and repeatedly seen." In the vicinity of about 525 wave-length, Mr. Copeland observed the maximum of intensity of the continuous spectrum. Mr. Copeland's observations agree very well with those of Herr Vogel; the only exception being the violet line (456) for which Herr Vogel found a greater wave-length. Line 414, observed by M. Copeland, may possibly have been the fourth hydrogen line Hδ, if 414 is not a mistake of the printer and should be 434, as it would be strange if Mr. Copeland had not seen the third hydrogen line H γ (434) which was distinctly visible. Mr. Copeland pointed out that the line of 580 wave-length corresponds closely with a line which Herr Vogel observed in the spectra of three faint stars, also belonging to Cygnus, which have quite abnormal spectra, and Herr Vogel owns that, however small the similarity between these spectra and that of the new star may have been at first, a certain resemblance appeared when the latter grew fainter; not only the line in question coincided with one in these spectra, a coincidence was also evident in a maximum of brightness in the blue (467 wave-length) and in a dark broad band close to this maximum.

Mr. Backhouse, of Sunderland, observed the spectrum on January 26, and found the brightest line to be of 503 wave-length. He remarked in a note to NATURE (vol. xv. p. 295), that at the end of December, not this line but line F was brightest; Herr Vogel's observations quite agree with those of Mr. Backhouse.

In summing up and in his final remarks on this subject, Herr Vogel first of all declares that he cannot agree with M. Cornu's view as expressed by this observer in the following phrase:—"Malgré tout ce qu'il y aurait de séduisant et de grandiose à tirer de ce fait des inductions relatives à l'état physique de cette étoile nouvelle, à sa température, aux réactions chimiques dont elle peut être le siège, je m'abstiendra de tout commentaire et de toute hypothèse à ce sujet. Je crois que nous manquons des données necessaires pour arriver à une conclusion utile, ou tout au moins susceptible de controle; quelque attrayantes que soient ces hypothèses, il ne faut pas oublier qu'elles sont en dehors de la science, et que loin de la servir, elles risquent fort de l'entraver."

Herr Vogel thinks that the fear that a hypothesis might do harm to science is only justifiable in very rare cases; in most cases it will further science, in the first place because it draws the attention of the observer upon things, which, without the hypothesis, he might have neglected. Of course, if the observer is so strongly influenced, that in favour of a hypothesis he sees things which do not exist—and this may happen sometimes—science may for a while be arrested in its progress, but in that case the observer is far more to blame than the author of the hypothesis. On the other hand it is very possible that an observer may—involuntarily—arrest the progress of science, even without originating a hypothesis, by pronouncing and publishing sentences which have a tendency to diminish the general interest in a question, and which do not place its high significance in the proper light. Herr Vogel is almost inclined to think that such an effect might result from the reading of the above phrase by M. Cornu, and is of opinion that nowhere better than in the present case, where in short periods colossal changes showed themselves occurring upon a heavenly body, the necessary data might be obtained for drawing useful conclusions, and the test of those hypotheses which have been ventured with regard to the physical condition of heavenly bodies, might be made.

A stellar spectrum with *bright* lines is always a highly interesting phenomenon for any one acquainted with stellar spectrum-analysis, and is well worthy of deep consideration. Although in the chromosphere of our sun, near the limb, we see numerous bright lines, yet only dark lines appear in the spectrum whenever we produce a small star-like image of the sun and examine it through the spectroscope. It is generally believed that the bright lines in some few star-spectra result from gases which break forth from the interior of the luminous body, and the temperature of which is higher than that of the surface of the body, *i.e.*, the same phenomenon we observe sometimes in the spectra of solar spots, where incandescent hydrogen, rushing out of the hot interior, becomes visible above the colder spots through the hydrogen lines turning bright. But this is not the *only* explanation. We may also suppose that the atmosphere of a star, consisting of incandescent gases, as is the case in our sun, is on the whole colder than the nucleus, but with regard to the latter is extremely large. Herr Vogel cannot well imagine how the phenomenon can last for any long period if the first hypothesis be correct. The gas breaking forth from the hot interior of the body will impart a portion of its heat to the surface of the body and thus raise the temperature of the latter; consequently the difference of temperature between the incandescent gas and the surface of the body will soon be insufficient to produce bright lines, and these will disappear from the spectrum. This view applies perfectly to stars which suddenly appear and soon disappear again, or at least decrease considerably in intensity, *i.e.*, for so-called *new* stars, in the spectra of which *bright* lines are apparent, if the hypothesis mentioned below is admitted for their explanation. For a more stable state of things the second hypothesis seems to Herr Vogel to be far more adapted; he thinks, therefore, that stars like β Lyræ, γ Cassiopeiæ, and others, which show the hydrogen lines and line D_3 *bright* on a continuous spectrum, with only small oscillations of intensity, possess very large atmospheres in proportion, consisting of hydrogen and the unknown element which produces the line D_3. With regard to the new star Herr Vogel points to a hypothesis which Herr Zoellner deduced from Tycho's observations of the star named after him, long before the considerable progress had been made in stellar physics by means of spectrum analysis. Zoellner supposes that upon the surface of a star, through the constant exhalation of heat, the products of cooling, which in the case of our sun we call sun-spots, accumulate in such a way that finally the whole surface of the body is covered with a colder stratum which gives much less light or none at all. Through a sudden and violent tearing up of this stratum the interior incandescent materials, which it incloses, must naturally break forth, and must, in consequence, according to the extent of their eruption, cause larger or smaller patches of the dark envelope of the body to become luminous again. To a distant observer such an eruption from the hot and still incandescent interior of a heavenly body must appear as the sudden flashing up of a *new* star. That this evolution of light may under certain conditions be an extremely powerful one, could be explained by the circumstance that all the chemical compounds which under the influence of a lower temperature had already formed upon the surface, are again decomposed through the sudden eruption of these hot materials, and that this decomposition, as in the case of terrestrial substances, takes place under evolution of light and heat. Thus the bright flashing up is not only ascribed to the parts of the surface which through the eruption of the incandescent matter have again become luminous, but also to a simultaneous *process of combustion*, which is initiated through the colder compounds coming into contact with the incandescent matter.

Zoellner's hypothesis on the gradual development of heavenly bodies, as he states it in his "Photometric Researches" (p. 231, &c.), has been confirmed *in its essential points* by the application of spectrum-analysis to the stars. We recognise the different states of cooling in the spectrum, and in the cases of some fainter stars we have distinct data that in the atmospheres surrounding incandescent bodies, chemical compounds may already form and continue to exist. The hypothesis on *new* stars is in no wise contradicted by the spectral observations made of the two new stars of 1866 and 1876. The very bright continuous spectrum and the bright lines, which at the beginning only slightly exceeded its brilliancy, could not be well explained if we only suppose a violent eruption from the interior, which again rendered the surface (or part of it) luminous, but are easily explained by the hypothesis that the quantity of light is considerably augmented through a simultaneous process of combustion. If this process is of short duration, then the continuous spectrum, as was the case with the new star of 1876, will very quickly decrease in intensity down to a certain limit, while the bright lines in the spectrum, which result from the incandescent gases that have emanated in enormous quantities from the interior, will remain for some time.

The observations of the spectrum show beyond doubt that the decrease in the light of the star is in connection with the cooling of its surface. The violet and blue parts decreased more rapidly in intensity than the other parts, and the absorption bands, which crossed the spectrum, have gradually become darker and broader.

Finally Herr Vogel regrets that the news of the discovery of the new star by Herr Schmidt was only known so late, as doubtless during the first few days most interesting changes must have occurred in the spectrum, while the star rapidly decreased in brightness. Herr Vogel recommends that in case of appearance of other new stars no time should be lost before spectral observations are made, and points out that even with small telescopes very useful results may be obtained, if care is taken that spectroscopes are used of sufficiently low power of dispersion.

The position of the new star with regard to two neighbouring stars of magnitudes 9·1 and 9·4 Herr Vogel has determined as follows :—

Nova − ∗ 9m·1 (*Bonn. Durchmust.*, + 42°, 4184)
1877·0 $\Delta\alpha = -25^s{\cdot}00$ $\Delta\delta = +1' \, 15''{\cdot}4$
Nova − ∗ 9m·4 (*Bonn. Durchmust.*, + 42°, 4185)
1877·0 $\Delta\alpha = -35^s{\cdot}34$ $\Delta\delta = -1' \, 13''{\cdot}2$.

As the first of the comparison stars has been determined at the meridian circle of Bonn, the position of the new star is :—

1877·0, 21h. 36m. 52s·48 + 42° 16′ 54″·5.

THE TELEPHONE[1]

In the following paper I call instruments employed in the transmission of musical sounds, tone telephones, and those employed in the transmission of the human voice, articulating telephones.

In the year 1837, Page, an American physicist, discovered that the rapid magnetisation and demagnetisation of iron bars produced what he called "galvanic music." Musical notes depend upon the number of vibrations imparted to the air per second. If these exceed sixteen we obtain distinct notes. Hence, if the currents passing through an electro-magnet be made and broken more than sixteen times per second, we obtain "galvanic music" by the vibrations which the iron bar imparts to the air. The iron bar itself imparts these vibrations by its change of form each time it is magnetised or demagnetised.

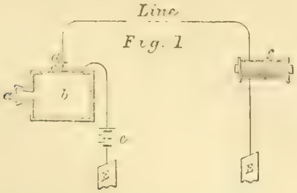
Fig. 1

De la Rive, of Geneva, in 1843, increased these musical effects by operating on long stretched wires which passed through open bobbins of insulated wire.

Philip Reiss, of Friedrichsdorf, in 1861, produced the first telephone which reproduced musical sounds at a distance. He utilised the discovery of Page by causing a vibrating diaphragm to rapidly make and break a galvanic circuit. The principle of his apparatus is shown Fig. 1.

b is a hollow wooden box into which the operator sings through the mouthpiece *a*. The sound of his voice throws the diaphragm *c* into rapid vibration so as to make and break contact at the platinum points *d* at each vibration. This interrupts the current flowing from the batteries *e* as often as the diaphragm vibrates, and therefore magnetises and demagnetises the electromagnet as often. Hence whatever note be sounded into the box *a* the diaphragm *c* will vibrate to that note, and the electromagnet *f* will similarly respond and therefore repeat that note.

Musical sounds vary in tone, in intensity, and in quality. The tone depends on the number of vibrations per second only; the intensity on the amplitude or extent of those vibrations; the quality on the form of the waves made by the vibrating particles of air.

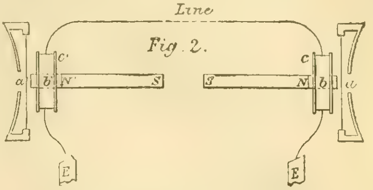
Fig. 2.

It is evident that in Reiss's telephone everything at the receiving end remains the same, excepting the number of vibrations, and therefore the sounds emitted by it varied only in tone and were therefore notes and nothing more. The instrument remained a pretty philosophical toy and was of no practical value.

Cromwell Varley, in 1870, showed how sounds could be produced by rapidly charging and discharging a condenser.

After alluding to the invention of Mr. Elisha Gray (NATURE, vol xiv. p. 30), Mr. Preece said :—

It remained for Prof. Graham Bell, of Boston, who has been working at this question with the true spirit of a philosopher since 1872, to make the discovery by which tone, intensity, and quality of sounds can all be sent. He has rendered it possible to reproduce the human voice with all its modulations at distant points. I have spoken with a person at various distances up to thirty-two miles; and through about a quarter of a mile I have heard Prof. Bell breathe, laugh, sneeze, cough, and in fact make any sound the human voice can produce. Without explaining the various stages through which his apparatus has passed, it will be sufficient to explain it in its present form. Like Reiss he throws a diaphragm into vibration, but Prof. Bell's diaphragm is a disc of thin iron *a*, which vibrates in front of a soft iron core *b*, attached to the pole of a permanent bar magnet N S (Fig. 2). This core becomes magnetised by the influence of the bar magnet N S, inducing all around it a magnetic field, and attracting the iron diaphragm towards it. Around this core is wound a small coil *c* of No. 38 silk-covered copper wire. One end of this wire is attached to the line wire, the other is connected to the earth. The apparatus at each end is identically similar, so that it becomes alternately transmitter and receiver, first being put to the mouth to receive sounds, and then to the ear to impart them. Now the operation of this apparatus depends upon the simple fact that any motion of the diaphragm *a* alters the condition of the magnet field surrounding the core *b*, and any alteration of the magnet field, that is either its strengthening or weakening, means the induction of a current of electricity in the coil *c*. Moreover, the strength of this induced current depends upon the amplitude of the vibration, and its form or the rate of vibration. The number of currents sent of course depends upon the number of vibrations of the diaphragm. Now each current induced in the coil *c* passes through the line wire to the coil c^1, and then it alters the magnetisation of the core b^1, increasing or diminishing its attraction for the iron diaphragm a^1. Hence the diaphragm a^1 is vibrated also, and every vibration of the diaphragm *a* must be

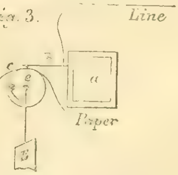
Fig. 3.

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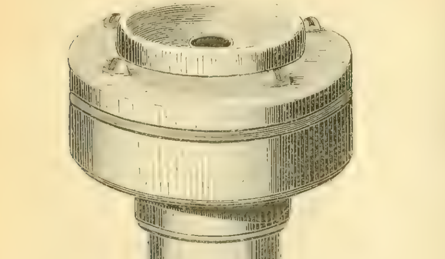
[1] Paper read by Mr. W. H. Preece, Memb. Inst. C.E., at the Plymouth Meeting of the British Association. For the sectional cuts we are indebted to *Engineering*.

repeated on the diaphragm a with a strength and form that must vary exactly together. Hence, whatever sound produces the vibration of a is repeated by a^1, because its vibrations are an exact repetition of those of a.

It is quite evident, however, that Bell's telephone is limited in its range. The currents operating it are very weak, and it is so sensitive to currents that when attached to a wire which passes in the neighbourhood of other wires, it is subject to be acted upon by every current that passes through any one of those wires. Hence, on a busy line, it emits sounds that are very like the pattering of hail against a window, and which are so loud as to overpower the effects of the human voice.

Now Mr. T. A. Edison, of New York, has endeavoured to remedy these defects in Bell's by introducing a transmitter which is operated by battery currents, whose strength is made to vary directly with the quality and intensity of the human voice. In carrying out his investigations in this field he has discovered the curious fact that the resistance of plumbago varies in some ratio inversely with the pressure brought to bear upon it. Starting from Reiss's transmitter he simply substitutes for the platinum point (d) a small cylinder of plumbago, and he finds that the resistance of this cylinder varies sufficiently with the pressure of the vibration of the diaphragm to cause the currents transmitted by it to vary in form and strength to reproduce all the varieties of the human voice. His receiver also is novel and peculiar. In 1874 he discovered that the friction between a platinum point and moist chemically-prepared paper varied every time a current was passed between the two, so that the rate with which the paper moved was altered at will. Now by attaching to a resonator a a spring b, whose platinum face c rested on the chemically prepared paper d, whenever the drum e was rotated and currents sent through the paper, the friction between c and a is so modified that vibrations are produced in the resonator e, and these vibrations are an exact reproduction of those given out by the transmitter at the other station.


Bell's Articulating Telephone.[1]

Edison's telephone, though not in practical use in America, is under trial. In some experiments made with it songs and words were distinctly heard through 12,000 ohms, equal to a distance of 1,000 miles of wire.

Bell's telephone is, however, in practical use in Boston, Providence, and New York. There are several private lines that use it in Boston, and several more are under construction. I tried two of them, and though we succeeded in conversing, the result was not so satisfactory as experiment led one to anticipate. The interferences of working wires will seriously retard the employment of this apparatus, but there is no doubt that scientific inquiry and patient skill will rapidly eliminate all practical defects.

To Prof. Graham Bell must be accorded the full credit of being the first to transmit the human voice to distances beyond the reach of the ear and the eye by means of electric currents.

THE BRITISH ASSOCIATION

REPORTS.

Report of the Committee, consisting of the Rev. H. F. Barnes, C. Spence Bate, Esq., H. E. Dresser, Esq. (Secretary), Dr. A. Günther, J. E. Harting, Esq., J. Gwyn Jeffreys, Esq., Prof. Newton, and the Rev. Canon Tristram, appointed for the purpose of inquiring into the possibility of establishing a Close Time for the Protection of Indigenous Animals.—Your Committee begs leave to report that the object for which it was appointed continues to receive a considerable share of public attention, and that during the past year the three Acts of Parliament establishing a close time for certain kinds of birds have attracted so much notice that there is no fear of their falling into neglect.

There is no symptom of the diminution of the interest which the Sea-birds Preservation Act (1869) has always excited; and within the past twelve months application for the extension of the close time has been made, according to the provisions of that Act, by the Justices in Quarter-Sessions of Northumberland, Lancashire, and the North Riding of Yorkshire—facts which sufficiently speak for the general appreciation of the measure.

The Wild Birds Protection Act (1872) is possibly viewed by the public with greater favour than either of the others; but your Committee sees little reason to modify the opinion of it expressed in former reports. Nevertheless a conviction under it, presenting some rather important features, in May last, indicates that it is not so entirely useless as had been thought.

The Wild Fowl Preservation Act (1876) came into operation this year, and at first undoubtedly caused some discontent in many quarters, a warm discussion of its principle and provisions being raised by a portion of the public press. Your Committee, however, has noticed with much satisfaction that virtually no objection was taken to its principle, while the necessity of some enactment of the kind was conceded on almost every side. Furthermore, very nearly the sole cause of complaint lay in regard to the limits of the close time therein imposed, on which point no blame attaches to your Committee. The limits of the close time proposed in the Bill, as draughted by your Committtee, and introduced into Parliament, were, as stated in last year's report, altered in its passage through the House of Commons; the change being such as your Committee then declared did not meet with its approval. Your Committee is therefore in no way responsible for the unseasonableness of the close time which was enacted, and believes that the soundness of its views on the subject is now generally admitted. In confirmation of this belief it may be stated that the Justices in Quarter-Sessions of the counties of Dorset, Norfolk, Kent, Somerset, Southampton, Wigtown, and Essex, have severally made application to the Home Office for such an alteration of the close time as will bring it more or less nearly in accordance with that originally proposed by your Committee.

Another charge was brought against this Act. It was alleged to be imperfect in that it did not expressly prohibit the possession or sale, during the close time, of birds of the kinds professedly protected, which had been imported into this country from abroad. This charge was supported by the dismissal (on the latter ground) by two magistrates of informations laid against certain poultrymen or game-dealers in London, and if it could have been sustained would undoubtedly have proved the Act to be defective. But the Royal Society for the Prevention of Cruelty to Animals appealed against one of these decisions; and on June 15 judgment was given in the Common Pleas Division of Her Majesty's Court of Appeal against the defendants in the case, thus proving that the legal interpretation of the Act agreed with the intention of its promoters.

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[1] This instrument was used by Mr. Preece in his experiments (see NATURE, August 23, p. 342).

Your Committee has satisfaction in finding that the Fisheries (Oysters, Crabs, and Lobsters) Bill passed the House of Commons on August 2, and it has now doubtless become law. It appears curious that no close time had hitherto been provided by the legislature for these important and favourite articles of food.

Having regard to the applications made from time to time to different members of your Committee by various persons interested in seeing the close time principle more widely applied, your Committee respectfully solicits its reappointment.

SECTION A.—MATHEMATICAL AND PHYSICAL.

PROF. S. Haughton gave a summary of the first reduction of the tidal observations made by the recent Arctic expedition. The results arrived at were of great importance, and as far as tidal observations were concerned, the late Arctic expedition was a complete success; they also came in at the right moment for comparison with those made in the *Polaris* expedition. Lieut. Archer had charge of the tidal observations made on board the *Discovery*, and his notes were as complete and valuable as any he had ever examined. The *Discovery* was anchored at Bilhow Harbour; the *Alert* lay off Cape Sheridan, and was in every way less favourably situated for observing the action of the tides. The *Discovery* rose and fell with every tide, while the *Alert*, on the contrary, was surrounded by grounded ice of a formidable kind. Observations were made on board the *Discovery* every hour, instead of once in four hours, as required by the instructions. It was an unprecedented feat that during seven months observations were made every hour, and that there were only six days in the whole course of that time in which he had to interpolate coefficients. The *Alert's* observations required coefficients for fifteen days. The results obtained by Dr. Bessel from the *Polaris* expedition were confirmed by the English expedition, viz., that there was a junction of two important tides in the largest portion of Smith's Sound. The position of the *Discovery* was much nearer to the point of meeting, and this afforded another reason why her observations were more valuable than those of the *Alert*. It seemed that a new type of tide had been discovered, which could not be confounded either with that from Baffin's Bay or that from Behring's Straits, thus confirming Sir George Nares's opinion that Greenland is an island, as this new tide could only have come round from the east coast by a northern route. This stronger tide also presented a feature of great promise in tidal theory, indicating the probability of actual measurable tides occurring every eight hours. Dr. Moss, of the *Alert*, who was present, explained the very different conditions under which the two ships wintered.

Sir William Thomson read a joint paper by himself and Capt. Evans *On the Tides of Port Louis* (*Mauritius*) *and Freemantle* (*Australia*). The investigation had been undertaken in consequence of the recent Government grant for scientific researches. A sum of 200*l.* had been voted from this grant on the urgent representations of Capt. Evans and himself for the purpose of investigating the tides of the southern hemisphere, and making certain definite advances in the investigation of the tides in other parts of the world, particularly in the Mediterranean. The first proceeds of the work done was the present paper. The observations showed that there was a very near approach to equality between the solar diurnal tide and the lunar diurnal tide. The diurnal tide at Liverpool and Freemantle was about the same, but the semi-diurnal tide at Liverpool was about two hundred times as great at Liverpool as at Freemantle. He hoped that as the investigations were continued, they would be able not only to obtain scientific results but to obtain a practical way of giving tidal information in a form which would be useful to sailors—a desideratum that had not yet been obtained. Hitherto the theory of the Government had been that the information given in the Admiralty tide tables was sufficient for the practical purposes of navigation; and it was on that theory that Mr. Lowe, when Chancellor of the Exchequer, refused the application of the British Association for assistance in the matter. The fact was that the Admiralty tables did not give all that was necessary for the purposes of navigation, so that at all events the reason given by Mr. Lowe was a bad one. However the money had now been obtained from the Government grant of 4,000*l.* for scientific purposes; a grant, the working of which would, he hoped, more than fulfil all the aspirations of those who looked forward to it as a great boon to science, likely to produce results beneficial to the whole world.

Prof. G. Carey Foster gave an interesting paper *On the Mode of stating Certain Elementary Facts in Electricity*, and Mr. J. Traill Taylor described a binocular microscope of high power which Capt. Abney stated overcame the many difficulties in the way of obtaining microscopic pictures. Mr. S. P. Thompson exhibited some new optical illusions, which were much appreciated by the section, and also an improved lantern galvanoscope; and Messrs. C. H. Stearn and J. W. Swan exhibited a new form of the Sprengel air-pump. Among the other papers were *An Account of some Recent Advances in the Lunar Theory*, by Prof. J. C. Adams; *On the Eclipse of Agathocles considered in Reply to Prof. Newcomb's Criticism on the Co-efficient of Acceleration of the Moon's Mean Motion*, by Prof. S. Haughton; *On the Lower Limit of the Prismatic Spectrum*, by Lord Rayleigh; *On a New Unit of Light for Photometry*, by Mr. A. Vernon Harcourt; *On a New Form of Apparatus to Illustrate the Interference of Plane Waves*, by Mr. C. J. Woodward; and *On the Physical Properties of Solids and Liquids in Connection with the Earth's Structure*, by Prof. Hennessy. This last paper referred to experiments made by the author on the motion of fluids with a view to determine the conditions of viscidity and friction by which such motions are influenced. The principal results had been communicated to the Royal Irish Academy; the investigation had reference to Hopkins's theory of the great thickness of the earth's crust.

Mr. W. H. Preece gave an account of the telephone (which we give elsewhere), with illustrations, and this excited much interest in the section. On a later day Dr. J. Graham Bell, who arrived at Plymouth during the meeting, gave a series of experiments with the telephone before Sections A and G conjointly to a large audience; it is scarcely necessary to say that the telephone was the chief attraction of the meeting.

The supply of valuable papers in Section A was very good, and the meeting, as far as this section is concerned, was a successful one, though the attendance was not so large as last year at Glasgow. On account of the number of papers it was necessary to divide the section into two parts on Monday and Tuesday, Prof. Cayley and Lord Rayleigh being respectively the chairmen of the sub-sections on these days. On Monday the sub-section was occupied wholly with mathematics. There were papers by Prof. de Haan *On the Variation of the Modulus in Elliptic Integrals*, by Prof. Cayley *On a Suggested Mechanical Integrator for the Calculation of an Integral* $\int (X\,dx + Y\,dy)$ *along an Arbitrary Path*, by Mr. J. W. L. Glaisher *On the Values of a Class of Determinants*, and *On the Enumeration of the Primes in Burckhardt's and Dase's Tables*, by Mr. H. M. Jeffery *On Cubic Curves*, and by Sir William Thomson *On Solutions of Laplace's Tidal Equation for Certain Special Types of Oscillation.* Prof. J. C. Adams gave *An Account of his Calculation of the first Sixty-two Bernoulli's Numbers.* Only thirty-one had been previously calculated. Prof. Adams had also calculated the value of the sum of the reciprocals of the first thousand integers to 260 decimals, and thence, by means of the Bernoulli's numbers, the value of Euler's constant also to 260 places.

At the conclusion of the business of the section Sir William Thomson referred to the great loss the Association had sustained by the death of Mr. Gassiot, which took place on the first day of the meeting.

SECTION C.—GEOLOGY.

On the Exploration of some Caves in the Limestone Hills in Fermanagh, Ireland, by T. Plunkett.—This paper gave an account of trial explorations begun in the caves in question. They will now be carefully explored by a committee of the Association, aided by a grant from its funds. Large numbers of bones associated with human remains are found in cave-earth under stalagmite. The author also states that "a human jaw was found imbedded in glacial clay and associated with scratched stones." It is evident that the caverns here are of great interest and importance.

On the Origin and Antiquity of the Mounds of Arkansas, U.S., by Prof. J. W. Clarke.—The mounds vary from three to five feet high, and are from fifty to 140 feet in diameter. The author suggests that they were evolved from the simple carnhillock by a race of men who followed the retreating glaciers.

A Short Sketch of the finding of Silurian Rocks in Teesdale, by W. Gunn, F.G.S.—It has always been believed that no rocks lower than the carboniferous limestone are exposed in Teesdale. The recent work of the Geological Survey has proved that certain peculiar beds at the Cronkley Pencil Mill are not carboniferous, but silurian. Messrs. Gunn and Clough have lately described these beds in a paper read before the Geological Society. The notes submitted to the Association record further discoveries of silurian beds near Widdybank Farm. They probably belong to the Stockdale series of pale slates.

Note on the Correlation of certain Post-Glacial Deposits in West Lancashire, by C. E. De Rance, F.G.S.—This paper described the post-glacial drifts of the Ribble Valley, and compared them with the drifts of the Lancashire Plain. The submerged forest of the coast and the peat of the plains are continuous with the peat of the valley. They contain beech-nuts. The Ribble has excavated its valley in glacial drift to a depth of from 150 to 200 feet; the sea at the same time has cut back the coast, forming a lowland plain on which the forest grew; subsequently the drainage became obstructed and the peat was formed. A subsidence of the land of some seventy feet or more submerged the peat and forest.

On the Influence of the Positions of Land and Sea upon a Shifting of the Axis of the Earth, by A. W. Waters, F.G.S.—The author pointed out how the unequal distribution of land and sea may be an agent in preventing the movements of elevation and depression of the land in one part of the globe, balancing those in another, and further showed how similar movements in various localities would differently affect the pole.

Any movement such as a submarine elevation which displaced water would spread it over the oceanic area, and the effect of this would, with the present configuration, be the same as if about one-twelfth of the weight had been added in the southern hemisphere along 45° 44′ long. E., viz., in a line passing by the entrance of the White Sea over the Caucasus through the middle of Madagascar.

As every submarine movement would create a force acting in this direction there seems reasonable ground for thinking the tendency would be for the shifting of the axis to take place near this line. Dr. Jules Caret considers that the pole must have moved approximately in a line passing through the meridian of 52° long. E., and what is cause, what effect, and how far they react on one another, is fully worthy of examination by any physical geologist.

The shifting caused by any elevation of land near the water or poles is very slight, so that the effect of the water displaced is up to about the fifth degree of latitude as great or greater than that caused directly by the movement of the land. From this it is apparent that near the equator a submarine movement may act on the pole in a contrary direction to that exercised by a similar movement nearer 45° lat. As about one-eleventh of the globe is included between the latitudes 5° N. and 5° S. the effects of the movements here are specially worthy of consideration.

The effects of the drying up of an ancient Caspian Sea was taken as an illustration of the points brought forward. The loss of water of double the area of the Caspian evaporating to a depth of about 200 feet would by the loss of weight of water, shift the pole about 166 feet towards the White Sea, but as this water would be so distributed as to cause additional weight along 45° 44′ long. E. in the southern hemisphere, it would shift the pole still further in the same direction, making a total of about 176 feet. If there were a Caspian Sea in the south along this line then similar phenomena would cause a movement of 156 feet as against 176 feet in the north.

On the Source and Functions of Carbons in the Crust of the Earth, by A. J. Mott.—Plants get their carbon from the air, and as carbon deposits in the earth's crust, from the graphite of the Laurentian to the Lignites of the Tertiary, are believed to be derived generally from plants, the origin of those deposits must be looked for in the source of the atmospheric supply. Calculations based on the reports of the Royal Commission on Coal, and other data, show that the average quantity of unoxidised carbon of vegetable origin in the earth's crust cannot be less, and is probably many times greater, than 3,000,000 tons per square mile of surface. This is 600 times as much as the atmosphere now contains in the form of carbon-dioxide, and if it had been drawn from an atmosphere originally charged with it to this extent, the oxygen liberated in the process would have been twice as much as now exists. As all animal life is destroyed by any considerable change in the constitution of the air; as it was abundant before the coal-formation, and as a great part of the carboniferous deposits are of later date, the theory becomes incredible. We are obliged to conclude that the carbon withdrawn from the air and returned to the ground by plants, has been annually supplied, and the liberated oxygen regularly removed, and the only rational explanation is found in the hypothesis that the oxygen and carbon are reunited; in other words, that carbon equal in quantity to the annual deposit is annually burnt underground.

The objection to this, founded on the small quantity of nitrogen in subterranean gases, is readily shown to be invalid. The annual deposit of carbon, which is a measure of the quantity annually burnt underground, is estimated at three cubic miles; the estimate being based on the annual produce and the known facts concerning its destination.

It is shown that by this process of oxidation and its physical consequences, the heat developed internally is probably equal to the annual loss, and that the earth, therefore, is not cooling. The extent of geological change thus accounted for is also considered, and the quantitative deductions are compared with known facts. It is concluded finally that the carboniferous deposits now existing can only be accounted for on the supposition of previous similar deposits, and consequently that nothing is known at present as to the origin of vegetable life, or concerning any period before it existed on the earth.

On the Occurrence of Pebbles in Carboniferous Shales in Westmoreland, by G. A. Lebour.—This was merely a note of occurrence of rounded and subangular pebbles of quartz or quartzite (which were exhibited) in a bed of carboniferous shale in Angill, Westmoreland. The pebbles were all of the same character, and were probably derived from some of the Lake District rocks and not from veins.

Notes on the Age of the Cheviot Rocks, by G. A. Lebour, F.G.S.—The Cheviot Hills consist of porphyrites, passing into granite and syenite; ashes and doleritic rocks also occur. These igneous rocks are newer than the silurians, on the denuded upturned edges of which they rest; they are older than the lowest carboniferous (or tuedian) beds of Northumberland, for these rocks are in part composed of porphyrite pebbles. This evidence fixes the age of the mass of the Cheviots as Devonian, or thereabouts. The author showed that on the south side of the Cheviots, near the head of Redewater, there are vesicular dolerites breaking through the tuedian beds; elsewhere (as in Punchestown Burn) there are doleritic breccias containing fragments of porphyritic and lower carboniferous rocks. We thus have evidence, in the Cheviot range, of rocks of probably Devonian, tuedian, and Bernician age, belonging to the same eruptive centre.

SECTION D.—BIOLOGY.

Department of Anatomy and Physiology.

ADDRESS TO THE DEPARTMENT BY PROF. MACALISTER, VICE-PRESIDENT.

AFTER referring to the strength and independence now possessed by the sciences of animal morphology and physiology, Prof. MacAlister referred to recent important advances in embryology. Among researches respecting the early formation and primary developmental changes in the egg, he alluded to those of E. van Beneden, Bütschli, Ihering, and Oscar Hertwig, classified under three heads. (1) What is the method whereby the stimulus to development directly operates on the egg; (2) What becomes of the germinal vesicle; and (3) In what manner and from what source the directive corpuscles arise, and what function do they serve in the animal economy. The next subject dealt with was the history of the primitive groove of the fertilised egg, as discovered by Dursy, Schäfer, Balfour, and Rauber. Prof. MacAlister could not but believe that a change had taken place in the position of the embryo on the surface of the germinal disc in the evolution of vertebrates, and that the primitive groove was the heirloom of this ancestral change. Coming then to the question of the origin of vertebrate limbs, the address referred to the researches of Balfour and others, showing that the limbs are the remains of continuous lateral fins. The professor then went on to say :—

The vertebrate animal is primarily composed of a chain of similar segments, and there is no *à priori* reason in morphology why any one metamere should not bear limbs as well as any other. Nay, from the analogy of chætopod worms we might expect that—as in these each zonite usually bears two pairs of parapodia or stumpy foot-processes—so in similarly derived and similarly segmented forms there might be at least traces of a similar multiplication of appendages.

In effect we really do find a somewhat parallel series in the metameres of fishes, for, as Mr. Balfour has shown, the medio-dorsal fin comes into existence precisely in the same manner as the lateral fin ridge, and being a double structure, as we learn, both in its specialised form and even in the structure of the cartilaginous precursor of the interspinous bones, it may reasonably be supposed to represent structures homologous with the system of notopodia in a laterally-compressed worm, fused together, while the paired fins may be regarded as the neuropodia, separated by the visceral cavity, and which, in the degraded and compressed metameres behind the visceral cavity, also coalesce, forming another primary ridge, that of the anal and caudal fin.

In relation to the primary source of origin and method of derivation of limbs we have to account for two separate factors, the limb-girdle and the limb-rays ; with regard to the former I can now only refer to the hypothesis of Gegenbaur and Dohrn, that the limb-girdles represent modifications of the visceral arches, and I pass this by with two comments :—1. That the visceral arches are themselves to a certain extent specialised, and consequently it would be better to state the hypothesis thus, that the limb-girdles and visceral arches are specialisations of corresponding paraxial structures in different metameres. 2. In the light of the evident fundamental complexity of the limb-girdles it seems a simpler explanation of phenomena to regard each girdle as made up of the arches of several, probably three or more, metameres fused, rather than as subdivisions of a single arch.

As to the primary nature of the limb-ray, Professors Huxley and Gegenbaur have taught us in their recent reconstruction of the theory of the Archipterygium, that the primitive limb was constructed somewhat like the limb-ray of Ceratodus, having a central jointed axis from which diverge fore and aft lateral processes, or, to use the elegant nomenclature of Prof. Huxley, the primitive vertebrate limb consisted of a column of mesomeres, to each of which a lateral pre- and post-axial paramere was articulated.

But even this form, though doubtless the stock from which the limbs of all vertebrates above the Dipnoi have sprung, is regarded, and with reason, by Gegenbaur as a derivative one, formed by the coalescence of a still more archaic arrangement of rays appended to the paraxial arches referred to above. It is possible that the primary fusion may have taken such a form as that which Gegenbaur represented in his original Archipterygium with more than one cartilage appended to the girdle, a form of which the arrangement in the dogfish and angel-shark may be representative, and these, by a still farther concentration, attended with an exaltation of the mesopterygium and a displacement of the propterygium as in Hexanchus, or of the pro- and metapterygium as in Cestracion, may thus reach the elongated form of the limb in Dipnoi. It seems obvious that this fish Ceratodus, though singularly generalised, has arisen from a point in the vertebrate stem above the starting-point of the elasmobranchs.

Whether this has been the case or no, whether the elasmobranch has been derived from an earlier condition than the dipnoan progenitor or no, the researches of Prof. Huxley have made it plain that it is from the meso-, and not from the metapterygium that the single basal ray bone of the higher vertebrates has arisen.

A curious question will naturally occur to any one considering the genesis of limbs, What is the reason that in vertebrate animals the number of limbs is limited, and apparently has been always limited, to four? and as we have seen that there is an ontological possibility that each of these contains elements from several metameres, there is no morphological reason, and therefore must be some mechanical cause for this limitation. Were the primitive vertebrates terrestrial we could understand that the tetrapod has a mechanical advantage over the tripod or any condition with an inferior number of limbs, both statically from the indeterminateness of the strain on each support in the four-legged form, and in progression, from the easily understood conditions of stability of equilibrium in walking ; while the tetrapod excels the hexapod or millipede not only because, by a reduction in number, the amount of nutrition required for the use of the limbs is minimised, but it is absolutely demonstrable that the facility of rotation is increased by the reduction of the limbs to the lowest number consistent with other conditions of utility. In connection with this point Prof. Haughton has made some curious observations, the results of which I hope we shall have laid before us in this department during our present meeting.

But the earliest vertebrates were aquatic, and yet even here we find the four-fold division of these actival appendages. These primitive forms differed from worms, in the greater amount of fusion of their metameres, which at an early period had ceased to give to these animals an externally jointed appearance, so we may learn from Amphioxus, which has branched from the vertebrate stem long before most of the secondary characters, which are constant throughout the rest of the vertebrates, had been foreshadowed. Being thus more consolidated than worms, and moving, as they would necessarily do, more as a unit and less as a chain, the advantages of the mode of propulsion by a tail, over swimming by means of the continuous lateral fin of united parapodia, would be increasingly manifest with increasing somatic rigidity. Hence, naturally, the parapodia of the hinder somites would coalesce to form a tail as they have done in fishes, and the appendages placed farther forward would undergo retrogression unless some function could be found for them which would make their retention an advantage in the economy. In the long worm-like forms like lampreys, such a retrogression has absolutely taken place, as in fishes of this form the use of lateral fins is reduced to a minimum ; hence in the elongated form of ordinary fishes, like eels, band fishes, and blennies, the lateral fins become rudimental or varied. But these organs are of obvious use in giving a capacity to alter the plane of motion, a power which is necessary for most fishes, as they only act in elevation and depression but in lateral rotation, as any one can verify for himself by watching fishes in an aquarium. In order to accommodate these united lateral appendages most conveniently to the sinuous curves into which the body of a fish is thrown in swimming, and to diminish the surface of resistance to the water, the parapodia have divided themselves into two groups, leaving the centre of the body, where the cephalic and caudal curves meet during progression, free from lateral appendages.

The address concluded by a brief notice of work in human anatomy. It was remarked that much remained to be done in details even in this well-wrought department. We had not available correct and broadly based statements regarding the average conditions of the variable parts of the human body. Considering that something like six hundred bodies were annually dissected in Great Britain and Ireland, such a basis ought to be easily attainable. In such work Prof. Wenzel Grüber, of St. Petersburg, the prince of descriptive anatomists, was laborious and indefatigable. Yet still much remained to be done to make human anatomy a really scientific study, a practical application of morphological principles.

One of the features of this department was Prof. Haughton's discourse on the best possible number of limbs for an animal. The criterion was the most economical expenditure of force, according to mathematical principles. With regard to land animals, it was shown that the three-limbed vertebrate is superior to two and one-limbed forms, being perfectly stable when resting on the limbs. The four-limbed vertebrate, however, was able to preserve perfect stability on any three of its limbs, while using the fourth for offence. There was for the most part no advantage in having five or a greater number of limbs, the cost of feeding the extra limb outweighing the advantage of possessing it. Arboreal animals developed the tail as a fifth limb because of its special advantage in the medium in which they lived. Man gave up the superior stability of having four limbs for support in exchange for two adapted to higher uses, and in correlation with his brain-power. In discussing the limbs of aquatic animals, Prof. Haughton showed how advantage in the use of force and in directing motion was gained by concentration of motor power and limitation of the number of limbs. The three-limbed and one-limbed swimmers had advantages over all others, and odd-limbed forms over even-limbed. In many respects the three-limbed swimmers were superior to the one-limbed, but the latter had the valuable quality of being able to apply a maximum force in one direction, and so escape from their enemies. Prof. Haughton indicated fishes [as really possessing one principal limb in the tail, the fins being only capable of exerting a very slight force when compared with the tail.

The Rev. W. H. Dallinger described some of the processes and results of his investigations into the life-history of the simplest organisms. Among the points especially dwelt upon were the development of four out of two flagella in his calycine monad after fission; the determination of the diameter of the flagellum of *Bacterium termo* to be the two hundred and ten-thousandth of an inch; and special contributions to the spontaneous generation question. Although the life-histories of the forms examined were perfectly definite and distinct, yet the result of his investigations was to give a conviction of the truth of Darwinian principles among simple forms at any rate. He mentioned as a contribution to physiology the discovery that so highly organised an Infusorian as Paramecium could live for a long period in Cohn's (inorganic) solution. An important series of experiments had been conducted with regard to the extent of the adaptability of monads to changed conditions of temperature. It had been found that they could, by gradual increments, be raised from a temperature of 45° to one of 125°, and live and multiply perfectly; but a sudden transition of a much smaller extent was immediately fatal, as was also a sudden fall of temperature. Another series of inquiries showed that it took much longer to produce a modification in the ovum or germ than in the adult.

Prof. Rolleston, in a paper *On the Vascular Supply of the Brain*, ascribed right-handedness to the greater vascularity of the left side of the brain. He further speculated on the probable entire disappearance of the occipital lobe of the brain on account of its deficient relative supply of blood. Prof. Allen Thomson called attention to a remarkable series of photographs, many applicable to the stereoscope, representing the blood supply of various parts of the system. They were executed under the superintendence of Prof. Dantscher, of Innsbruck, from preparations made by himself. Dr. W. H. Pearse read a paper *On the Geography of Consumption in Devonshire*, claiming to demonstrate that the wildest moorland districts had the lowest death-rate from that disease. While England and Wales showed a rate for consumption of 2·47 per 1,000, the rate for western Dartmoor was 0·37, and for the west of Exmoor 0·45. This was notwithstanding the great rainfall.

Mr. G. T. Bettany gave a summary of the conclusions as to the structure and interpretation of the vertebrate skull, contained in the work on the Morphology of the Skull, just published by Prof. Parker and himself. The nature of the trabeculæ and the segmental relations of the skull were especially dwelt upon. In another paper Mr. Bettany advocated the harmonisation of animal and vegetable physiology, by the use of terms in the same sense in both, referring especially to the term assimilation, now so vaguely and discordantly employed in the two sciences. Among other papers may be mentioned Prof. McKendrick's *On the Physiological Action of the Substitution Compounds of Pyridene and Chinoline*, and Dr. Cunningham's, *On the Muscular and Nervous Systems of several little known Marsupials.* Only a small number of anatomists and physiologists were present, and the paucity of papers in physiology was ascribed to the discouragement and hindrance to experimental research caused by the vivisection controversy.

Department of Anthropology.

No striking novelty was brought forward in connection with the geological evidences of the antiquity of man. Various local discoveries of flint implements were noticed, the occurrence of flints in Cornwall and the Scilly Islands being of especial interest, owing to their distance from the chalk. Mr. John Evans described some palæolithic implements from the valley of the Axe, made of chert from the Blackdown beds, proving that where chalk flints were scarce, other siliceous rocks were utilised by palæolithic men for the same purpose. Mr. Widger's collection of extinct mammalia from Newton Abbot caves, as yet only imperfectly explored, excited much interest. Mr. Laws gave an account of a cave at Tenby containing a rich aggregation of extinct mammalia, and recommended the Welsh coast as a rich hunting-ground for anthropologists. Prof. Rolleston and Col. Lane Fox described their exploration of tumuli at Sigwell in Somersetshire, and near Guildford. The co-existence of different styles of interment among a people was confirmed by these researches. Prof. Rolleston's address *On our Knowledge of the Flora and Fauna of Prehistoric Times* was very attractive. In the course of his remarks on trees he noticed that remains of the ash were found in English but not in Scotch peat; the beech again was absent from Scotch peat. With regard to bees and bee-keeping, he said the only certain fact known in relation to the question where and when hives were introduced, was that in all cases the word for hive was allied to the Latin word for it. He thought honey became important very early, as a source of sugar; especially to people who had neither the sugar-cane nor beet-root. He believed great changes had taken place in British mollusca since prehistoric times. The so-called Roman snail was without doubt a very old and well-established British snail; while a little snail most abundant at present was not known in prehistoric deposits. He thought there were the best grounds for believing the rabbit not to be prehistoric in this country. In an old tumulus Prof. Rolleston had found several handfuls of jawbones of the common water vole, and not far off a large canine of a polecat, which had evidently made its nest in the tumulus and fed its young upon water-rats. These are but a few out of many most interesting facts mentioned in the paper. Mr. Spence Bate's description of the prehistoric remains on Dartmoor, visited by an excursion party, was another subject that attracted much attention.

There were a number of valuable papers on the habits, history, &c., of uncivilised peoples. Miss Buckland's paper *On the Stimulants of Ancient and Modern Savages* showed how in all parts of the world, as soon as agriculture was established, people began to make fermented drinks from the roots or grains cultivated for food. But it further appeared that still earlier stimulants were leaves and roots that when chewed were found to produce exhilaration. With the dawn of civilisation these roots and plants were steeped in water, inducing some fermentation. Later the cereals were used, the roots and plants being still retained for flavour and to produce fermentation. The juice of the grape became employed at a subsequent period still. Mr. Bertram Hartshorne read a valuable paper *On the Ancient People of Ceylon*, giving an account of their history as far as it could be gathered, giving many evidences of retrogression among them. The Rev. S. J. Whitmee, the well-known London Society missionary, gave some account of the Malayo-Polynesians, with abundant proofs of their degradation from a higher social and intellectual level. Among these were the comparatively high social position of the women, the existence of hereditary rank and titles, the tenure of landed property, the systematic division of land, and their poetry. Mr. F. M. Hunter described the peculiarities of Socotra Island. In reference to the Bedouin inhabitants, he said that religion seemed to sit lightly upon them. They only prayed when they had an audience, and even in the very act of prostration they would turn round, join in conversation, and again continue their devotions. The mark of the cross was still used on the head-stones of the graves. Mr. A. Simson's paper *On the Zaparos Indians* contained a great deal of curious Information.

Coming to regions nearer home, Dr. Beddoe endeavoured to trace the history and ethnology of the Bulgarians, but confessed that much was still unknown on the subject. Although they spoke Sclavonic they did not appear to be really Sclavs or even Turanian. The original Bulgarians were a tribe from the Volga region, probably connected with the Huns. Dr. Beddoe believed that the present Bulgarians were as much Ugrian as anything; they were evidently prognathous. The Rev. W. S. Lach-Szyrma in discussing the Cornu-Britons, divided them into two classes, those who came there under pressure of Saxon invasions, and those who came as immigrants to the coast districts. He did not believe in any Semitic admixture.

The report of the Anthropometric Committee spoke of the serious difficulties attending its observations in the attainment of uniformity and accuracy. Mr. H. C. Sorby described the several different colouring matters he had derived from human hair, the chief being a black pigment and a reddish-brown substance. Very red hair contained a small quantity of pink-red substance. A noteworthy opinion given by Mr. Sorby was that he did not think it possible that hair could change colour in a single night. Other interesting papers were by Prof. Rolleston *On Artificial Deformations of the Human Head* and *On the Rationale of Brachycephaly and Dolichocephaly*, by Dr. Phéné and others. Altogether the department sat on five days, and exhibited much vigour; both papers and discussions were very attractive, although the amount of novelty in regard to primitive man and his relations to geological history was not great.

Department of Zoology and Botany.

In this department the greatest general interest was excited by Mr. McLachlan's paper *On the Colorado Beetle.* He gave a most valuable account of its natural history, and then proceeded to remark on the panic respecting its introduction into England. He approved of restrictive legislation, but said it was as much needed four or five years ago as now. He believed if the beetle could have been introduced if would have been ere now, not so much among potatoes, which were mostly imported from America for seed, in a very clean state, or through the reception of specimens by scientific men, as in a promiscuous manner in general merchandise, owing to its great abundance on the quays of New York, &c. But there was much reason to expect the beetle could not readily be acclimatised in our moist climate. American animals in general failed to spread in Europe. Moreover, Great Britain possessed many insectivorous birds which had no representatives in the United States.

Prof. McNab read several important papers on botanical subjects. One, *On the Movement of Water in Plants*, gave an account of researches by Professors Pfitzer and Hoehnel, continuing and confirming results arrived at by his own investigations. Prof. McNab also brought forward a revised classification of plants, in which he adopted the term Order for the larger groups sometimes called cohorts, and in which the apetalæ were distributed among the petaloid orders to which they are allied. He further gave a synopsis of the present knowledge of fossil flowering plants, showing that Haeckel's postulates as to the evolution and period of first appearance of flowering plants were already shown to be false by new discoveries. It appeared, on the whole, that the gamopetalous forms arose later than the dialypetalæ. Prof. McNab inclined to the opinion that while the monocotyledons were monophyletic, or arose through one line of descent, the dicotyledons were polyphyletic, or derived from several main stocks.

Other papers of interest on botany were by Prof. Dickson, *On the Structure of the Pitcher of Cephalotus*, and by Mr. A. S. Watson, *On Structural Characters in Relation to Habitat in Plants*.

Prof. Rolleston described several features of interest in the zoology of New Guinea, especially the new Echidna, of which he had received a specimen. He detailed the evidences of the former connection by land between New Guinea and Australia, and accounted for the divergence between the vegetation of the two by influences due to the high mountains of New Guinea and the great barren plains of Australia. Mr. W. Ackroyd read a paper *On the Colours of Animals*. A contribution was read from Dr. Otto Finsch, giving the results of the North German Exploring Expedition to Western Siberia. Dr. G. Bennett, from Australia, gave an interesting account of the habits of the pearly Nautilus.

SECTION E.—GEOGRAPHY.

In this Section, presided over by Admiral Sir Erasmus Ommanney, the president's address consisted of a *résumé* of geographical discovery during the past forty years. Among the papers read none were of special interest.

Major Wilson read a paper by Lieut. Kitchener, R.E., *Report of the Line of Levels from the Mediterranean to the Sea of Galilee*. The levelling commenced in June, 1875, but was soon interrupted by local circumstances. It was resumed in March, 1877, and carried to a successful conclusion by Lieut. Kitchener. There had, as yet, however, been no opportunity of applying corrections. The result of the work showed the depression of the Sea of Galilee to be 682·544 feet. Fortunately they had a perfectly calm day for securing the sea-level. The sea left a clearly-defined white mark at its highest part; the depression shown by Kitchener's observations was some forty or fifty feet greater than had been generally supposed. The depression of the Dead Sea was 1,292 feet, which gave to the Jordan a fall of a little over 600 feet; this fall was nearly even throughout, although there were one or two rapids in its course. The deepest part of the Jordan Valley would be 1,300 feet below the level of the Mediterranean.

Commander Cameron read a paper *On the Proposed Stations in Central Africa, as Bases for Future Exploration*. He thought the best means for the exploration of the continent would be the establishment of trading societies after the fashion of the East Indian and Hudson's Bay Companies, but the spirit of the age was against the granting of sovereign powers to commercial companies. That being so, he considered a system of central stations, placed at intervals of from 200 to 250 miles distant, the best available. The new stations should be placed under the charge of a consul or a vice-consul, or of consular agents, but if the British Government hesitated to undertake the responsibility, the Seyyid of Zanzibar should be asked to accept it, and he believed most of the traders and shyas would recognise the authority of his highness. The stations might be turned to account, not only for the purpose of map-making, but of ascertaining the commercial value of the surrounding districts, obtaining meteorological observations and botanical and zoological collections, accustoming the neighbouring populations to the nature and advantages of civilised rule, systematically extirpating the slave trade, and diverting the traffic now employed in this infamous trade to the development of the enormous national wealth of the continent. In concluding, Commander Cameron made an appeal on behalf of the British Society for African Exploration. He warned his audience that unless Britain speedily bestirred itself it would lose the pre-eminence it long enjoyed among the countries of Europe. At the call of its sovereign the little kingdom of Belgium had contributed 12,000*l.* for the fitting out of an expedition. Portugal had contributed 20,000*l.*, and he hoped the British public would not fail to do their duty as respects the opening up to civilisation of Africa. The more rapidly the light of civilisation is introduced into the continent the more rapidly would the slave trade and domestic slavery die away and become a thing of the past.

A paper by Mr. W. H. Tietkens, *On the Latest Exploring Expedition across Australia*, was read by Mr. Bates. It described the journey made in 1875 by Mr. Ernest Giles, accompanied by the author of the paper and by Mr. Young, from the settlements of South Australia to those of Western Australia. The result of the journey was the conclusion that the occupation of some portions of the country cannot be long delayed, being well adapted for wool growing, but the writer confirmed the opinion expressed by other travellers that the region between lat. 21° and 30°, and long. 123° and 132°, can never be colonised, and that any white men settling in it would become like the wandering nomads now inhabiting it. The expedition was most adventurous owing to the attacks made upon it by some of the natives, and also to the difficulties arising from scarcity of water, which, at one time, threatened to terminate the career of the travellers.

Mr. Trelawny Saunders called in question the conclusions come to by the author of the paper as to the future of Australia, and said such a future would be a serious matter for Plymouth, which was one of the great ports of emigration to Australia. He placed against this opinion the authority of Mr. Landsborough, who had recently described the great physical changes taking place in the Australian continent, in consequence of the occupation of it by our countrymen. Trees were growing where none were previously to be seen, the natives being prevented from burning the long grass for the purpose of getting game. The growth of forests would cause a greater rainfall, and lead to the improvement of the productive qualities of the country.

Col. Godwin-Austen read a paper *On the Course of the Brahmaputra or Sanpu*. The author's argument, founded on researches and surveys in Assam, was that the true outlet of this great river was by the Sukushiri, and not, as was generally supposed, by the Deipong.

THE FRENCH ASSOCIATION AT HAVRE

Havre, *August* 30

The final meeting took place this afternoon at 2 o'clock, under the presidency of M. Broca.

Montpellier has been selected as the place of meeting for 1879. The University of Montpellier has been for centuries a rival to Paris, and even now is bold enough to compete with its formidable rival. Montpellier is also notable as the birthplace of Auguste Comte, the founder of Positivism. A number of scientific gentlemen have subscribed a large sum to receive the Association in 1879; the General and Municipal Councils will also vote a large sum.

It has been suggested by some influential members that the meeting of 1880 should be held in Algeria. But the vote will not be taken till next year at Paris. The year 1880 is the fiftieth anniversary of the conquest of Algeria. There are at the present time very few scientific institutions in the colony. Should the choice be made, the Algiers Academy would probably be then transformed into a University for Algeria.

The Association, at the close of the Havre meeting, has voted an exceedingly limited number of recommendations to the Government. One of them relates to the organisation of the meteorological service. The Association directs the attention of the Government to the inferiority of French meteorology, and urges the Government to establish an official investigation into its working. All efforts to take the Service des Avertissements out of the hands of the observatory have been defeated. The proposed reform does not aim at diminishing the influence of M. Leverrier on the service he has created in France.

A request is to be sent to the Government to endow the Geological Society of Normandy with the privileges of an institution of public interest, which means to grant it a charter and incorporate it. It is expected that the admirable geological exhibitions collected through M. Lennier's exertions will remain permanent, and become a fair specimen of regional geology. A number of exhibitors have consented either to give their objects or to lend them until similar objects can be procured in their stead. M. Lennier, whose exertions have been indefatigable, is the director and founder of the Havre Aquarium, which is a model institution, not for the variety of species, but for the number of objects and the health of the animals.

The several industrial establishments at Havre were opened for public inspection, as well as the Government buildings. The most interesting object was a fog trumpet of British make. The steam engine working it has a power of three horses. It is calculated to compress about 800 litres of air at a pressure of little more than two atmospheres in two tanks put into communication by a large tube. One of these tanks is in communication with the pump, and the other with the trumpet. The latter is closed by a self-acting valve, which opens once every forty-eight seconds, and during seven seconds gives a voluminous sound in *la* of the diapason.

The last excursion (to Rouen) was more successful than the first. The *Frigorifique* had been sent from Havre to Rouen in order to increase the attraction, and was visited by many of the excursionists.

The number of members of the Havre meeting was not more than 600 altogether. The foreign members numbered about forty, upwards of twenty being Englishmen. Very few ladies attended the proceedings. No final banquet took place, owing probably to the fear of some political discussion disturbing an assembly which ought to be devoted entirely to science.

There is only a single scientific society in Havre which, in spite of its ambitious title (Société d'Etudes diverses) has only sixty members out of a population of 85,000, including more than fifty millionaires. The society meets regularly and publishes yearly a handsome volume. It has been decided to establish a local society of commercial geography, and a local committee to collect meteorological observations taken on board the transatlantic steamers.

If we consider the work done in certain sections the Havre meeting has not been a failure; but it was altogether a sectional meeting from the inaugural speech of President Broca to the two or three lectures which were delivered in the theatre. We are confident that M. Fremy will spare no pains to render the Paris meeting next year a success and as far as possible international. He will endeavour to get the presidents of sections each to deliver an inaugural address. It is intended to establish a temporary daily newspaper to publish at full length the reports of sectional proceedings, &c.

In the Section of Meteorology the principal business was the vote of the requisition sent to the Government, which will probably induce the Ministry to increase the grant to French meteorology. The majority of the section are in favour of the establishment of a central meteorological institute to investigate large meteorological problems and centralise all meteorological services except weather previsions. A resolution was proposed by Dr. Janssen, urging transatlantic companies to take meteorological observations on board; another resolution asked M. Giffard to organise a meteorological observatory on board his captive balloon, during the exhibition of 1878. Mr. James Glaisher gave an address on the result of his thirty scientific ascents, and the experiments made in the Ashburnham captive balloon. His address was well received, and he was invited to sit with the bureau. M. Alluard, the Director of the Puy-de-Dôme Observatory, engaged to establish intermediate stations on the flanks of the mountain, and to keep observations during the time when the monster captive balloon is making its ascents. This proposal will be communicated to Gen. Nansouty, Director of the Pic-du-Midi Observatory. A resolution was voted protesting against the delays in the construction of a telegraph line from Pic-du-Midi to Bagnères, for the purpose of sending regular observations during the time when the observatory is cut off by snow from all communication with the world below.

M. Alluard presented to the section diagrams of comparative barometric measures taken on the summit of the Puy-de-Dôme and at Clermont-Ferrand during storms. This shows clearly that the law of pressure varies in inverse ratio, diminishing on the top of the mountains when increasing at Clermont, and *vice versa*. It shows evidently that storms are produced, not by a single wind, but by a conflict of several winds at a certain distance from the earth.

In the Geological Section a large proportion of the papers were on various points connected with the geology of Normandy, one of the most important being a paper by M. Morière on the presence of the liassic stage in the department of Orne and on the fossils he has collected during many years investigation into the geology of the region. In this section, also, M. Pomel read a long paper to prove that M. Roudaire's project of an inland sea in Algeria, of which we recently gave details, is impossible M. Pomel maintains that the level of the Chotts is too high, and that if by any process they could be filled, the water would very soon find its way back to the Mediterranean. Another paper of some importance was by MM. de Tromelin and Grasset, being a "Summary Study of the Palæozoic Fauna of Lower Languedoc and the Pyrenees," for the purpose of comparing the primary formations of the South with those of the North-West of France. M. Jannettaz gave an account of his observations on the propagation of heat in schistose rocks and in crystals. From his experiments he concludes that heat is transmitted more readily along the planes of cleavage of crystals and along the plane of schistosity of slates, gneiss, crystalline or agillaceous schists, than along the direction perpendicular to these planes. He thinks we may thus explain, to a great extent, the variation observed in the increase of temperature with depth in different parts of the globe.

In the Botanical Section M. Corenwinder continued his account of his investigations on the Functions of Leaves. After twenty-five years' work M. Corenwinder thus sums up the results he has obtained:—The leaves of vegetables in their relations to the atmospheric air are the seat of two distinct functions. By their protoplasm they absorb oxygen and constantly exhale carbonic acid. By their chlorophyll, they inspire, on the contrary, during the day only, carbonic acid, and expire oxygen. In their early stage the protoplasm predominates; chlorophyll is not abundant. Hence the respiratory function predominates during all that period over the chlorophyllian function, and consequently leaves exhale carbonic acid without interruption. In proportion as leaves grow the colourless protoplasm diminishes and the chlorophyll increases; thus the capacity of emitting carbonic acid rapidly decreases, and soon they exhale, during the day, nothing but oxygen gas. Henceforth it is only by shutting off or diminishing the light, when the action of the chlorophyll is diminished or suspended, that the effect of respiration becomes more or less sensible. There is then among living beings only one kind of respiration. The part played by chlorophyll is of a different order; it is an act of assimilation which has nothing in common with the preceding. M. Corenwinder hopes that henceforth it will cease to be taught that plants are provided with two respirations, one for the day and the other for the night.

NOTES

WE are informed by a cable telegram from a New York correspondent that Prof. O. C. Marsh, the eminent palæontologist, has been elected president of next year's meeting of the American Association for the Advancement of Science, which has just concluded its Nashville session. We have received the *Proceedings* of the last year's meeting at Buffalo.

THE Aquarium Winter Garden at Tynemouth, near Newcastle, is rapidly approaching completion. The building occupies a commanding position on the Long Sands between the town of Tynemouth and the little fishing village of Cullercoats. The entire basement is devoted to the Aquarium with its reservoirs and pumping machinery, and the show tanks, of which there will be both a sea and a fresh-water series, will contain upwards of 140,000 gallons of water. The plan adopted is the circulation system, which has proved so successful in the maintenance of aquatic life at the Crystal Palace, Naples, and elsewhere. Special arrangements are being made for the culture of salmon and trout, and in conjunction with the Aquarium, but out of doors there will be an enormous seal-pond, into which a supply of sea-water will be pumped direct when required. Mr. E. Howard Birchall has been appointed curator.

Mr. Alexander Buchan, the well-known secretary to the Scottish Meteorological Society, has been sent to Upsala as representative of the Royal Society of Edinburgh, at the celebration of the 400th anniversary of that University.

The autumn meeting of the Institute of Naval Architects in Glasgow was brought to a conclusion last Thursday, and seems to have been altogether successful. A large number of papers were read and discussion raised on the technical subjects with which the Institute deals, and much of the time was devoted to visiting several of the most important Clyde establishments. The next autumn meeting of the Institute will probably be at Newcastle-on-Tyne.

On August 29 the Dorset Field Club met at the romantic spot of Lulworth Cove. One section gave themselves to the geology of the district in which the "Purbecks" are so finely exhibited. After some opening remarks by Mr. Mansell Pleydell, the president of the Society, Mr. Damon, F.G.S., gave an address bearing on the geology of the cove and neighbouring strata, followed by Prof. Buckman. Other sections directed their attention to entomology, botany, &c.

A summer school of practical mining for the instruction of the student in the details of miner's work has recently been added to the curriculum of the School of Mines of Columbia College, U.S. This summer school has been put in the charge of Henry S. Munroe, a former graduate of the school, who was recently elected by the trustees to the position of Adjunct Professor. It is proposed that the students of the school shall visit mines in different parts of the country worked for coal, iron, copper, lead, precious metals, &c., and spend a month or six weeks at each, making a careful and detailed study of the mine, and actually engaging, under the instruction of skilled miners, in all the details of mine work. The first experiment was made this summer at the mines of Coxe, Bros., and Co., Drifton, Pa., and with very remarkable success. A dozen students, volunteers from the class of '78, made a stay there of five weeks from July 2. Half of the students worked in the morning and the other half in the afternoon, one squad at a time with each miner. The times of going and coming to and from work were so arranged that each student spent from four to four and a-half hours in the mine each day. On coming out of the mine each squad was required to make a written report of the work done, with sketches showing the location, direction, depth, &c., of each shot fired, and the effect produced. While in the mine they assisted the miner to load his car, thereby learning readily to distinguish "slate" and "bony" from good coal, even in the uncertain light afforded by their mine lamps. They were also instructed in the use of the drill and pick, boring themselves the blast holes, judging the quantity of powder required, making up the cartridge, tamping, and firing the shot. After the students had spent in this way about twenty days at different kinds of work underground and in the "breaker," subjects for more detailed study and investigation were assigned them. Each student, having chosen his theme, spent the remainder of his time in collecting material for a memoir. The experiment has, in fact, been in every way a success.

At the meeting of the Royal Society of New South Wales on June 6, Mr. Russell exhibited an improved form of bichromate battery, by which the current of electricity generated is kept quite constant so long as it may be required. This is accomplished by allowing the bichromate solution to drop in slowly, and flow out at the same rate through a pipe which commences at the bottom of the cell and passes through the side at three-quarters of an inch from the top. When the supply tap is turned, the solution collects in the cell until it rises to the level of the pipe, and it then begins to pass out as fast as it comes in. As the bichromate solution passes down the cell its active properties are made use of, and when it reaches the bottom it is waste, and passes out as described. In use it is found that the zinc and solution are more economically used than in the ordinary bichromate cell. The Society held its annual *conversazione* on May 16. About 600 members and their friends were present. Amongst the exhibits were some very rare and choice plants from New Guinea. Mr. Russell, Government Astronomer, was most successful in showing large smoke vortex rings, which blew out a spirit-lamp flame at a distance of fifty feet.

The Report of the Leicester Literary and Philosophical Society speaks satisfactorily of its progress. A year ago the society took possession of new buildings, and its main work as a society is carried on by its six sections, which include the various departments of science. The number of members now exceeds 300.

We have received a very useful Russian *brochure* by M. Bogdanoff, "A Review of Expeditions and Natural History Researches made in the Aralo-Caspian Region from 1720 to 1874," being the first fascicule of a large work. In 1874 Prof. Barbot-de-Marny, geologist, with MM. Bogdanoff and Butléroff, jun., zoologists, explored vast tracts of land between the Caspian and the Aral lake, and MM. Grimm and Alénitzin studied the fauna of these two interior seas. Preliminary reports by the explorers have been already published. M. Bogandoff's paper is intended to give, in a very condensed form (52 pages), an introductory review of scientific work in the Aralo-Caspian region during the 150 years before this expedition. It has been visited and explored by no fewer than sixty men of science. Yet the great problems so vividly and skilfully developed and discussed by Humboldt and his followers—the problems of the former extension of the Caspian, of its junction with the Aral and the Balkash, of the geological time when this immense interior basin existed, of the causes which determined its drying up and the change of bed of the Amu-darya—all remain as open a field of inquiry as ever. We find immense gaps in every department even in the description of the country. The hydrology of the Caspian and Aral is sufficiently well known, but the third great basin, the Balkash, and a great number of smaller lakes remain unexplored. The flora is well known, and the great work of Borshioff sums up numerous local accounts, but the zoology is very unequally advanced; the lower organisms and the amphibiæ are all but totally unknown. The insects, and still more the vertebrates, are well described, but even in this branch the fishes have been neglected. Prof. Kessler finds many unknown forms even among those of the Caspian, which have been best explored, and the fishes of the Aral and Balkash promise to present plenty of most interesting new forms and varieties. As to the much-debated question of the former bed of the Amu-darya, M. Bogdanoff points out that this remarkable geological phenomenon remains unexplained. We must hope that the labours of the Aralo-Caspian expedition, and of other explorers in the same region, will finally throw some light on the problems involved in the study of this depression of the old continent.

In an interesting pamphlet on "The Work of Mechanics' Institutes in our Towns," by Mr. Swire Smith, the author contends that the work of mechanics' institutes in towns consists on the one hand in supplying the deficiencies of the day school, and directing its education into useful channels; on the other in giving general information and providing recreation and amusements of a social character. He endeavours to point out the improved prospects of mechanics' institutes in the future, and appeals to public spirit for the erection in many towns of more appropriate buildings. He suggests the propriety of establishing day schools as nurseries for the rudimentary teaching of science, and refers to the help from national and other sources that may be obtained by taking up this work systematically. He urges the vast importance of scientific knowledge for our artisans, the equal importance of a training for girls in the matters appertaining to their welfare, and points out the great need for making classes more attractive. He speaks of the value of penny banks for promoting thrift. Finally, he tries to show the necessity of making the institute more popular as a place of resort for the people of our towns, as a safeguard against pernicious attractions, and for the supply of information, entertainment, and recreation.

IN connection with Capt. Howgate's scheme of Polar exploration, the *Florence*, under Capt. Tyson, of *Polaris* fame, left New London on August 16, *viâ* Cumberland Coast, for whaling purposes. It will then proceed to Greenland to engage Esquimaux families. This pioneer party will meet at Disco, next August, the main Polar expedition under Capt. Howgate. The expedition, it is expected, will be fitted out by the United States Government as soon as Congress meets, and will have voted the required subsidies for establishing a scientific colony at Lady Franklin Bay. The use of balloons having been suggested, Capt. Howgate has written the following letter to M. W. de Fonvielle, who has offered to become a member of the scientific staff of the expedition. It is dated from the "War Department, Office of the Chief Signal Officer, Washington, D.C., August 16, 1877."—"Your valued favour of the 16th June, addressed to Capt. Tyson, was forwarded to me for reply. This reply has been necessarily delayed by the amount of work thrown upon me during the fitting out of the *Florence*. But I avail myself of this first opportunity to say that, should Congress, as I hope, legislate favourably for the proposed Arctic colony at its next session, it is my intention to try the value of balloons as an agent of exploration; and your services, so courteously offered, will be thankfully accepted. Your reputation as a man of letters and science is too well known to render such references as you allude to necessary."

THE additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus*) from North Africa, presented by Mr. E. Barclay; a White-fronted Capuchin (*Cebus albifrons*) from South America, presented by Mr. Shipman; a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mrs. Rintoul; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by the Rev. S. J. Whitmee, C.M.Z.S.; an American Tantalus (*Tantalus loculator*), two Prince Albert's Curassows (*Crax alberti*), three Black-faced Spider Monkeys (*Ateles ater*) from U.S. Columbia, a Kinkajou (*Cercoleptes caudivolvulus*) from Honduras, deposited; a Tamandua Ant-eater (*Tamandua tetradactyla*), an American Darter (*Plotus anhinga*), an Ashy-headed Gull (*Larus cirrocephalus*) from South America, a Brazilian Motmot (*Momotus brasiliensis*) from Brazil, a Sorry Thrush (*Turdus tristris*) from Mexico, purchased.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 27.—M. Fizeau in the chair.—The following papers were read:—Note on M. Lavy's catalogue of stars of longitude and lunar culmination, by M. Faye. This catalogue of 521 stars is recommended as a work of precision which should be of real service.—Two general laws of geometric curves of order and class, *m* and *n*, by M. Chasles.—On the relation which should exist between the diameter of cores of iron and the thickness of their magnetising helix, by M. Du Moncel. It is shown from experiment that there is advantage in winding electro-magnets so that the thickness of the coil-layers is equal to the diameter of the cores; and for this law to be well applied, the diameter of the cores should naturally be proportioned to the electric intensity which is to act on them, and chosen so that this intensity develop in them a quantity of magnetism pretty near the point of saturation.—On an example of reduction of Abelian integrals with elliptic functions (continued), by Prof. Cayley.—Observations of the planets 173 and 174, and remarks on the discovery of this latter planet, by M. Stephan. On August 8 Mr. Watson observed a star of the tenth magnitude not marked on his maps, but it was not till the 16th that it was recognised as a planet. M. Borrelly first perceived the star on the 10th, and next day its planetary character was ascertained; priority is thus claimed for M. Borrelly.—Provisional geographical map of the planet Mars, by M. Flammarion. In this work (commenced in 1863) the author has aimed at giving a general *ensemble* of observations from the beginning.—Observations on a recent note of M. Du Moncel, on the best conditions of employment of galvanometers, by M. Raynaud.—Reply to a recent communication of M. Angot, on the evaporation in the region of the Algerian chotts, by M. Roudaire.—On the termination of the nerves in the electric apparatus of the torpedo, by M. Rouget. In opposition to MM. Boll, Ciaccio, and Ranvier, M. Rouget observes constantly, and reproduces by photography, a network in the ventral face, formed by the divisions of the last branches of the pale ramified fibres. The appearances of termination in buttons or free extremities, which show here and there in all the preparations, are manifestly connected with the network, in the enlarged photographs, by prolongations which escape direct observation.—Variations of the temperature during the total eclipse of the moon on August 24, 1877, by M. Berigny. There was a very marked cooling at 10 o'clock, and this had a perceptible effect on the minimum. M. Berigny asks did this arise from the astronomical fact, or was it a mere coincidence? M. Faye was not disposed to attribute it to the eclipse, and for two reasons: First, the sky was that night exceptionally clear, so that the terrestrial radiation must have been very active, giving a notable fall in the thermometer; second, physicists have had the greatest difficulty in rendering perceptible heat from the moon, even with the most delicate apparatus. So that when we are deprived of this radiation for an hour or two this can hardly affect our thermometers, still less our senses.

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THURSDAY, SEPTEMBER 13, 1877

STAR OR NEBULA?

FOLLOWING close upon the publication of Dr. Vogel's paper on the new star in Cygnus, Lord Lindsay has communicated an interesting letter to the *Times* announcing the fact that the new star has now put on the appearance presented ordinarily by the so-called planetary nebulæ.

Of all the lines chronicled by Cornu and Vogel only one remains, that namely which the latter observer showed to be constantly increasing in brightness while all the rest were waning, and which, moreover, as Vogel also distinctly showed, is coincident in position in the spectrum with that observed in the majority of the nebulæ.

The observations of such rare phenomena as the so-called new stars, are of such vast importance, and will no doubt ultimately provide us with a clue to so many others of a different order, that we may well congratulate ourselves that the recent *Nova* was so well watched, and that there is such perfect completeness and unity in the chain of recorded facts.

It should have been perfectly clear to those who thought about such matters that the word star in such a case is a misnomer from a scientific point of view, although no word would be better to describe it in its popular aspect. The word is a misnomer for this reason. If any star, properly so called, were to become "a world on fire," were to "burst into flames," or in less poetical language, were to be driven either into a condition of incandescence absolutely or to have its incandescence increased, there can be little doubt that thousands or millions of years would be necessary for the reduction of its light to the original intensity.

Mr. Croll has recently shown that if the incandescence observed came for instance from the collision of two stars, each of them half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, light and heat would be produced which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

A very different state of affairs this from that which must have taken place in any of the Novas from the time of Tycho to our own, and the more extreme the difference the less can we be having to deal with anything like a star properly so called.

The very rapid reduction of light in the case of the new star in Cygnus was so striking that I at once wrote to Mr. Hind to ask if any change of place was observable, because it seemed obvious that if the body which thus put on so suddenly the chromospheric spectrum were single, *it might only weigh a few tons or even hundredweights*, and being so small might be very near us. Mr. Hind's telescope was dismounted, and I have not yet got any information as to change of position; and as I am now writing in the Highlands, away from all books, I have no opportunity of comparing the position now given by Lord Lindsay in R.A. 21h. 36m. 52s., Dec. + 42° 16′ 53″, with those given on its first appearance by Winnecke and others.

We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceeding slow.

Let us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy;—in those small meteoric masses which an ever-increasing mass of evidence tends to show, occupy all the realms of space.

In connection with this, perhaps I may be permitted to quote the following from one of my "Manchester Lectures":—

> "There is one point to which I think I may be permitted to draw your attention, although at present it rests merely upon an unendorsed observation of my own. I thought it would be worth while to try what would happen if I inclosed specimens of meteorites, taken at random, in a tube from which I subsequently exhausted the air by a pump. After the pumping had gone on for some considerable time, of course we got an approach to a vacuum; and arrangements were made by means of which an electric spark could pass along this apparent vacuum, and give us the spectra of the gases evolved from the meteorites. Taking those precautions which are generally supposed to give us a spark of low temperature, and passing the current, we got a luminous effect which, on being analysed by the spectroscope, gave us that same spectrum of hydro-carbon which Mr. Huggins, Donati, and others have made us perfectly familiar with as the spectrum of the head of a comet. There, then, we get the atmosphere of meteorites, not necessarily carbonaceous meteorites, but meteorites taken at random; and this atmosphere is exactly what we get in the head of a comet.
>
> "Now let me go one step further; and to take that step with advantage, allow me to refer to another point . . . . that whereas Schiaparelli has connected meteorites and falling stars with comets, Professors Tait and Thomson, on the other hand, have connected comets with nebulæ, both of them being, according to those physicists, clouds of stones. Now how was one to carry these spectroscopic observations into the region of the nebulæ? A Leyden jar was included in the circuit, and we had what is generally supposed to be an electric current giving us a very much higher temperature than we had before. What, then, was the spectrum; the spectrum, so far as the known lines were concerned, was the spectrum which we get from the nebulæ; for the hydro-carbon spectrum, which we get from the atmospheric meteorites at a low temperature, was replaced by the spectrum of hydrogen; the spectrum of hydrogen coming, of course, from the decomposition of the hydro-carbon, with the curious, but at present unexplained, fact that we got the spectrum indications of hydrogen without indications of carbon. In my laboratory work I have come across other curious cases in which compound vapours, when dissociated, only gave us one spectrum at a time—by which I mean that in a vapour consisting of two well-known substances, under one condition we only get the spectrum of one substance, and under another condition we get the spectrum of the other substance alone, so in others again of both combined. The evidence seems, therefore—though I do not profess to speak with certainty—entirely in favour of the ideas of Sir William Thomson and Prof. Tait on the one hand, and of Schiaparelli on the other. I note this because I shall have again to refer to the conclusion to be drawn from it, namely, that there is probably an intimate connection between nebulæ, comets, meteorites, and falling stars."

I have given the above extract to show that a mass of meteorites at a temperature higher than that found to exist in a comet's head could give us the hydrogen spectrum which was discovered with such richness in the *Nova*, and which is represented in the spectra of most nebulæ.

The *Nova* now exists as a nebula so far as its spectrum goes, and the fact not only goes far to support the view I have suggested as against that of Zöllner, but it affords collateral evidence of the truth of Thomson and Tait's hypothesis of the true nature of nebulæ.

The nebular hypothesis in its grandeur and simplicity remains untouched by these observations; the facts so far from being in direct opposition to it help us, I think, all the better to know exactly what a nebula is.

There is another point of extreme interest to the spectroscopist if we accept the bright line observed in the star by Dr. Copeland and others to be veritably the chief nebula line.

It is clear from Dr. Vogel's diagram (given in last week's NATURE) that this line brightened relatively with each decrease in the brilliancy of the hydrogen lines. On December 8, 1876, it was much fainter than F, while by March 2, 1877, F was a mere ghost by the side of it. On any probable supposition the temperature must have been higher at the former date.

Now it is well known that within certain limits the lines in the spectrum of a compound body get brighter with *decrease* of temperature, because at the higher one the compound almost entirely ceases to exist as such, and we get the lines of its constituents. It is a fair theory then to suggest that the famous nebula line may belong to a compound. Nay the fact as it stands alone further points to the possibility that the compound in question contains hydrogen as one of its constituents.

J. NORMAN LOCKYER

Craig Dhu, Kingussie, September 10

THE GLACIAL GEOLOGY OF ORKNEY AND SHETLAND

NO one can claim to speak with more authority on matters Orcadian than Mr. Laing, and few men are better fitted to judge of evidence and probabilities. His interesting letter (see p. 418 of this number of NATURE) calls attention to certain points which he regards as affording a crucial test of the value of some contending hypotheses in geology.

He asserts (1) That there is no evidence that the Orkney and Shetland Islands have ever participated in the general glaciation of Britain. (2) That these islands contain no raised beaches or marine terraces to prove any alteration of the relative levels of sea and land.

I. It would indeed be extraordinary on any hypothesis that no traces of glaciation should exist in Orkney. Could it reasonably be supposed that at a time when "the adjacent islands of Great Britain and Ireland" lay under a deep mantle of snow and land-ice which protruded even from the opposite shores of Caithness, these northern islets enjoyed a happy immunity from the cold which sealed up the more frigid south? I am afraid that on the contrary we must believe Orkney to have been in as evil case as its neighbours, no matter even if it should have succeeded in subsequently divesting itself of all traces of its wintry garb. It will not be necessary to discuss the bearing of Mr. Laing's facts upon any rival geological doctrines if it can be shown that the facts themselves do not exist. He courteously invites examination and disproof, and I think with all deference to him that I can point to evidence which when he next revisits his county will satisfy him that Orkney is no exception to the general glaciated condition of Scotland.

I have twice visited Orkney, and each time was too intent upon the curious history of the Old Red Sandstone of that region to have time to note all the features bearing upon the glaciation of the islands. But these features were too striking to escape notice, and I find in my notebooks and on my map records of the observations jotted down at the time. So far from there being, as Mr. Laing asserts, no trace of ice-work among these islands, I found them to be well glaciated and to contain in particular, excellent illustrations of (1) *roches moutonnées*, (2) boulder-clay, and (3) valley-moraines.

1. Mr. Laing mentions the granitic axis which runs north from Stromness. When he has occasion to cross it again, gun in hand, let him stop here and there on the exposed hummocks and he will find them admirably ice-worn and striated. Well-preserved surfaces of this kind overlook the wild cliffs of Yesnaby, and others, of equal clearness, occur on the slopes behind Stromness. But further examination will show him that these markings are not confined to the hard granite and gneiss. Thus on the roadside at the south-east end of the Lake of Stennis, beautifully striated flagstones may be seen, the striæ in all these cases running north-west and south-east, as if produced by a movement from the latter quarter. Nay, even among the soft yellow sandstones of Hoy, well smoothed and striated surfaces may be noticed on the summit of the cliffs near the Old Man, at a height of 600 or 700 feet above the waves of the Atlantic.

2. Unmistakable boulder-clay occurs in Orkney. It is not generally or thickly spread over the surface, as in the lowlands of Scotland, but rather, as in Caithness, lies here and there in hollows, the rest of the surface of the islands being covered with a thin argillaceous soil, derived, as Mr. Laing points out, from the decay of the underlying flagstones. A thick mass of this boulder-clay lies on the north-west side of Shapinsha, another in the sheltered hollow of Kirkwall Bay, and a third forms a notable feature on the north coast of Flota. Mr. Laing cannot but be familiar with these and other localities, and he probably refers the deposits there to disintegration of the rock underneath. Of course the boulder-clay consists here, as elsewhere, mainly of the *débris* of the rocks below, and as these rocks are flagstones, breaking up into sharp-edged fragments, the stones in the clay are very commonly more or less angular. If, however, he finds, as he will assuredly do, that many of the stones are well polished and striated along their major axis, he may be satisfied that the deposit is a glacial one.

3. So far, the evidence which I have adduced shows that the Orkney Islands participated in the general wide-spread glaciation of the adjacent mainland. But we may believe that in so northern a locality, if the form and height of the ground in any manner permitted, the lingering snows would still form glaciers on the hills, though they had retreated from the lower grounds. Now there is only one mass of high ground in Orkney—the island

of Hoy, and there, if anywhere, traces of the last glaciers should be found. Two years ago, when engaged with my friend and colleague, Mr. B. N. Peach, in making a careful examination of the north end of that interesting island, I found what we had been in search of—a beautiful and complete proof of the unconformability between the Upper Old Red Sandstone and the Caithness flags. So engrossed were we with the magnificence of the natural sections where this structure is displayed, that we had climbed into the mouth of the green corrie below the Coulax Hill before we were aware that we stood upon a glacier-moraine. But from the top of the ridge, and, still better, from the steep grassy slope on the west side, three or four successive horse-shoe-shaped mounds could be seen extending across the valley, and becoming progressively lower and shorter when traced upwards, till the last of them died out at the base of the acclivity behind. Not only were they in external form and arrangement as perfect examples of moraines as could be desired; their internal composition bore ample testimony to the same origin. My companion and I found further proof that the other valleys of Hoy had also once nourished their separate glaciers, the most striking evidence being supplied by a moraine-mound nearly half a mile long and fifty or sixty feet high, which runs across the mouth of the glen to the east of Hoy Hill on the north-east side of the island. The angular rubbish of this moraine rests upon a stiff, red, sandy boulder-clay full of striated fragments of red sandstone. The hills from which these glaciers descended rise from 1,400 to 1,550 feet above the sea. That so small and so low an island as Hoy should have had its glaciers, creeping probably even down to the sea-level, need not surprise us, when we remember that small ocean-girt groups of mountains, like those of Skye and Mull, had their glaciers, and that even in Arran, more than three degrees of latitude further south, and from hills little more than 100 feet higher than those of Hoy, glaciers existed on such a scale as to leave behind them the huge moraines of Glen Cloy.

Mr. Laing refers also to Shetland, and though he states that his acquaintance with that region is not so intimate as his knowledge of Orkney, he believes that as little evidence of glaciation can be found there as among his native islands. In this case, too, I am afraid his statements are too absolute. It is now many years since Mr. C. W. Peach chronicled the occurrence of abundant striated rock-surfaces and boulder-clays with striated stones in the Shetland Islands (see "Report" of British Association for 1864, Sections, p. 60). From my own observation also I can speak confidently as to the correctness of these determinations. Even on the low and remote westerly islet of Papa Stour Mr. B. N. Peach and myself found boulder clay and many transported blocks of gneiss, schist, and other rocks foreign to the immediate locality, while the prevailing pink porphyry showed glacial striæ running N. 5° W. On the Mainland also, between the head of Bixetter Voe and Walls, we observed some curious mounds which if not true moraines are at least parts of the glacial series. Since our visit my colleague, Mr. John Horne, has spent some time in Shetland and has obtained ample evidence of the presence of a sheet of ice over that region (see NATURE, vol. xv. p. 139). There can indeed be no doubt that both Shetland and Orkney have been severely ice-ground and that the movement of the ice has been on the whole along a north-west and south-east line. So far therefore from these islands offering any exception or difficulty in regard to this geological question they bear their independent and concurrent testimony to the now generally received doctrine.

II. There is, however, one very remarkable feature of Orkney and Shetland to which Mr. Laing has referred, and with regard to which my own observations, so far as they go, thoroughly bear out his statement. I allude to the absence of raised beaches. During the surveys which I have made in conjunction with Mr. B. N. Peach we have continually asked each other what has become of the familiar raised beaches which skirt the Scottish coast-line even as far as the shores of Sutherland. Mr. Horne was equally struck by their absence. It is indeed inconceivable that if our raised beaches be due to a rise of the ocean level from the accumulation of a polar ice-cap (a doctrine which I for one have never accepted) there could fail to be found some remnants of them among the innumerable sheltered creeks and bays of these northern islands, in positions where on the near mainland they would assuredly be found. Well-marked raised beaches skirt the north coast of Sutherland within sight of the hills of Orkney. And yet I never observed any trace of a terrace which by possibility could be made to do duty for a raised beach, either in Orkney or in Shetland, and Mr. Laing's much wider acquaintance with these islands confirms my belief that such terraces probably do not exist in Shetland, if not also in Orkney. But the difficulty of accounting for their absence is not inconsiderable, even if we hold that our raised beaches point to successive elevations of the land. Why should they cease with the northern bays of the mainland of Scotland? Can we suppose that the upheaval so marked in Sutherland did not affect Orkney?

During a recent visit to Sutherland and Caithness I tried to find some satisfactory solution to these questions. It is important to observe that on the mainland the raised beaches disappear when we pass from the crystalline rocks into the Old Red Sandstone. Travelling, for example, along the coast-line from Inverness, by the Beauly, Cromarty, and Dornoch Firths, we find ourselves, almost without intermission, upon one or other of the level sandy terraces which form so conspicuous a feature of these shores. Even upon the strip of Jurassic rocks the same platform runs on to Helmsdale. But northwards the coast rises in one long line of precipice, from which slice after slice is cut as the lines of joint split open under the influence of air and sea. I have seen no satisfactory raised beach in Caithness. The only places where, from the shape of the coast-line, the existence of such deposits was possible are in Thurso Bay, on the coast between Dunnet and Duncansbay Heads, and in the bays between Freswick and Wick. But even on these more sheltered and less precipitous shores the rock usually stands up in low cliffs and runs out in reefs, or steep banks of boulder-clay rise from the edge of the beach, or ridges of blown sand stretch for some distance inland. Now the rocks of Orkney are identical with those of Caithness; they split up into the same long lines of sea-cliff, they are swept by the same stormy seas, and washed by the same heavy tempestuous rains. Along by far the larger part of the immensely-extended coast-line of these islands no raised beach could have been formed, or, if formed, could have remained until now. So rapid is the retreat even of the solid cliffs, that both there and in Caithness a Pict's house may now and then be found, from which the outer walls on the seaward side have disappeared, together with the solid ground on which they stood, while the surge is ever breaking at the base of the cliff below. Even into the sheltered inlets the same vertical sea-cliffs often run, so that the possible localities for the formation and preservation of raised beaches are comparatively few in number. A more diligent search among these few resting-places may yet reveal the existence of some fragments of marine terraces in Orkney. In the meantime the want of raised beaches in Caithness, where, to judge from the proximity of those in Sutherland, they probably at one time existed, should put us on our guard against a too hasty and sweeping inference from their absence in Orkney.

With regard to Shetland, however, the case is far stronger. Rocks of many varied kinds form the islands of that group running out into ridges and chains of islets, and inclosing innumerable *voes* and land-locked inlets. Nowhere could there be a more admirable surface for the formation and conservation of raised-beaches. The absence of these deposits cannot therefore be accounted for except, as I am constrained to believe, on the supposition that they never existed there at all. That interrupted elevation of the land, to the pauses in which the raised-beaches point, seems to have lessened towards the north. It is still traceable by means of these terraces on the northern shores of the mainland. Evidence of it has not been detected in Orkney, though as I have said, this may not show that it did not affect these neighbouring islands. But when we recede to the far Shetlands, all trace of the former lower level of the land ceases—at least it is not preserved in lines of raised beach.

ARCH. GEIKIE

PENNINGTON'S "BARROWS OF DERBYSHIRE"

Notes on the Barrows and Bone-Caves of Derbyshire. With an Account of a Descent into Elden Hole. By Rooke Pennington, B.A., LL.B., F.G.S. (London: Macmillan and Co., 1877.)

MR. PENNINGTON has done good service to science by publishing his "Notes." The objects he describes belong to the palæolithic, the neolithic, and the bronze ages of Britain and Western Europe generally; but, following Prof. Boyd Dawkins, the author includes the entire period between the close of the palæolithic age and the earlier part of the iron age under the comprehensive name of the prehistoric ages. Moreover, to bring the eras of the archæologist into correlation with those of the biologist, he reminds the reader that during the prehistoric ages, "the animals living in Europe were generally speaking the same as those which live there now," whilst palæolithic man was accompanied by the mammoth, and many other extinct forms.

The author's prehistoric researches were conducted partly in caverns, but mainly in barrows. The latter, usually heaps of stone and turf, were either of an oblong form, or, much more frequently, "round heaps, like a basin or saucer turned upside down."

The circular barrows appear to have been in some cases nearly fifty feet in diameter, and fully five feet high at the centre. That on Abney Moor was surrounded with a rampart of earth fifty feet in exterior diameter, and having on it ten upright equidistant stones about three feet high, whilst the inclosed mound measured but twenty feet across. Almost all the barrows appear to have yielded human bones, and in some instances more or less complete skeletons, some of which occupied stone cists, whilst others did not. The body of a young man, about seventeen years of age, had the skull protected with four stones, one being a cap stone, whilst large pieces of limestone were piled irregularly round the rest of the skeleton. All the bodies found entire were in a contracted position, and there seems to have been a tendency to place them on the left side, facing north or north-westerly. Two or more skeletons were found in some cases in the same barrow, and two were met with in the same cist in a barrow on Gautriss Hill. In Siggett barrow the skeleton of a child was found very near the feet of that of an adult. Some of the barrows contained evidence of cremation; thus, in the centre of that on Abney Moor was a large flat piece of sandstone, on which human bones, accompanied by flint flakes, a chert flake, beads of jet and of amber, and a good arrow head, had been carefully deposited. There was satisfactory evidence that the funeral fire had been lighted on the spot.

Relics of water-rat, horse, red deer, roe deer, *Bos longifrons*, goat, hog, and dog were also found in the barrows, and, with the exception of the first only, commingled with the human remains. In a cist in Oxlow barrow part of a boar's tusk had been placed with the human skeleton. The horse, roe deer, goat, and dog appear to have been the least prevalent forms. On the other hand, when speaking of water-rats, the author says, "I never explored either a burial mound or a cave without finding plenty of them;" and in one instance he says "Rats came out by spadefuls."

Of articles made or selected by man the barrows yielded a cut antler, quartzite and other "foreign" pebbles, chipped flints, pottery, chert flakes, beads of jet and of amber, holed stone hammers, bone pins, arrow heads, and bronze rings and celts.

The prehistoric caves and "rock shelters" situate in Cave Dale, Hartle Dale, and Creswell Crags, contained, with the exception of roe-deer only, remains of all the barrow animals, and of wolf, fox, shrew, badger, cat, hare, rabbit, duck, and fowl, in addition. They also yielded flint flakes, a holed sandstone hammer, charcoal, pottery, some of which was Roman, a cut stag's horn, a bone comb, pieces of jet, a celt and some ornaments in bronze, a few iron articles, and a coin of Hadrian.

When speculating on his discoveries the author remarks of the skeleton of the youth supposed to be about seventeen, that the people who buried him must have been "actuated by some other feelings of respect than those springing simply from personal valour or wisdom. This boy must have been of some rank; possibly the eldest son of the chief. The rudiments of government and of hereditary station seem to have existed, for it was not every person who was honoured by so large a cairn."

The contents of the long-shaped barrows differed from those of circular outline. The former contained neither metals nor burnt bodies; all the human skulls were long or "boat-shaped;" and the barrows seemed of higher antiquity than the others.

The neolithic and the bronze people had similar customs; each disposed of their dead by cremation, as well as by burying some of them entire and in a contracted position; each used polished stone celts, and jet and amber ornaments; each made coarse pottery, and ornamented it with the same rude designs; and during each period the skulls of some of the people were long and narrow, whilst those of others were round.

The evidence of infanticide, slaughter of slaves, and cannibalism during the prehistoric ages is thought to be too conclusive to admit of doubt.

We must content ourselves with a brief mention of the Palæolithic "finds" described by the author. He first found bones in 1870 in Windy Knoll quarry, near the northern part of the mountain limestone of Derbyshire. Aided by Mr. Tym he began systematic work there in 1874, and Prof. Boyd Dawkins joined them in 1876. A cavity in the rock—not a cavern—proved to be crammed with remains of grizzly bear, wolf, fox, water-vole, shrew, bat, bison, reindeer, roe deer, hare, and rabbit. Omitting vast numbers of mere fragments, there were more than 3,500 bones and teeth of bison, of which a large number were calves; 1,200 specimens of reindeer, also including calves, but in a lower ratio; and sixty canine teeth of grizzly bear—the only ursine species met with. The remains varied much in their state of preservation, but a very large number were perfect, and many were in their proper relative positions.

The history of the "find" was probably this:—"A swampy place was resorted to by the migrating herds of bison and reindeer. The overflow would escape into the 'water-swallow' hard by, a precipitous place into which animals might and did fall." There was no trace of mammoth, rhinoceros, hyæna, or man.

A fissure in a mountain limestone quarry at the Staffordshire village of Water-houses yielded, in 1864, remains of mammoth, hippopotamus, and rhinoceros, about twenty feet below the surface of a deposit of loam and angular fragments of limestone, and containing a number of quartz pebbles. In 1873 relics of bison, horse, and wolf, were met with in a prolongation of the same fissure, but at not quite so low a level. The bones were probably all of them those of animals which had fallen in.

Mr. Pennington has increased the value of his book by giving an account of the Rev. Mr. Mello's discoveries in the caverns of the Permian formation at Creswell Crags, on the confines of Derby and Nottingham shires. Mr. Mello began his researches in 1875, and in 1876 an exploring committee was formed, who have thoroughly examined the Pin Hole, Church Hole, and Robin Hood Caves. In the last the deposits were, 1st, or uppermost, soil containing Romano-British relics; 2nd, breccia; 3rd, light-coloured "cave-earth;" 4th, a mottled bed; and 5th, or lowest, red sand. Remains of extinct animals occurred in the lowest three, and included *Machairodus latidens*, cave lion, leopard, wild cat, cave hyæna, wolf, fox, Arctic fox, glutton, grizzly bear, brown bear, pole-cat, water-vole, mammoth, woolly rhinoceros, horse, bison, reindeer, great Irish deer, wild boar, and hare. We observe, however, that Prof. Boyd Dawkins does not mention the Arctic fox, or the glutton, or the wild boar as amongst the "finds" (see *Quart. Journ. Geol. Soc.*, No. 131, pp. 590, 602). The remains of hyæna were very numerous, and the condition of the older osseous relics betokened that at least most of them had been introduced by him. The author is of opinion that the fauna was Arctic or north temperate.

The lower deposits contained large numbers of broken and chipped fragments of quartzite, which must have been derived from a distance. Flint flakes occurred in hundreds, and of all sizes and forms, in the upper layers, where quartzite fragments were but few. Scrapers and lance-points were the commonest of the flint tools. Bone implements were also met with, and included a needle and a pin or lance-head, &c. There was also a sketch of a horse on a piece of flat bone—the first, and, up to the present time, the only example of palæolithic fine art found in Britain. The explorers also met with a piece of amber and bits of charcoal, and found reason to believe that the hare was largely used as food. The amber does not appear to be mentioned by Prof. Dawkins.

Our limited space forbids us to follow the author through his interesting speculations on palæolithic anthropology; but we cannot help doubting whether the exploring committees of the caverns near Settle and Torquay will endorse his opinion that "no caverns in this country have furnished such a variety of evidence as to ancient man and the animals which furnished him with food and clothing" as those of Creswell Crags. Those of us who at the close of the Plymouth meeting of the British Association, visited the caverns at Brixham and Torquay, and noted that they almost overhang the sea, cannot but regard the author's proposition that "the palæolithic cave dweller of England was an inlander" as being much too sweeping.

Finally, whilst perusing the volume which we now close reluctantly, we have again and again caught ourselves wishing that anthropologists would supply us with good definitions of "savage" and "barbarian," and tell us whether the words are to be used as synonyms.

OUR BOOK SHELF

Mathematical Questions, with their Solutions, from the "Educational Times." Edited by W. J. C. Miller, B.A. Vol. xxvii. from January to June, 1877. (London: C. F. Hodgson and Son.)

JUST fifteen years ago we became aware, by the chance sight of a copy of the *Educational Times*, of the existence of a paper which gave up three or four pages monthly to the proposal and solution of mathematical questions. We at once sent to England, and a more careful examination of the copy we received showed us that it was a publication of very high merit, at least as regarded this one department. Hitherto we had in the main confined our mathematical reading to the usual rut passed over by mathematical masters who have only to do with the teaching of ordinary boys; now we were induced to join the, at that date, small band of contributors who rallied round the mathematical editor and derived much pleasure and profit from the study of the many elegant solutions which were constantly being given. We have the number for October, 1862, before us, in which are Questions 1312 to 1320 proposed under six different names; we turn to the number for the current month, and the questions range from 5387 to 5419 from as many individual proposers, whose names are given. Here is evidence that a want has been met, and that there is considerable vitality in this direction; indeed, we may remark that this is the sole English periodical (since the demise of the *Lady's and Gentleman's Diary*) to which mathematicians can send high-class problems. University and college examinations swallow up a great deal of what is produced by residents at the universities, but these pages are open to all comers.

It soon occurred to us that here was a great mass of useful work being done and yet not producing the full benefit it might do if it were reproduced and published in a separate volume. The editor at once fell in with our views; indeed we found that the like idea had occurred to himself. There were, however, supposed pecuniary and other difficulties to encounter, but at last these were got over and the work, after one volume had been published, took its present form, which is now a conspicuous one on many a student's shelf. The fact that now their solutions would be treasured up in this more desirable shape seems soon to have led our foremost mathematicians to give in their adhesion, and as we run over the long list of contributors prefixed to the volume before us, there is hardly a name familiar to us which is not to be found there. France, Italy, and America also, are fairly represented. Ladies, too, there are, showing that

> "the gay determinant
> For (them) its rows exchanges,
> While Hamilton's weird delta turned (▽)
> O'er all the symbols ranges."

It says very much for the ability, in more directions than one, of the editor, that he has nursed the bantling which was handed over to his care more than sixteen years ago into the vigorous and lusty athlete of to-day. Nothing mathematical comes amiss to his net, but we may say that though the Dii majores roam about in their own special pastures, he has a marked predilection for the line taken up and well-worked out by Messrs. Woolhouse and Crofton, *i.e.*, of probability in its many applications.

It only remains to say that the "Reprint" is more than a reprint, for it contains about as much more original matter as appears in the monthly paper. Space is found for detached papers and notes, and for alternative solutions, often of equal, if not greater, interest, than the previously published matter.

There are occasional parenthetical notes—we think it should be more clearly indicated who is responsible for these, as they are often valuable ones.

The training the printers have gone through in getting out these solutions has placed them on a high level as printers of mathematics, and the volumes of this series reflect great credit upon them.

Cronicon Cientifico Popular. Por D. Emilio Huelin. Vol. I. (Madrid: 1877.)

WE perused this volume with interest and pleasant surprise; we were pleased at finding it to be an excellent and well-written review of all new occurrences in the scientific world, and we were surprised to see such a work emanate from a country which hitherto has contributed but too small a share towards the progress and welfare of science. If we place Turkey at the head of the list of the most unscientific countries in Europe, Spain and Portugal certainly come second on that list; it is gratifying, therefore, to see some sign of improvement. We congratulate Senor Huelin on his valuable publication, which is one of the best of the kind that has yet come under our notice. The arrangement of the contents of the volume is particularly good. The first few chapters are dedicated to generalities and the philosophy of sciences; some of them contain detailed lists of all scientific publications in the world. Then follow numerous chapters relating to the latest discoveries, inventions, theories, &c., on the domains of physics, chemistry, astronomy, meteorology, mineralogy, and geology; the chapters of the physical section alone numbering no less than eighteen, and those of the chemical section as many as twenty. Any occurrence at all worthy of record up to the end of last year is faithfully mentioned in the book. The second volume will contain the biological and mathematical sciences. We wish Senor Huelin and his publishers every success with their valuable addition to scientific literature.

Die Naturkräfte. Band 21. "Die Insekten" (1st part): "Der Organismus der Insekten." With 200 original Woodcuts. By Dr. Georg Mayr. (München: R. Oldenbourg, 1877.)

THE importance of an examination of the internal as well as the external anatomy of insects has unhappily not hitherto engaged the attention which it deserves at the hands of British entomologists. It is a fact which cannot be disputed that by far the greater portion of that energy which our country has exhibited in the investigation of this branch of natural science has been devoted to the mere founding of types, and in consequence but little light has been thrown upon the ever-increasing array of problems which puzzle the biologist.

In studying the affinities of insects it is quite as important, and in all probability more so, that the internal structure and the embryology of insects should be known, as the external characters and the metamorphoses; it is therefore with unmixed pleasure that we welcome the appearance of Dr. Mayr's admirable and ably-illustrated treatise.

It would be impossible here to give even an outline of the vast series of facts which the learned author has brought together, nothing relative to the organism of insects being regarded as too insignificant for careful and unwearied research; as an instance of the thoroughness of his labours we would especially call attention to his interesting observations on the action of the legs of insects when walking, a point which he seems thoroughly to have studied and which he has amply illustrated, although many students would probably have regarded it as a matter of little moment. In fine, the entire volume is most valuable, and should be esteemed as a necessary hand-book, not only by every entomologist, but by all who have the interests of natural science at heart.

A. G. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Glacial Geology of Orkney and Shetland

A RECENT visit to Orkney has brought forcibly before me certain points of the highest interest in modern glacial geology, upon which, I believe, the state of the surface deposits in these islands is calculated to throw considerable light.

I may premise that although I am perfectly well acquainted with all the usual glacial phenomena of the North of Scotland, as described in Geikie's and other works, I am not a sufficient practical geologist to speak with positive certainty, though I think I know enough of the subject to establish a *primâ facie* case for what I have seen with my own eyes, and which I put forward in the hope that more competent observers may direct their attention to the subject and either confirm or disprove facts which, if true, would seem to afford a crucial test of the truth or falsehood of some of the most important theories of modern geology.

The fact which I assert is, that there are no traces of glacial action, or of raised sea-beaches in Orkney.

I speak from an intimate personal acquaintance with these islands, which are my native county, and almost every yard of whose surface and shores I have explored with rod and gun, and in the course of canvassing at elections, and for many years back keeping a special eye on this very subject. Now I can assert positively that I never saw a boulder or perched block, or the trace of any till, boulder clay, kame, eskar, raised beach, or other form of glacial or marine action.

The whole of the islands consist—except a small patch of granitic axis—of Devonian strata, bare in places, but for the most part covered with a mantle of soil, which is the obvious result of the disintegration of the subjacent rock by existing subaërial causes, such as wind, frost, and rain.

In places, where soft strata come to the surface, this soil is deep and clayey, and the sections of it, afforded by the coastline, might readily be mistaken at a distance, or by a superficial observer, for boulder-clay. But a close examination will show that the stones in this stony clay are always angular and always similar to the adjoining strata, and that the larger stones are generally deposited, allowing for subsidence and displacement, in the original lines of stratification conformable to those of the unworn rock below them. A good example of this may be seen within 200 yards of Kirkwall, on the east side of the bay under Cromwell's old fort.

Let any one compare this with the section of glacial boulder-clay shown on the other side of the Pentland Firth at Scrabster, and he does not require to be a geologist to understand the difference between a surface soil of glacial deposit and one of disintegrated rock.

In like manner I have observed innumerable sections of surface soil and of mounds and ridges, which at first sight might have passed for marine or glacial, and I have invariably found them to consist of angular fragments of the subjacent rock passing on the one hand into thoroughly decomposed rock or soil, and on the other into the solid strata on which they rest.

I believe I may state broadly that there is not a rolled or rounded stone or pebble, or trace of sand or gravel, in all Orkney above the level of the present sea-beach and blown sands, and away from the beds of the existing lakes and small streams.

There is not the vestige of a raised beach along the hundreds of miles of rocky coast of the various sounds and islands, or in the many sheltered inlets where, in the nearest counties of Scotland such as Sutherland, Ross, and Cromarty, raised beaches are invariably seen. All recent movements seem to have been movements of subsidence and not of elevation. The Loch of Stennis, with its surrounding plain, affords conclusive proof that at no recent geological period can the level of the sea have stood higher relatively to that of the land than it does at present. Had it done so the Loch of Stennis, which is now exactly level with the sea so that the tide flows into and out of it, must inevitably have been a sheltered inland fiord of salt water extending to the hills which bound the plain, which as the land rose or the sea retreated, must have left the plain covered with sand, shingle, and marine or brackish shells, of none of which is there the slightest trace, but, on the contrary, the ordinary rock strata with their disintegrated surface soil, occupy the whole plain and come up to the margin of the existing loch.

Now as to the inference from these facts.

The received theory of most glacialists is, that during the glacial period there was a great polar ice-cap extending over the whole of Scandinavia, Scotland, and a great part of England and Ireland. As a corollary of this many draw the inference that such an accumulation of ice, by displacing the earth's centre of gravity, would raise the level of the sea in the Northern hemisphere, and thus account for the higher levels relatively to the land at which it has undoubtedly stood.

Others contend that the glaciation was more limited and only extended in islands as it were, round each considerable mountain group in northern latitudes, and these attribute the phenomena of raised beaches, &c., to local elevations of the land rather than to general elevation of the sea.

Now here appears to me to be an opportunity of applying the *experimentum crucis* to these two conflicting theories.

If it be true that Orkney is not glaciated, and has no raised beaches, it seems to follow that the second, and not the first, of these theories must be the true one.

The second theory would account perfectly for the boulder-clay being found in Caithness, over the plain of which we may easily suppose the glaciers from the great mountain range which bounds it on the south and west, to have extended as far as Scrabster and the south shore of the Pentland Firth, while in Orkney there were no glaciers, because there was no great local mass of mountain region to produce them.

But, on the theory of a great ice-cap, I cannot see how Orkney could fail to have been planed by ice and covered by boulders, perched-blocks, and masses of glacial clays, sands, and gravels.

In any case the absence of raised beaches and of all traces of marine action above the present sea-level, seems to be inconsistent with any theory of a general and uniform rise of the ocean in these latitudes.

As regards the Shetland Islands I cannot speak with the same confidence, not being so intimately acquainted with them; still, having travelled over a great part of the principal islands, and coasted along their shores, I can assert that I have never seen any traces of glacial action, or of raised beaches. The latter must, I think, inevitably have shown themselves in the form of sea-caves at a higher level, such as those at Cromarty, had they ever existed, as the present line of exposed rocky coast is worn by the waves into innumerable caves and clefts.

As to boulders or boulder-clay, I do not believe they exist, and the only rounded or water-worn stones I have ever seen have been rolled in the Devonian and not in any modern seas, and result from the surface disintegration of the great conglomerate.

These are abundant in exposed situations, and they show the necessity for care in inferring modern glacial or marine action from the presence of rolled stones of foreign rocks.

In conclusion, I believe that these groups of islands, Orkney and Shetland, have never been subjected to glacial action or submerged and subsequently elevated, in any recent geological period, and that these facts are inconsistent with any theory of a great polar ice-cap, or of any uniform rise of the level of the ocean in northern latitudes.

S. Laing

Brahan Castle, Dingwall, N.B., August 25

Meteorological Effects of Eclipses

In connection with certain variations of temperature observed during the total eclipse of the moon on August 24, 1877, by M. Berigny, and discussed at a late meeting of the French Academy of Sciences, as reported in Nature (vol. xvi. p. 412), I am reminded of some observations made on board H.M.S. *Challenger* during the total eclipse of the sun on April 6, 1875. The position of the ship at noon of the day of the eclipse was in lat. 27° 13′ N., long. 137° 59′ E. about 400 miles south of Japan and 200 miles due west of the Bonin Islands. If my memory be correct, the eclipse was only partial for the part of the world we were in, a portion of the sun's disc being still visible in the shape of a thin crescent at the moment of maximum obscuration. The eclipse, occurred in the afternoon, and was heralded by a breeze from the south-west, which continued during the rest of the evening; but what at the time struck us as very remarkable was the fact that it was accompanied by a rise of the surface temperature of the sea, as will be seen from the following observations made at the time:—

April 6, 1875.	Temperature of sea-surface.	
4 A.M.	20·3° C.	
10 ,,	20·9 ,,	
Noon to 3 P.M.	21·1 ,,	Time of the eclipse.
4 ,,	22·5 ,,	
5 ,,	22·2 ,,	
6 ,,	21·9 ,,	
7 ,,	20·9 ,,	
8 ,,	20·0 ,,	

The *Challenger*, progressing at the rate of about three knots per hour, had just entered an area of alternate streaks of warm and cold water, the former due to the North Pacific equatorial current, known as the Kuro-Siwo or Japan current, the latter to the Arctic current which flows down off the east coast of Nipon, so that the observed rise of temperature, and perhaps also the south-westerly breeze which sprung up at the commencement of the eclipse may be a mere coincidence, and I give the observations for what they are worth.

During the discussion in the French Academy the theory suggested by M. Berigny that an eclipse of the moon might have an appreciable effect upon the temperature-conditions of our atmosphere, namely, by cooling the latter, does not seem to have met with much encouragement. It would be more extraordinary if such a phenomenon as a total eclipse of that luminary were found to exercise no disturbing influence of any kind upon the terrestrial atmosphere during the time of its occurrence.

J. J. Wild

The Development of Batrachians

In reference to the article in Nature (vol. xv. p. 491) on the development in certain instances of Batrachians without metamorphosis, Mr. B. G. Wilder writes to the *American Naturalist* (vol. xi. No. 8, August, 1877, p. 491) to point out that the author of the article in Nature has overlooked Prof. Wyman's observations on the development of *Pipa americana*, published in the *American Journal of Science and Art* for 1854 (ser. 2, xvii. pp. 369-374).

Wyman has there stated that the eggs of *Pipa* are transferred by the male to the back of the female, which presents "a uniform surface throughout." "Their presence excites increased activity in the skin, which thickens, and is gradually built up around each egg, so as at length to inclose them in a well-defined pouch." On pages 370 and 371 he figures and describes the earlier embryos as having "three branchial appendages on each side of the head." "In a later stage the external branchiæ had disappeared, but a small branchial fissure was detected on each side of the neck, and within this on each side a series of fringed branchial arches."

In endeavouring to obtain some confirmation of Prof. Wyman's observations, Mr. Wilder suggested an examination of certain eggs of this singular Batrachian preserved in the Warren Anatomical Museum of Harvard University. The examination was made by Mr. C. S. Meriot, who reported as follows:—

"I have examined two eggs from the back of the *Pipa*, and found the embryos a little more advanced than that figured by Prof. Wyman; they are between 12 and 13 mm. in length. The gills were partly absorbed, but a single slit with the gills still projecting could be readily seen on each side at the back of the head. I could not make a more detailed examination, as the eggs were not well enough preserved."

It would therefore appear that Dr. Peters' remark (which was translated in the above-mentioned paper in Nature on this subject) that "no one has detected branchiæ in the embryo of the Surinam toad" is not well founded. It would be very desirable, however, to have further observations on this interesting subject made, as likewise on the development of *Hylodes martinicensis*, which was the principal subject of the former communication to Nature.

P. L. S.

Notes on the North East Australian Monotremata

English naturalists seem to be "all abroad" on this subject, judging from some remarks in Nature, vol. xv. p. 257.

P. L. S. states his conviction that Tachyglossus will be discovered in the ranges of N. Queensland when these have been properly examined, being evidently quite unaware that both Tachyglossus and Ornithorhynchus have been known to inhabit the northern part of this colony for several years, and that a discussion has been carried on for some months in the columns of the *Queenslander*, on the "Generation of the Echidna and Platypus," between Dr. Bancroft and Mr. Bennett. I merely allude to this subject to give English naturalists the latest discoveries made by Dr. Bancroft in his researches into this more than usual *prickly* subject.

Dr. Bancroft dissected a female specimen and found a quantity of milk in the stomach. He was unable to find any mammary glands, and came to the conclusion that the mother Echidna fed its young by regurgitation. This theory was combated by Mr. Bennett.

On dissecting a second female Dr. Bancroft discovered the mammary glands, but not like those of any other known mammal, for the nipples were inverted, the cavities thus formed being protected by stiff bristle-like hairs.

The young Echidna has to thrust its bill into the cavity to obtain its food. This is a curious adaptation to the wants of an animal, for it is certain, from the curious formation of the head and rostrum, that it would be impossible for the young to suck a nipple; as it is the forcing in of the bill expresses the milk which it is enabled to suck in when lying in the bottom of the cavity.

I secured a specimen of an adult female having a fine healthy young one in the pouch, and preserved both. On opening the stomach I noticed a quantity of a white substance which seemed to be inclosed in a thin membrane. I did not know what this was until I read Dr. Bancroft's letter on the subject, but unlike him I arrived at a different solution of the puzzle. Instead of the females sucking themselves (the way in which Dr. Bancroft accounts for the presence of milk in the stomach), I believe that, after a certain time, when the spines begin to grow on the young Echidna, the irritation causes the mother to take it out of the pouch, and to feed it by regurgitation, until its spines are sufficiently grown to protect it from its enemies. I was led to this belief by discovering a nearly adult male with no sign of any food in its stomach except milk.

I was encouraged to dig this individual out of its stronghold, by noticing several unusually large tracks going in and out of the burrow. These, undoubtedly, were the footprints of the female when she came to feed the young. The spines on this specimen were not at all stiff, especially at their bases, and would have offered little or no protection against the attacks of a native dog or eagle. This may seem a wild proposition, but I have only adopted it after mature thought, and observing four different females.

We must adopt some explanation to account for so strange a phenomenon, unless we jump the question by allowing that the females of Tachyglossus suck themselves, a most unlikely proceeding on their part, and a solution to the difficulty as unsatisfactory as it is improbable. We have only to guard against chronicling false facts which seem to me to be the greatest enemies which science has to contend with, and we are not so apt to go wrong. Mistakes are often made, but invariably meet with a correction, and should the above solution to a difficulty which has taxed, and is still employing, some of our wisest heads, prove wrong, I shall be only too glad to accept the correction, thankful that my blunder has assisted in rightly solving an interesting and vexed question. The blacks inform me that the Echidna lays a white egg, and the Platypus a black one, which are hatched in the abdominal pouch.

I trust that some of your many scientific contributors will come to our rescue in solving this point. W. E. A.

The late discoveries by Goldie, D'Albertis, and MacFarlane, have produced a number of forms (botanical) identical with those obtaining in N.E. Queensland, thus further proving the original fundamental unity of the two countries. The Australian Dingo ought to be found in New Guinea as also our tiger cat.

W. E. A.

On Time

I have deferred my answer to the remarks of Mr. J. J. Murphy (Nature, vol. xvi. p. 182) till now, in order to see if my letter of June 14 would cause more discussion. But it seems that my views are not deemed worthy of much consideration in England; I shall therefore say only a few words in conclusion here, but hope to take up the question elsewhere.

Mr. Murphy thus summarises a part of my letter:—"The postulate that a velocity, *e.g.*, that of the earth's rotation continues unchanged, is arbitrary, incapable of proof, and justified only by practical convenience." I should wish that he had added, "and to be settled by definition." Mr. Murphy goes on to say, "It seems to me, on the contrary, that the postulate is not necessarily arbitrary but may be absolutely justified by fact." I do not believe there is a great difference between "justifying something by practical convenience" and "justifying it absolutely by facts." Perhaps, in the opinion of Mr. Murphy, the constancy of the velocity of the earth's rotation is proved by the pendulum. But it is universally admitted that the pendulum is controlled by the earth and not the earth by the pendulum.

Mr. Murphy, in admitting that he sees no way of proving that the force of gravitation continues unchanged, acknowledges that in natural philosophy many things are taken for granted which call for closer consideration, and this was precisely my motive for writing my letter.

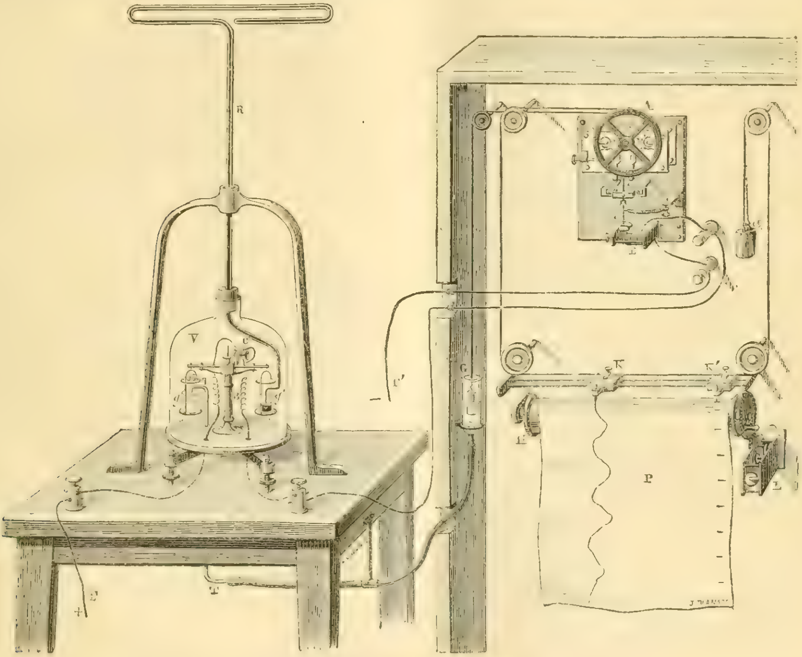
Though Mr. Murphy does not agree with me in all points I am thankful for his remarks. V. A. Julius

Breda, Holland, August 29

A NEW REGISTERING THERMOMETER[1]

THE registration of temperature is one of the most difficult of meteorological problems. Among the registering instruments employed the thermometer is certainly that to which most attention has been devoted, and yet no solution has hitherto given results altogether satisfactory. The extreme mobility of the temperature of the air and the small force at our disposal for acting upon the registering apparatus, are special hindrances to the solution of the problem. In England, in the various observatories, the photographic method is used. The reservoir of the thermometer is placed outside under cover, and the tube, entering the wall, is re-curved vertically in the interior; a photographic apparatus placed opposite this column of mercury registers the different heights. This process necessitates a thermometric reservoir of considerable volume in order that the displacements of the column of mercury may be appreciable for very small variations. These exigencies affect the sensitiveness of the apparatus; it is not a less serious inconvenience that the reservoir must be placed near the wall of the shelter where the self-recording photographic apparatus is arranged. In Switzerland the metallic thermometer is employed, and is more easily managed, but here again the metallic spiral must be placed very close to the registering apparatus.

The new registering thermometer which M. Hervé Mangon has sought to construct by utilising the differential wheelwork of M. Redier, seems to us based upon a sound principle. It consists of a mercurial thermometer with weights so arranged that the thermometer may be placed at such a distance from any dwelling as not to be subject to the influence of surrounding objects. Communication between the thermometer and the registering apparatus is established by means of electricity.



M. Hervé Mangon's New Registering Thermometer, constructed by M Redier.

The instrument consists of two quite distinct parts:—1. The thermometer proper, and the balance which serves to indicate the differences of weight which are the result of variations of temperature. 2. The registering apparatus. The thermometer, the diameter of the mercurial column of which we have considerably amplified in our illustration, to render it appreciable, is composed of a very fine tube, R; it presents a large surface, containing in reality only a very thin column of mercury. This tube R is supported by a cast-iron frame-work, and is connected with a bell-glass V; its very slender extremity is plunged into a small cup *g'*, containing mercury, and placed upon one of the scales of the balance B.

The balance B is an ordinary balance of precision; it bears above the beam a small metallic disc which determines a contact at C every time the equilibrium is broken in consequence of an increase of temperature. The second scale also bears a cup *g*, containing glycerine. A glass tube T T, connected with the registering apparatus, dips into this cup *g*, and communicates at its other extremity with another cup, G, which forms a vessel communicating with the former. The bell-glass V covers the balance, and permits the exposure, without danger, of that part of the instrument to the inclemencies of the air. It will be at once seen how to arrange things in order to put the instrument in working order. After having fixed in its support the tube filled with mercury, and being assured that the fine extremity dips well into the cup g', we place the balance in equilibrium by adding weights to the other scale. The bell-glass V, which has been raised for this operation, is then put in its place, and the instrument is ready for action.

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[1] From an article in *La Nature* by M. Gaston Tissandier.

The registering apparatus is composed of the double differential wheelwork of M. Rédier, which works as follows:—Two wheels M and M′ moving in opposite directions, are terminated by small flies, very delicate, and turning very swiftly; they are connected by a differential train, the axis of which carries a pulley with a double groove A. Between the two flies oscillates a needle, one extremity of which serves to arrest alternately one of the two flies. At the other extremity a, of the needle, is a soft-iron pallet on which acts an electro-magnet E, every time contact is made by the balance at C. The needle is mounted on an axis which permits it to oscillate right or left according as it obeys the electro-magnet or a small antagonistic spring.

The double-grooved pulley A carries two threads the one attached to the pencil K and terminating in a weight O, the other bearing a small cylinder and plunging into the cup G containing glycerine and connected by a tube with the cup g placed on one of the scales of the balance. Of course the cups G and g must be placed in the same horizontal plane. A cylinder H, moved by the clockwork L, carries the paper. A second pencil K′ serves to trace upon the paper a small mark intended to control the progress of the wheel-work L. This mark may be made automatically by an electric contact proceeding from a regulator of precision.

Let us see now how things work. Suppose the temperature rises (the explanation which follows will account for the mode of action of the apparatus when the temperature falls), the weight of the mercury in the cup at g' will increase, the equilibrium will be destroyed, and the contact C of the balance will be established; the electro-magnet E will attract the end a of the needle, and the fly of the wheel M′ will be free; the pulley A will then turn to the left, the float will sink in the cup G, and the pencil will be directed towards K′. The float of the cup G, in descending, will raise the level of the liquid at G, and at g, and consequently will increase the weight in the scale of the balance which holds the cup g, and at the moment that equilibrium is thus again established, the contact at C will be broken. The end a of the needle ceasing to be attracted by the electro-magnet will respond to the appeal of the antagonistic spring, and disengage the other fly. This fly of the second wheel M disengaged, permits the pulley A to turn to the right, drawing the pencil from K′ to K, and causing the float of the cup G to reascend, and consequently diminishing the weight at g. That loss of weight breaks anew the equilibrium of the balance, the contact at C is re-established, and the same course is repeated as we have explained above.

It will be seen from what we have said that the clockwork is always in motion—now to the right, now to the left—even when the temperature does not vary; the curve obtained has then the aspect of a small zigzag, but so fine that it is difficult to detect it. This arrangement permits, so to speak, the double wheel M M′ to test the balance for the slightest change in the conditions of equilibrium.

The tube T T connecting the two cups G and g may be placed underground, and the electrical communication between the balance and the electro-magnet E is easily established at any distance desired.

On the prolonged axis of the pulley A we may place a rigid needle, and indicate by a simple transmission the temperature on a large card placed outside.

This apparatus has been constructed in a thoroughly artistic manner by the able constructor, M. Redier.

NEW ELECTRIC LIGHTS

AN examination of the voluminous records of the Patent Office discloses the fact that the activity in a particular line of invention periodically waxes and wanes. After slumbering for a number of years the problem of procuring effective electrodes for the production of the luminous electric arc has of late been revived, and with a success hitherto unattained. The immediate cause of this has probably been the recent improvements of magneto-electric machines culminating in the Gramme and the Siemens machines. An efficient source of electricity for the production of the light having been supplied by these and other machines of a similar kind, a stimulus was given to the invention of electrodes or *wicks* which would employ the magneto-electric current to best advantage in giving out light. The old faults of the carbon points had never been quite overcome. The manufacture of the points from soft-wood charcoal, fine coke dust, lamp-black, calcined sugar, tar, resin, or mineral oil, &c., had done much to render their consumption steady and uniform; and the regulators of Serrin and Dubosq had very successfully overcome the widening of the luminous arc by the wasting of the positive electrode. For large fixed lights with several sets of luminous points, such as are employed as beacon-lights on land or at sea, the ordinary carbon points thus improved answered very well, but for the purposes of general illumination they are still defective. To give a light suitable to a room or hall the points require to be small, and any inequalities in their action are very discernible in the light. One great difficulty to be overcome, too, is the division of the light. How to cause the current from a powerful magneto-electric machine to produce a number of separate small lights, such as would be essential for the lighting of streets or buildings? If the different lights were all joined up "in circuit" and the currrent sent through the whole series one after another, the break-down of any one of the series would extinguish the whole and plunge the street or building into darkness.

During the last thirty years there have been many attempts made to secure good electrodes for the electric light as well as devices for adjusting them. Electrodes of spongy platinum, palladium, and iridium have been used. Another plan was to make the positive electrode a fine stream of mercury flowing from a funnel and breaking upon a negative electrode of carbon or platinum placed underneath. An objection to these metals was the coloured lights they produced owing to the incandescence of their vapours in the arc. The carbon electrodes were given divers shapes, and various combinations of carbon and metal electrodes suggested. For instance, it was proposed to use bar electrodes emitting the light from their sides, and also to fuse iridium between two carbon electrodes. An ingenious plan for getting a steady light was proposed by Mr. Harrison in 1857. It consisted in giving a rotary motion to the positive electrode and pointing the negative electrode at right-angles to it and giving the latter a motion of translation, so that fresh surface of the positive carbon was always appearing in front of the negative carbon. A similar idea was again patented in 1874 by Messrs. Wildman and Whitehouse. About twenty years ago there was a great deal of activity in this direction, but up till quite lately the usual carbon points have always been fallen back upon.

Within the last five years, however, two notable new lights have made their appearance, namely, the lights of Lodighin and of Jablochkoff. M. Alexandre Nicolævitch Lodighin is a Russian engineer of St. Petersburg. His plan was first publicly tried there in 1873, and patented in England in the previous year. It is designed to facilitate the use of the electric light for general lighting purposes. The great defect of the ordinary carbon points is the flickering of the light caused by the consumption of the carbon points, a great portion of which is due to the combustion of the points in the air. M. Lodighin's plan is to employ not two but a single stick of carbon, inclosing it in a hermetically sealed glass chamber from which all air has been exhausted, and an azotic gas which does not combine with carbon at a high temperature, such as nitrogen, let in. When the current from a magneto-electric machine, such as Wilde's, Gramme's, or Noble's, is passed through this carbon it gradually gets heated to a white heat, and emits a brilliant, and at the same time soft and steady light. Fig. 1 shows the form of the carbon used; the light is given off at the narrow central part. The advantages of this plan are that there is a continuous circuit, so that any number of lights may safely be joined up in series to form one or more lamps. The lights can be made as small as desired, the flame is continuous and not injurious to the eye, the cost of new carbon points is saved, and the current can be strengthened or weakened at will very easily. It burns equally well under water, and would be very useful for illuminating dangerous mines, there being no fear of explosion from it. One magneto-electric machine driven by a 3 horse-power engine, generates a light equivalent to many hundred lanterns, and the light can be easily divided up into smaller ones. There was one defect in M. Lodighin's original light which has been remedied by M. Kosloff, of St. Petersburg. The unequal expansion of the metal holder of the carbon and the carbon itself caused the latter to split and give way. The metal also fused, and sparks passed between the carbon and the expanded sockets. Kosloff fixed the carbon on insulating supports of china, clay, crystal, &c., and connected it in circuit by wires. The improved light of Lodighin and Kosloff was first tried in London in 1874, and was very successful. It was awarded the Lomonossow Prize by the Russian Academy of Sciences.

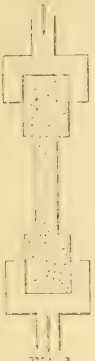

Fig. 1.

But the "electric candle" of M. Jablochkoff has, for the nonce at least, quite cast Lodighin's light into the shade. It appears to be one of those lucky inventions crowning a long series of more or less unsuccessful ones in the same direction. In the electric candle the two carbon points are not dispensed with. They are placed side by side and separated from each other by a slip of an insulating substance such as porcelain, brick, magnesia, but preferably kaolin or pure clay. One of the points is a little longer than the other, and may also be stouter. The positive current is passed down the longer carbon, and leaps across the air space to the shorter carbon, forming the luminous arc at the point of the candle. Such an arrangement of the points is shown in Fig. 2. It is called a candle because it can be burned upright in a support like a candlestick. The kaolin plays an important part besides insulating the carbons from each other. It becomes incandescent, emitting a beautifully soft, steady, light, and melts away like wax at the same rate as the carbons, just as a candle is consumed with the wick. No mechanism is required for the adjustment of this electric candle. The discovery that kaolin becomes intensely incandescent under the current also enables M. Jablochkoff to dispense with the carbon points for small and medium lights. He made the discovery, we believe, in studying the effect of a succession of sparks from the secondary coil of an induction machine on refractory bodies. He first heated a plate of kaolin to incandescence, but did not fuse it. Then he led the induction current along the edge of the plate by means of a more conductive coating. This caused the edge to fuse and emit a splendid band of light as soft and steady as any known source. This discovery disclosed a feasible system of lighting towns and dwellings by dividing the electric light. It would be possible to generate lights of all sizes by means of the kaolin; and by employing a number of separate secondary coils, one to each candle, for one primary, the current could be simply and effectively divided. By having the carbon candles for large warehouses and public buildings, and a very simple pincher holding a kaolin wick for offices and corridors; and by having separate secondary circuits to each set of lights, electricity could be laid on for illuminating purposes as easily as gas. The passage of the current through the kaolin makes the circuit complete as in Lodighin's plan, and a number of lights can be joined up in the same circuit, so as to form a set of luminous centres. As many as eight candles have been kept steadily burning in the circuit of an ordinary magneto-electric machine. Some of the principal halls of the Louvre have have been lighted by the candle in this way. MM. Denayrouze and Jablochkoff have, we are told, easily obtained fifty luminous centres of various intensity in graduated series, the weakest yielding a glow equivalent to one or two gas burners, the strongest equal to fifteen burners, from one current. By employing a magneto-electric machine giving alternating currents the current interrupter and condenser of the induction coil may be dispensed with, the alternating currents being simply passed through the primary coil. Again, by employing a magneto-electric machine yielding several powerful intermittent currents, the induction coil with its several secondary coils may be dispensed with altogether and the magneto-electric currents passed through the candles. This power of being able to divide up the current so as to have several circuits with several candles of various degrees of illuminating effect in the same circuit, or only one, gives to electric lighting the convenience of gas. It cannot be so expensive as gas, and it must be far less pernicious and dangerous than gas in a house. The lights require to be shaded by ground or opal glass shades to diffuse the rays. The consumption of kaolin is very small. It is said that a piece the length of a centimetre will last ten hours.

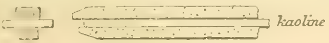

Fig. 2.

The recent public trials of Jablochkoff's light at the West India Docks have been recorded in NATURE. The first was unsuccessful owing to some defect in the magneto-electric apparatus. An account of the second and successful trial was given in NATURE, vol. xvi. p. 152. A large tent inclosing 900 square feet was illuminated by four candles fixed on lamp-posts and surrounded by globes of opal glass. At twenty or thirty feet from the lamps very faint pencil lines could be distinguished on paper, and small print read at a considerable distance. When common candles were substituted for the electric lights the effect was most marked, and the light a sickly yellow. In the electric illumination the most delicate colours retained their purity of tint. A warehouse was also lighted up by three naked candles; and a ship lying alongside a wharf by two, in order to show that lading or unlading could be carried on at night.

J. MUNRO

REDUCTION OF THE HEIGHT OF WAVES BY LATERAL DEFLECTION UNDER LEE OF BREAKWATERS[1]

WHEN a wave encounters an obstacle such as a breakwater, the portion which strikes it is either entirely destroyed or reflected seawards, while the portion which is not so intercepted passes onwards, and spreading laterally under lee of the barrier, suffers a reduction of its height. In the second edition of my book on Harbours, I expressed regret that no attempt had been made, so far as I was aware, to obtain any numerical value of this reduction of height derived either from theory or experiment, although the extent of shelter which is to be gained by the erection of our great national breakwaters depends entirely upon its amount.

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[1] By Thomas Stevenson, F.R.S.E.

From a few observations taken in the sea under lee of the breakwater in Wick Bay, and from some experiments made in a large brewer's cooling vat, it appeared that after passing round an obstruction *the reduction in the height of waves varied as the square root of the angle of deflection.* The approximate formula given in my book was—

$$x = 1{\cdot}00 - {\cdot}06\sqrt{a}$$

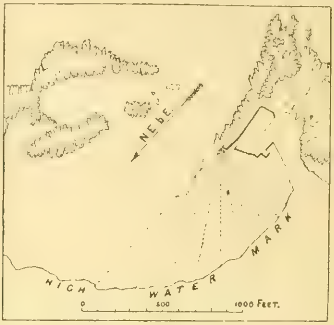
where x represents the ratio of the reduced to the unreduced wave, and a the angle of deflection.

On a recent visit to North Berwick, the finely-curved storm and tide marks traced out on the sandy beach under lee of the promontory at the harbour, reminded me of some observations I had made many years ago at other parts of the Firth of Forth. These observations, which were, however, very imperfect, had for their object the determination of the reduction of the waves by ascertaining the positions in reference to the centre of divergence of different parts of the line of high water mark where, of course, all the wave forces become *nil*. If a beach throughout its whole extent consist of easily moved materials such as sand or gravel, the incursion made at any one place by the sea will obviously depend upon the force of the waves which reach the shore at that place, providing the materials of the beach are homogeneous. In other words, the heavier the waves at any part of the shore the farther inland will the high-water margin retire beyond the tide mark of more sheltered places. And where the waves vary in height owing to some local cause, as, for example, the existence of a sheltering promontory, the high water mark instead of being straight and parallel to the prevalent waves will assume a curved outline.

At North Berwick, the projecting promontory at the harbour, shelters a small bay or rather *bight* from the heaviest waves that fall on that part of the coast. The waves, therefore, are deflected at the pierhead, from which point as a centre, each section of every wave taking its own divergent direction, runs its course till its energy is expended at high water mark. The maximum effect on the beach will consequently be in the line of direction of the undeflected swell, and the minimum effect will be in the direction of the landward end of the promontory where the waves are most deflected from their natural course. Under these conditions, supposing the particles of sand to be of uniform size and of the same specific gravity, the high water margin must assume, as it does at North Berwick, a curved outline owing to the inequality produced by deflection on the height of the waves.

If the distance between the pierhead and the high-water mark measured parallel to the usual direction of the undeflected swell (shown by the arrow in the diagram) be assumed as unity, that distance may be regarded as the measure of the amount of work that the undeflected part of the wave has been able to do, inasmuch as its force has been wholly expended within that distance in driving the beach landwards. The varying lesser distances between the same point and other parts of the high water mark, may in like manner be regarded as representing the work that has been done by the varying lesser forces exerted by the different parts of the wave after being deflected. It is, no doubt, true that the undeflected wave has the full force of the wind to help it, while the deflected has not; but in so far as relates to the engineering aspect of the question, this effect, even though it had been much greater than it is, would be of no importance, as the same conditions hold true with an artificial as with a natural breakwater.

I may mention in corroboration of the views that have been expressed that in the course of my practice as an engineer I have, at different exposed parts of the coast, had occasion to fill up a small creek with soft materials produced by works of excavation at an adjoining part of the shore. In the course of time the whole of these artificial deposits have, in every instance, been removed by the waves, and the former line of high-water been restored. By analogy, therefore, we must believe that if the bay at North Berwick were in like manner filled up artificially with sand as far seawards as the pier-head we should find, after a certain number of storms had occurred, that the whole of the sand had been washed out and the former line of high-water reproduced. If this be true, then the different distances between the pier-head and the high-water mark at North Berwick may justly be regarded as the measures of the varying forces of the different sections of the deflected wave under lee of the promontory.



The first column in the accompanying table shows the angles of deflection, while the second gives the measurements from the pier-head to the high-water mark as taken from the Ordnance map of North Berwick. The directions in which these measurements were taken are represented by dotted lines on the accompanying woodcut. The third column shows the ratios of those measurements to unity. The fourth column gives the ratios of the heights of the deflected wave calculated by the formula $x = 1 - {\cdot}06\sqrt{a}$, and the last the *plus* and *minus* differences. Though the employment of the *square-root of the angle* may perhaps be regarded as somewhat unusual, the formula as given is nevertheless more convenient for use than a logarithmic spiral formula, which might give nearly the same results.

Angles of deflection θ.	Distances from centre of deflection to high-water mark v.	Ratios of measurements.	Ratios of Heights of waves calculated by formula.	Differences.
0°	1150	1·00	1·00	·00
10	1000	·87	·81	– ·06
20	920	·80	·73	– ·07
30	840	·73	·67	– ·06
40	735	·64	·62	– ·02
45	700	·61	·60	– ·01
50	675	·59	·58	– ·01
60	600	·52	·53	+ ·01
70	570	·50	·50	·00
80	555	·48	·46	– ·02
90	530	·46	·43	– ·03

Although it is possible that the agreement of the measurements with the results calculated by the formula may turn out to be to some extent accidental, yet the results can hardly be regarded as very far from correct. And in a case of such importance to the maritime engineer where we have so very few direct observations of the waves in the open sea to guide us, and where it is undeniable that all such observations are invariably found to be excessively difficult to get, and even when got prove often unsatisfactory, any contribution to our knowledge, however imperfect, may be considered of some value; and all the more when, as in this case, the curve traced out on the beach is the result of long-continued action produced by innumerable storms.

A RUSSIAN ACCOUNT OF SCIENTIFIC PROGRESS IN INDIA[1]

WE have already noticed the meteorological journey of M. Wojeikoff round the world. The volume referred to below contains a series of letters written to Baron Osten-Sacken and M. Rykatcheff during his stay in India (December, 1875, to February, 1876).

He had great hopes of the development of meteorology in India. A series of stations working upon one uniform plan, together with a system of weather-warnings, was about to be established throughout the country under the superintendance of Mr. Blanford. That gentleman expected a great deal from a thoroughly organised system of weather-forecasts, owing to the periodicity and comparative regularity of meteorological phenomena in India. The non-periodical fluctuations are yet certainly very large—especially as to rains—but they are less complicated than elsewhere, and it was likely to be easier to detect the laws they obey. Already in 1874 the Government asked Mr. Wilson whether it was probable that the rainy period would be as short that year as it was in 1873; Mr. Wilson answered that he expected heavy rains at the end of the monsoons, and October was in fact very rainy. The importance of such forecasts may be seen at a glance, as the rice-crops depend entirely upon the quantity of rains and the time when they finish, the rice-fields giving the best crops when they remain under water during the first two months after the sowing.

A subject treated at greater length by M. Wojeikoff is the Black Earth of India. This fertile soil appears mostly in the western and southern parts of the country, especially on the table-land of the Deccan, whilst on the plains of Bengal and in the north-western provinces it is, on the contrary, nearly wanting. It attains its largest development on traps, being found only as smaller patches on the bottoms of valleys in the districts of crystalline rocks. Altogether, it does not occupy in India such extensive uninterrupted spaces as in Southern Russia, and even in the provinces where it is most developed, it covers but from fifty to seventy per cent. of the surface of the land. The data as to its thickness are few; six feet is not unusual, but thicknesses of twenty feet must have been observed on some deposits washed down from the slopes of the hills. A few analyses show a percentage of from 7·7 to 9·2 of organic matters, not much different from what was found in the black earth of Russia.

As to its origin, the most curious opinions continue to prevail among Indian geologists. Some suppose it to be merely a product of the disaggregation of traps; others continue to support the old opinion as to its origin in marshes. Dr. Oldham, who was the first to renounce an erroneous view long established in Western Europe, in a letter to M. Wojeikoff, adopted the theory of the origin of black earth from "a dense vegetable growth, principally herbaceous, but partly arborescent," although there are localities where it may have come "from jheels and marshes." M. Wojeikoff supports the opinion now prevailing in Russia, that Black Earth is the result of a herbaceous steppe-vegetation accumulated during long centuries. He points out that its marshy origin is contradicted by the facts that, 1, the percentage of organic matter in its upper and lower parts is much the same, while in marshy deposits it constantly decreases in the upper parts; and 2, Black Earth never contains a large amount of acids, as is always the case in marshy deposits. Therefore, Black Earth mostly covers the surface of the lower table-lands, and is of far rarer occurrence in the bottoms of valleys. As to these latter deposits many misconceptions still prevail. Many of them are secondary, being washed down by rains from the tops and slopes of hills, and M. Wojeikoff supposes that the black-earth in the lower parts of the Nerbudda, Taptee, Godavery, Kistna valleys, &c., has mostly such a secondary origin. There are many instances when the black-earth of low levels is not a secondary deposit. It is then the product of a grassy meadow-vegetation, grown upon the former marshy deposit *after* the total draining up of the marsh.

We notice, also, his remarks upon the interest afforded by India for ethnographical and anthropological explorations. There is much to do in these departments. An official report says that not less than two-thirds of the old monuments of India remain unexplored; and there are large parts of the country, as, for instance, the Central Provinces, where almost nothing was done in this direction. The question as to the origin of some of the aborigines of India is still very obscure. The origin of the Dravidians, for instance, seems to be very uncertain, and M. Wojeikoff had much trouble to procure for Dr. Hochstetter some twenty photographs of this interesting people. He warmly recommends India as a field for anthropologists.

METEOROLOGY AND THE INDIAN FAMINE

THE following letter appeared in the *Times* of Saturday last:—

In a recent article on the Indian Famine you asked whether science could do nothing to foresee and provide for these appalling calamities. I think that, as regards Madras at any rate, science may safely accept your challenge. The present famine was foreseen on meteorological grounds last year, and the continued drought during the present summer (an unusual feature in Indian famines) was indicated in a printed research as early as February. Meteorologists have for some time been aware that the eleven years' cycle of sun-spots is coincident with a cycle of atmospheric conditions producing ascertained terrestrial effects. Thus the minimum periods of sun-spot activity are coincident with the minimum appearances of the aurora and with the minimum number of cyclones, while the maximum periods of sun-spot activity are contemporaneous with the maximum activity of the aurora and of cyclones. The coincidence between the sun-spot cycles and the variations in the indications of the magnetic needle has also been affirmed, and a periodic connection between solar activity and terrestrial magnetism is now an accepted fact of science. A similar connection between the eleven years' cycle of sun-spots and the temperature and rainfall had also been suspected, and various researches had been undertaken to show that the supposition was well founded. It was at this stage of the inquiry that Dr. W. W. Hunter, the Director-General of Statistics to the Government of India, commenced his investigations last year into the rainfall of Madras. During this century six years of minimum sun-spots had occurred (1810 to 1867); and for practical purposes the present year, 1877, may be taken as the seventh period of minimum sun-spots within this century. Dr. Hunter also found that six great scarcities of sufficient gravity to be officially returned as "famines" had occurred during the same period (1810-77). Of these six famines five were caused by years of drought coincident with, or adjoining to, the periods of minimum sun-spots, and within Dr. Hunter's "minimum group." He further showed that the rainfall at Madras passed through an eleven years' cycle, corresponding with the cycle of sun-spots. That is to say, the rainfall reaches its minimum in the eleventh year, rises to its maximum about half-way through the cycle in the fifth year, and then declines again to its minimum in the eleventh year. The following condensed table shows the results of the six cycles for which records exist, from 1810 to 1876, the Madras register only having been kept, however, from 1813:—

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[1] *Isvestia* of the Russ. Geogr. Soc, 1876, No. 3.

Eleven Years' Cycle of Sun-Spots and Rainfall at Madras for Six Cycles, from 1810 *to* 1876.

	Average rainfall in inches, registered at Madras. (1813 76)	Average relative number of sun-spots (Wolf). (1810-60.)
Eleventh series of years in the cycle of eleven years	37·03	10·9
First and second series of years in the cycle of eleven years	42·07	10·0
Third and fourth series of years in the cycle of eleven years	49·12	39·8
Fifth and sixth series of years in the cycle of eleven years	54·64	73·4
Seventh and eighth series of years in the cycle of eleven years	52·36	53·7
Ninth and tenth series of years in the cycle of eleven years	49·02	33·5
Eleventh series of years in the cycle of eleven years	37·03	10·9

The general average of rainfall for sixty-four years, from 1813 to 1876, is 48·51 inches.

The average relative number of sunspots, calculated on the fifty-one years then available to Dr. Hunter, from 1810 to 1860, is 38·68.

This statement forms one of a series of eleven tables by which Dr. Hunter exhibited the coincidence of the two cycles. In my opinion, and I believe in the opinion of the other professional meteorologists in this country who have examined the evidence thus submitted, Dr. Hunter has established his conclusions as regards Madras, but he carefully abstains from hasty generalisations with reference to other parts of India. I may add, however, that from a careful examination of the rainfall at Bombay, it is evident that there are the clearest indications of a similar general coincidence, while evidence has recently been adduced of a cyclic character of the Calcutta rainfall, complementary to (although different from) the cycle at Madras. But, adopting Dr. Hunter's cautious estimate of the degree of certitude warranted by his examination (necessarily a partial one) of the Indian rainfall, I think that science may safely make the following replies to your challenge:—

1. That a period of deficient rainfall may be expected to recur in cycles of eleven years at Madras.

2. That the deficiency is of so serious a character that in five out of the seven of these cycles occurring within this century up to the present date, the deficiency has sufficed to cause a great famine in Madras.

3. That the duty imposed by the laws of Nature on the Indian Government is not to make a series of costly spasmodic and unsatisfactory efforts, but to deal with the water-supply in such a way as to meet a regularly recurring deficiency.

4. That the discovery of the cyclic character of the rainfall clearly points, as regards Madras, to the method to be adopted for this end. In the eleven-years' cycle there is a period, at the extremities, of greatly deficient water-supply—namely, in the eleventh, first, and second years of the cycle. There is also a period of excessive water-supply in the middle of the cycle—namely, in the fifth, sixth, seventh, and eighth years; and half way between these two periods—that is to say, on each side of the maximum central period—there are years of intermediate but ample water-supply—namely, in the third and fourth years on the one side of the central maximum period, and in the ninth and tenth on the other side of it. The following table, taken from Dr. Hunter's paper, very clearly illustrates this:—

	Average rainfall in inches, registered at Madras. (1813-76.)	Average relative number of sun-spots (Wolf). (1810-60.)
Minimum group—eleventh, first, and second years	40·39	10·32
Intermediate group—third and fourth with tenth and ninth years	49·07	36·71
Maximum group—fifth, sixth, seventh, and eighth years	53·50	63·61

5. That the permanent remedy for famine in Madras is, therefore, to deal with the rainfall in its cyclic aspect, and to husband and equalise the water-supply, not merely of the individual year, but of the cycle.

It is beyond my province to offer any opinion upon the form of hydraulic engineering best adapted to secure this end. But I would point out that while some of our modern Indian canals are principally useful in husbanding and distributing the water-supply of the year, the old native system of great embanked lakes or reservoirs unconsciously hit the true solution of the difficulty by husbanding and equalising the water-supply of the cycle.

I need hardly say that we are only at the beginning of this inquiry. What science asks from the Indian Government is the means of prosecuting it, and foremost among such means is a small solar observatory, for which it is understood that the necessary instruments were sent out to India some years ago, although they have not yet been utilised for this purpose.

ALEXANDER BUCHAN, Secretary of the Scottish Meteorological Society.

THE IRON AND STEEL INSTITUTE

THIS Association, one of the most active in the kingdom, and which has already done so much to bring the discoveries of science to bear on the iron and steel industries, commences its annual autumn meeting at Newcastle, on Monday, as we have already intimated. As usual, while several important papers are down for reading, much of the time of the meeting, between September 17 and 21, will be devoted to visiting some of the many industrial establishments in and around Newcastle.

The president of the meeting will be Dr. C. W. Siemens, F.R.S., and we notice that in succession to the late Mr. Jones, Mr. James S. Jeans has been appointed general secretary. On the first day the usual formal business will be transacted, the real work of the session commencing on Tuesday, when the Mayor of Newcastle will receive the members in the lecture-room of the Literary and Philosophical Society at half-past 10 A.M., and during the forenoon a selection of papers will be read. After luncheon the remainder of the day will be devoted to visits to various establishments, including Consett Ironworks, the works of Stephenson and Co., R. and W. Hawthorn, Hawks, Crawshay, and Co., the Newcastle Chemical Works, and others. A number of collieries will also be open to the inspection of members, and should a sufficient number be found willing to join in an excursion to the Roman Wall, it is proposed to organise a party, on Tuesday afternoon, to visit that interesting object, near the residence of Mr. John Clayton, the well-known antiquarian, who has kindly promised to receive the members.

The forenoons of Wednesday and Thursday will also be devoted to the reading of papers, and the afternoons to visits and excursions. On Wednesday the New Swing Bridge, one of the largest of its kind in the world, will be opened, and afterwards two steamers will take the members on excursions up and down the Tyne, the first steamer proceeding up the river as far as the New Cut, for the purpose of affording members an opportunity of witnessing the extensive dredging operations of the Tyne Improvement Commissioners, and thence sailing down again to the shipping spouts, the new Coble Dene Dock, and the piers, while the second steamer will take another party to some of the most important works down the river, as Leslie's and Mitchell's ship-building yards, Forster and Co.'s lead-works, the Jarrow chemical works, &c. On Wednesday evening a *conversazione* will be held in the Town Hall, Newcastle, when it is hoped that the telephone will be exhibited.

Doubtless one of the most interesting excursions will be that of Thursday afternoon, when a special train will convey the members to the Elswick Works (Sir W. G. Armstrong and Co.), thence proceeding to the Steel Works of Messrs. John Spencer and Sons, at Newburn. Friday will be entirely devoted to an excursion to Middlesbrough and the works on Tees-side. In the forenoon the new Browney Colliery Works and the Clarence Works of Messrs. Bell will be visited, and the Eston Steel Works and Blast Works of Bolckow, Vaughan and Co. After luncheon in the Royal Exchange, Middlesbrough, ten different works will be visited, including the Tees-side Iron Works, where the first Danks' rotary furnaces constructed in this country will be seen in full operation; the Ayresome Iron Works; the Tees Iron Works; the Tees-side Engine Works (Hopkins, Gilkes, and Co.); the Linthorpe Iron Works (Lloyd and Co.); the Newport Rolling Mills (Fox, Head, and Co.); the Ayrton Rolling Mills (Jones, Brothers, and Co.); the Middlesbrough Wire Works (Hill and Co.); the Newport Ironworks (B. Samuelson and Co.); the Middlesbrough Tube Works.

Among the papers to be read are the following: I. L. Bell, M.P., F.R.S.—Part II. of paper on the Separation of Carbon, Silicon, Sulphur, and Phosphorus in the Refining and Puddling Furnace and in the Bessemer Converter. Dr. Percy, F.R.S.—On some Scientific Facts connected with the Manufacture of Iron, &c. R. Howson.—On Mechanical Puddling. T. W. Plum, Old Park, Salop.—On Improvements in Blast Furnace Water-Cooled Tuyeres. A. L. Steavenson.—On the Manufacture of Coke in relation to the Iron Trade of the North of England. Mr. Greenwell.—On the Geological Features of the Great Northern Coal Field. Chas. Wood.—On Four Years' Improvements in the Utilisation of Slag. F. Giesbers.—On the Removal of Phosphorus from the Materials used in Smelting Pig Iron under M. Stein's Patent. A. Thomas.—On the Latest Improvements in Belgian Merchant Rolls. William Walker.—On a New Machine for Drilling Ironstone. M. Gautier, C.E.—Results of Experiments with Cannon manufactured from Steel without Blows.

When we state that in addition to what we have mentioned, an exhibition of various objects connected with the Iron and Steel Trades will be held in the Wood Memorial Hall, it will be seen that the members of the Institute have plenty of work before them, and that the meeting is likely to be one of great interest and practical importance.

OUR ASTRONOMICAL COLUMN

THE OUTER SATELLITE OF MARS.—As a guide to those who may be examining the immediate vicinity of Mars, with the view to detecting the exterior satellite, an ephemeris of its positions from September 8 to 18, for 8h. 30m. and 11h. 0m. each evening is subjoined. It will enable an opinion to be formed as to the chance of any object glimpsed within ninety seconds' distance from the centre of the planet, being the satellite or not. The elements employed in the calculation are the following:—

Passage of Ascending Node, 1877, Aug. 11·7495 Greenwich M.T.

Longitude of the node	82 48'
Inclination of orbit to ecliptic	25 24
Daily motion in orbit	285 26·928
Logarithm of the radius of orbit in seconds, at the mean distance of Mars from the sun	1·32795

The angles of position in the ephemeris are reckoned as in double-star measures—

	At 8h. 30m. P.M. Pos.	Dist.	At 11h. 0m. P.M. Pos.	Dist.
	°	"	°	"
Sept. 8	82	76	71	85
,, 9	199	30	125	40
,, 10	251	85	240	74
,, 11	298	37	269	65
,, 12	60	73	40	46
,, 13	89	66	76	83
,, 14	218	44	149	28
,, 15	256	82	246	79
,, 16	325	28	277	53
,, 17	65	77	50	56
,, 18	96	54	81	76

The apparent diameter of Mars according to Kaiser's measures is 25"·0 on the 8th and 24"·0 on the 18th.

M. Leverrier characterises Prof. Asaph Hall's discovery of the satellites of Mars as "une des plus importantes observations de l'astronomie moderne." It is in the highest degree an honour to American science. The magnificent instrument with which they have been detected, a masterpiece of mechanical skill, is of American construction, and we think every astronomer must admit that since it was mounted at the Naval Observatory, Washington, the national astronomical institution, admirable discernment has been shown in the selection of a class of observations upon which its extraordinary optical power could be brought to bear with the greatest advantage in the actual state of the science. Already our knowledge of the motions of the four satellites of Uranus and of the satellite of Neptune has been greatly advanced, and tables to facilitate the calculation of their positions have been skilfully prepared by Prof. Newcomb, with the aid of measures made with this instrument. The period of rotation of Saturn has been determined, and a series of observations of all the eight satellites of this planet has been vigorously prosecuted, which must soon allow of a much more intimate acquaintance with their motions than we yet possess. The notable discovery of two satellites of Mars is a fitting achievement in the same interesting branch of astronomy.

In striking illustration of the truth of the assertion of Sir W. Herschel, that when a very faint object has been once discovered with a large telescope, it may be seen with a much smaller one, we receive, since the above was written, a communication from Mr. Wentworth Erck, of Sherrington, Bray, dated September 8, in which he writes: "The outer satellite has been seen here three times; 1st, on September 2, at 22h. 40m. G.S.T., when the position was about 290°, and distance from limb something less than three diameters of the planet; 2nd, on September 3, at 23h. 0m. G.S.T., when the position was 64°; this position is pretty accurate; on this occasion I watched the satellite for two hours, during which I saw it move from 64° to 55°; at the latter position its distance from limb was equal to two diameters of the planet; 3rd, on September 8, at 22h. 35m. G.S.T., when the position was about 78°. It was steadily visible with 7-inches aperture on my Alvan Clark, and was, I should say, something brighter than Enceladus, the second satellite of Saturn."

On comparing these observations with positions calculated from the above elements (which closely represent the Paris observation of August 27), it is evident the object observed on September 2 was a star, the satellite at the time being on an angle of 325°, and only fifteen seconds from the limb, but it appears beyond doubt that

Mr. Erck observed the outer satellite on the following night, when the position at the time named would be 65°, distance from centre of planet seventy-nine seconds, and two hours later the angle would have diminished to 53°, and the distance to sixty-one seconds, or roughly two diameters from the planet's limb as observed. On September 8 the angle was 71°, distance eighty-five seconds, so that the satellite may have been seen again this evening. So far as we know these are the first observations of a satellite of Mars in these islands, and it is singular that they have been made with an instrument constructed by the same optician as the great Washington telescope with which the satellites were discovered.

In the elements of the satellites transferred to this column last week from the Washington Circular, for major and minor axes of the apparent orbit it is necessary to read semi-axes.

VARIABLE STARS. — The following are geocentric minima of Algol and S Cancri, which will be observable in this country during the last quarter of the present year. The epochs are in Greenwich time, and depend upon Prof. Schönfeld's elements.

ALGOL.

		h.	m.			h.	m.			h.	m.
Oct. 2	...	15	19	Nov. 11	...	18	42	Dec. 4	...	17	14
,, 5	...	12	8	,, 14	...	15	31	,, 7	...	14	3
,, 8	...	8	57	,, 17	...	12	20	,, 10	...	10	52
,, 11	...	5	45	,, 20	...	9	9	,, 13	...	7	41
,, 22	...	17	0	,, 23	...	5	58	,, 24	...	18	57
,, 25	...	13	49					,, 27	...	15	46
,, 28	...	10	38					,, 30	...	12	36
,, 31	...	7	27								

S CANCRI.

		h.	m.			h.	m.
Oct. 16	...	13	27	Dec. 12	...	11	6
Nov. 4	...	12	40	,, 31	...	10	20
,, 23	...	11	53				

MINOR PLANETS.—On August 11 M. Borrelly detected a new planet, which, it may be presumed, is identical with one seen by Prof. Watson on the 8th, though not identified as a planet until the 16th; this will be No. 174. The latter astronomer has since announced the discovery of No. 175 on September 3, in R.A., 23h. 10m., N.P.D. 89° 15′, eleventh magnitude.

Of the small planets which come into opposition during the last quarter of 1877, Iris attains the greatest degree of brightness, her magnitude in the middle of November being a little higher than the seventh. This planet, from proximity to the earth, will afford a favourable opportunity of applying Prof. Galle's method of determining the solar parallax, and with the view of facilitating observations, an ephemeris from Prof. Brünnow's tables will be given in this column before the end of the present month. The rough ephemeris of the *Berliner Jahrbuch* is not sufficient for practical purposes.

NOTES

THE health of M. Leverrier is so far restored that he is daily expected at the Paris Observatory to resume his official duties. The glass of the large refractor has been put in position, after having undergone repairs, and will be tested again before being silvered. Bischofsheim's transit instrument is in use, and works admirably. The magnetic instruments are also in operation in the new grounds given by the municipality. Magnetical observations are also taken at Montsouris Observatory with similar instruments, and at a distance of two kilometres. Both establishments are satisfied with eye observations.

ON August 14 Denmark celebrated the centenary of one of her most eminent sons—Hans Christian Oersted, born August 14, 1777, known all over the world as the discoverer of the laws of electro-magnetism. It was in 1813 that Oersted first published his investigations.

AT the recent biennial meeting of the German Astronomical Society, which was held at Stockholm, the members received the news of the discovery of the two satellites of Mars with manifestations of grave doubts. The president at their request telegraphed to the Berlin Observatory, and in reply received a copy of the original telegram as it was sent from America. The next meeting of the Society is fixed to take place at Berlin in 1879.

WE have received the "Programme et Règlement" of the International Congress of the Medical Sciences, which commenced its fifth session at Genoa on Sunday and will conclude on Saturday. This programme contains a feature which we have not noticed before in connection with any similar congress. All the usual information as to meetings of various kinds, sectional proceedings, excursions, &c., is given in a well-arranged form. In addition to this, under each section is given along with the titles of the papers to be read, a summary of the conclusions come to by the author on each question treated. These summaries are sometimes of considerable length, and we cannot but think that it is an advantage both to speaker and to hearers that the latter are thus instructed and interested beforehand, and so able to follow intelligently a speaker's line of thought. Although the association is to meet during a whole week, there are only twenty-four papers in all to be read, thus allowing ample time for discussion.

THE inaugural address of the meeting of German naturalists at Munich on the 17st inst. will be delivered by Dr. von Pettenkofer. The following is the latest list of the general lectures announced:—Prof. Dr. Waldeyer (Strassburg), on C. E. von Baer and his influence upon the history of evolution; Prof. Dr. Ernst Haeckel (Jena), on the evolution theory of the present day in its relation to science in general; Prof. Dr. G. Tschermak (Vienna), on the early history of the terrestrial globe; Prof. Dr. Klebs (Prague), on the revolution in medicinal views during the last decades; Dr. G. Neumayer (Hamburg), on meteorology in daily life; Dr. R. Avé Lallemant (Lübeck), on animal life in the Amazon River; Prof. Dr. S. Günther (Ansbach), on the latest researches made on the mathematico-historical domain; Prof. von Virchow has not yet fixed his subject.

THE third annual conference of the Cryptogamic Society of Scotland will be held at Dunkeld during October 10, 11, and 12. The president is Col. H. M. Drummond Hay, C.M.Z.S., and the secretary Dr. F. Buchanan White, F.L.S., Perth. The business of the conference will consist mainly in excursions, *conversazioni*, and an exhibition of specimens. The Society is now prepared to issue a First Century of "Fungi Scotici Exsiccati," which will contain many of the new species and rarities recently discovered. The subscription price is *1l. 1s.*

THE Munich Society of Antiquaries has resolved to hold yearly exhibitions after the manner of those of our South Kensington Museum. Each exhibition will be devoted to a different branch of industry. A commencement will be made with glass articles.

ALTHOUGH the late M. Thiers was not himself a man of science he was anxious to possess some knowledge of the several sciences in order to the writing of a work on philosophy on which he was engaged during a number of years. His teachers were chosen from amongst his brother academicians; M. Leverrier being his instructor in astronomy and M. Charles Saint Claire-Deville in chemistry. He began to write his work under Napoleon's rule, desisted when he resumed his political career, and worked it up again when he resigned his presidentship. It is not yet known whether it will be published in its present imperfect form. At the time of his death he was revising what had been written in order to bring it up to the level of new scientific discoveries. Although Thiers was more than eighty years of age he was not the oldest member of the whole French Institute. The dean by age is now M. Chevreul, a man of extraordinary debating power, and who the very day of Thiers' death read before the Academy of Science an admirable chemical paper referred to in this week's report of the Paris Academy.

Mr. Layard, the British ambassador at Constantinople, has received a firman from the Sultan authorising excavations at Nineveh.

Several members of the enterprising Birmingham Natural History Society spent last week in a dredging excursion at Arran, Brodick being made the headquarters. The excursion was eminently successful, and several objects of considerable rarity were obtained. An interesting account of the excursion appears in the *Birmingham Daily Post* of September 10.

At the meeting of delegates held to discuss the formation of a Midland Union of Natural History Societies on August 28, at the Midland Institute, a basis of union was unanimously agreed to. The Union is to be governed by a council composed of two members from each society included within it, one of these members to be a secretary of the society. The annual meetings are to be held in the various towns in which are located the societies forming the Union. The president of the society in association with which the annual meeting is held shall be president of the Union for that year, and each annual meeting shall decide where the next is to be held. The levy from each society is to be 1*d.* per annum for each member, unless the members shall be less than twenty-four in number, when it shall be 2*s.* for the whole of the society. A monthly journal is to be published by the Union similar to the *West Riding Naturalist*, to be called the *Midland Naturalist.* The following Sub-Committee was appointed to arrange various matters:—Messrs. Lawson Tait, James Bagnall, John Morley, Egbert D. Hamel, and Charles Perks. The first meeting of the Council will be held at the Midland Institute, Birmingham, on Tuesday, October 2, at 5 P.M.

London now possesses an aquarium tank believed to be the largest in existence. By removing the partitions between several of the tanks in the Westminster Aquarium, the present naturalist, Mr. Carrington, has arranged one capable of holding 94,000 gallons. It is 150 feet long, 20 feet broad, and the depth of the water will vary according to circumstances. The tank is intended for the exhibition of large fish.

A shock of earthquake was felt at Bagnères de Bigorre at 2 P.M. on Friday, its direction being east to south-east, and its duration five seconds.

Local societies have organised in Switzerland a large number of district museums so arranged that the natural characteristics of the country are always open to the inspection of the public. The extent of these collections is sometimes astonishing. We have been favoured with a catalogue of the Vevey Cantonal Museum, from which we learn that it possesses 415 zoological objects, three different herbariums, four mineralogical collections, fifteen objects of ethnography, twenty-five archæological relics found in the country, 650 pieces of money or medals, complete laboratories of physics and chemistry, and a library. Vevey is a small country place having only 8,000 inhabitants.

The Council General of Guadeloupe have offered a premium of $20,000 for the best new process of extracting juice from sugar-cane, the cost not to exceed forty per cent. of the market value of the product. Applications may be made up to June 1, 1880.

We have referred to the expedition to Cambodia, headed by Dr. Harmand, on behalf of the French Government. A large parcel containing a number of natural history specimens has arrived at Paris for the Museum, from the expedition.

The Germanic Museum at Nürnberg celebrated the twenty-fifth anniversary of its existence on the 16th ult. The celebration was simple, dignified, and in good keeping with the national character of the institution. The foundation-stone of a new wing of the building of the museum was laid, the funds for this enlargement having been supplied by the German Government. The director, Herr Essenwein, addressed the company assembled, and gave an account of the development of the museum, pointing out that the institution was the result of common efforts made by the whole nation; that princes and people had equally contributed towards the furtherance of the work, and that the new wing would bear the Imperial arms upon its façade as a sign of its origin.

The European staff for the first international station to be established in Africa has been completed. It consists of Messrs. Cambier, Crespel, and Mais. The Austrian traveller, Ernst Marno, will accompany the expedition in the capacity of naturalist. The expedition will proceed to Natal in the Union Company's Royal Mail steamship *Danube*, appointed to leave Southampton on October 18 next, direct for Algoa Bay and Natal. The expedition will remain at Natal for a week or ten days, perfecting their arrangements, and will then go on to Zanzibar, for Tanganyika, in the same Company's steamship *Natal.*

Among the papers in the just-published part of the *Zeitschrift* of the Berlin Geographical Society are letters from Dr. Erwin von Bary to Baron von Richthofen on the travels of the former in North Africa; the itinerary of Dr. Pogge from Kimbundo to Quizemene, the Mussumba or residence of Muata Jamwo, and further eastwards to Inchibaraka, from September 16, 1875, to February 28, 1876; two papers on Persia, one by Dr. Kiepert on Dr. Stolze's journey in South Persia in 1875, and a description by Gen. Schindler of little known routes in Chorassan. Capt. von Schlienitz contributes a paper of much interest on the geographical and ethnographical observations in New Guinea, the New Britannia and Solomon's Archipelago, obtained by the Austrian *Gazelle* expedition. To the *Verhandlungen* (Nos. 5 and 6) of the same Society Baron von Richthofen contributes a paper of much historical value on the Central Asiatic silk routes up to the second century of our era.

At the last meeting of the Niederrheinische Gesellschaft für Natur und Heilkunde of Bonn, Herr Siegfried Stein reported on some new mirrors made of rock crystal and agate, and used for reflecting sextants. Dr. H. Eylert, the astronomer to the Deutsche Seewarte at Hamburg, had pointed out to him that glass mirrors lose their correct shape in course of time, or turn dim and render impossible the exact determination of the sun's altitude. The new mirrors are free of these faults.

Throughout the past week, says the *Sussex Advertiser*, Col. A. Lane Fox has been engaged, with several workmen, in making some interesting excavations into Mount Caburn, on behalf of the combined committees of the British Association and Anthropological Institute. A number of pits were found in the interior of the camp, and some of them have been opened. They are of different sizes, and between six and seven feet deep, and are of a square, oval, and round shape. They were evidently human habitations, and would contain perhaps two persons crouched up together, there not being room for them to lie extended. They were found to contain the bones of a great variety of animals used for food, but chiefly of the ox, pig, and goat, and the remains have been sent to Prof. Rolleston, of Oxford, for identification. The filling in of the pits appears to be of the late Celtic period, but whether the pits themselves are of the same age it is difficult to determine. A large basin-shaped shaft, sixteen feet deep, has been cleared on the south side of Mount Caburn. In this case it is also difficult to decide the object of the pit, but it appears in all probability to have been sunk by the inhabitants of an earlier period for the purpose of obtaining flints similar to those of Cissbury. A vein of flints was found near the bottom of the shaft, but there are no galleries, as is the case at Cissbury, where they are perfect in the shafts that have been discovered. Probably in the present instance the flints were found to be unsuitable, and the works were abandoned. On Saturday a section was cut through the rampart in order to ascertain by the pottery whether it was of the same age. Large quantities of pottery were found, which was of an earlier period to that in the pits in the interior, indicating that the rampart is probably of an earlier date, and that the fort was subsequently occupied by a later race of people in the Celtic age. At the bottom of the pits were discovered several implements of the late Celtic type—amongst other things a knife, battle-axe, and a kind of iron spud; also a bone comb.

THERE have just been placed at the entrance to the South Kensington Museum four magnified drawings of different stages of the Colorado beetle, drawn by Mr. Andrew Murray, F.L.S., naturalist to the Science and Art Department. They show the pupa—the "grub," as it is labelled, at full growth, the beetle at rest, and the beetle on the wing. This last is the most interesting drawing to people in England, as the appearance of the beetle at rest is now somewhat familiar through the many drawings and the models that have been issued. As Mr. Murray has studied the beetle in America, not only the shape, but the hue of the wings may be taken as correct. These are of a delicate pink, and look so beautiful that it might be welcomed in England as an addition to our insects were its ravages not so expensive. Of all the drawings of the beetle yet made public these are the most carefully drawn, but it is surely a mistake to place them where they are at present. They are really outside the turnstiles, as if not forming a portion of the museum.

A THIRD edition is announced by Hartleben, the Vienna publisher, of Brommy and von Littrow's work "Die Marine," a comprehensive treatise on the ocean and on navigation.

THE prospectus of a new work entitled "Ergebnisse physikalischer Forschung," by Dr. C. Bohn, Professor of Physics in Aschaffenburg, is being circulated by Engelmann of Leipsic. The work aims at giving the facts ascertained in physics, leaving out, as far as practicable, the processes by which they have been reached (a mode of treatment which offers certain advantages). Theories, hypotheses, and methods of experimentation are not wholly excluded, but the attention is limited to what seems useful and necessary in order to confidence in the correctness of the results. The work is designed especially as a preparation-book for examinations; also as a work of reference, for which purpose it will be fully indexed. The first part, just published, treats of I. Bodies and forces in general (as Introduction). II. General mechanics and gravitation. III. Physical mechanics. IV. The theory of heat (Part 1). Part 2 will also treat of IV. The theory of heat (second part). V. Radiation (light and heat). Part 3 will be devoted to VI. Magnetism and electricity.

THE *Kölnische Zeitung* publishes some interesting details regarding an excursion made by Herr Nikolai Sograf, a member of the Moscow Society of Naturalists, to the peninsula of Kanin in the Samoyede district. It appears that the natives showed themselves rather diffident towards the Russian traveller, and their want of confidence was still further increased when he proceeded to take measurements of their heads, extremities, &c. At last they evidently arrived at the conclusion that their reindeers and their tundras were to be taken from them and they themselves were to be transported far away, perhaps to the war now raging. The consequence of this conviction was a somewhat unpleasant intermezzo for Herr Sograf and his excursion. On one fine morning the traveller was aroused by a great noise and found that the Samoyedes were packing up their tents in great haste and preparing for an immediate departure into the interior of the country. All representations on his part were in vain, and he had to submit to being carried off. His position among these natives was, of course, a rather unpleasant one; his collections of insects, which were preserved in alcohol were destroyed, the preserving fluid finding its way into the stomachs of the Samoyedes. Herr Sograf was rescued at last by two fishermen from the Kanin Tundra coast, who had heard of his awkward situation. He arrived safely at Mesen after some time, and thence addressed a narrative of his adventures to the Russian newspaper the *Golos*. His courage and spirit seem, however, to be quite undaunted, as he has the intention to start at once upon another expedition to Cape Sswyatoi-Noss.

IN compliance with a suggestion from M. Engelhard, a member of the Municipal Council of Paris, the Prefect of the Seine has ordered photographs to be taken of every existing plan of the city of Paris, so that it will be possible to see all the series of transformations experienced by the city from the most remote period when such plans were made up to the present age.

FLAGS have been hoisted on the principal buildings of the Paris International Exhibition, which, according to the habit of French working-men, is an indication that the external part may be considered as having been quite completed. The part allotted to England has been given over to the architect of the English Government.

THE *American Journal of Science* records the death in Utah of Dr. Charles F. Winslow, formerly of Boston, at the age of sixty-six. Dr. Winslow has written numerous articles on physical science, relating more especially to earthquake phenomena, which he observed extensively during his residence of many years in California, South America, and elsewhere.

DRS. YARROW AND COUES have lately published a list of the fishes found at Fort Maçon, North Carolina, in which 108 species are enumerated; of these eleven are sharks and rays.

IN a paper by Prof. Wright, of Yale College, published in the early part of this year, a method was described of producing metallic films on the inner surface of exhausted glass tubes, by the action of a succession of energetic electrical discharges. Prof. Wright has been continuing this investigation, and has arrived at important results, both theoretical and practical. It seemed probable that the surface of deposit would be dull, but this proved incorrect, and the films when removed from the receiver presented surfaces of exquisite perfection, the most brilliant of which were comparable only to the surface of clean liquid mercury. This suggested the production of specula for optical purposes by this method, and Prof. Wright's subsequent inquiries have been directed to this end. Platinum seems to be the most suitable metal for the purpose; it is not readily tarnished, and can be cleaned with water or acids. By the new method it can be deposited on glass surfaces very easily, and a mirror of the most perfect surface produced at once. The adherence of the film seems very close. With silver, too, the process succeeds well, but it is more difficult to obtain good surfaces than with platinum, or gold, the metal being volatilised with extreme ease by the action of the current. The experiments, *inter alia*, showed it to be true of platinum and bismuth (as well as gold), that the light which has passed through a layer of metal varies somewhat with the thickness of the film. Platinum, with progressively thickening film, varies from a grayish tint to brownish, brownish yellow, deep yellow, and orange. This last colour is almost exactly complementary to that transmitted by silver, and Prof. Wright succeeded in getting a peculiarly white and brilliant reflecting surface by depositing first a thin stratum of silver, then one of platinum. The specula of a small

Gregorian telescope was covered with platinum by the method referred to, and with entire success. The larger mirror was about 4 ctm., and the time required for covering was about three hours, with a battery power equivalent to four or five Grove cells.

It is an interesting point to determine, even though only approximately, what amount of heat meteoric masses develop in their motion through the atmosphere. M. Schiaparelli has proved that to calculate the loss of velocity of a body which penetrates the atmosphere, it is not necessary to know the law according to which the density of the air varies in the different layers traversed, but it is sufficient to know the barometric pressure at the two extremities of the course, or (which comes to the same) the weight of air displaced by the body whose initial velocity is known. Starting with this theorem, and taking the average velocity of falling stars as 50,000 metres per second (it varies between 16,000 m. and 72,000 m.), M. Govi estimates, in a recent paper, that a falling star, on reaching a part of the atmosphere where the barometric pressure is about 1 mm., will have its velocity already reduced to about 28,000 m.; coming to a region where the pressure is 10 mm., its velocity will be about 5,916 m., and at a pressure of 100 mm. (should the meteorite reach so far), the velocity will not be more than 506 m. Thus the initial velocity of bolides diminishes very rapidly, and may be almost entirely lost on reaching earth. Now, knowing the loss of velocity of a moving object of given mass, the quantity of heat developed can easily be deduced. Taking a bolide of 14·66 kilogrammes reaching an air layer where the pressure is about 1 mm., M. Govi finds the loss of *vis viva* of the object to correspond to the enormous number of 2,921,317 calories, which would readily explain all the accompanying phenomena of heat and light, and also mechanical effects.

WITHIN the last year or two no spot on the American continent has furnished such an amount and variety of archæological material as the Santa Barbara Islands and the coast of California opposite to them. Dr. Yarrow, in Wheeler's report for 1876, describes his "Big Bonanza," as he calls it, on the main-land, near Santa Barbara; and Prof. Hayden has published an elaborate account of Mr. Paul Schumacher's researches made upon the islands. The crania and skeletons are counted by hundreds, and tons of stone implements, many of them of most beautiful workmanship, have been revealed by the winds blowing the sand from the burial-places. During the summer of 1877 Mr. Bowers has continued these explorations on behalf of the Smithsonian Institution, with the greatest success, and has found many new varieties of objects of stone, and in large numbers.

MR. GUNN writes with reference to our report last week (p. 406) of a paper read by him at the late British Association Meeting:—"The new Silurian beds discovered near Widdybank Farm were stated in the paper to belong to the volcanic series of the Lake Country. It is the Cronkley Pencil Mill Shale which was referred to the Stockdale Shales *or* Pale Slates."

THE additions to the Zoological Society's Gardens during the past week include a Mandrill (*Cercopithecus mormon*) from West Africa, presented by Mr. Francis Lovell; a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, presented by the Misses Rowney; a Common Fox (*Canis vulpes*), European, presented by Mr. R. Hayssen, an Angolan Vulture (*Gypohierax angolensis*), a Vociferous Sea Eagle (*Haliaëtus vocifer*) from Africa, presented by Mr. I. A. Solomon; a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, a Great Anteater (*Myrmecophaga jubata*), two Mealy Amazons (*Chrysotis farinosa*) from South America, a Short-toed Eagle (*Circaëtus gallicus*), a Common Genet (*Genetta vulgaris*) from Southern Europe, an Egyptian Vulture (*Neophron percnopterus*) from Africa, deposited.

THERMOMETRIC OBSERVATIONS MADE AT RAMA ON THE COAST OF LABRADOR

TWO years' thermometric observations made three times a day at Rama, Labrador, about 60° north latitude, furnish the following monthly and annual means. The degrees are Centigrade.

	Monthly mean of temperature.	Absolute monthly minima. (Dates in parentheses.)	Absolute monthly maxima. (Dates in parentheses.)
	°	°	°
1874, August	+ 7·7	+ 2 (3)	+ 23 (8)
September ...	+ 4·9	– 1·5 (6, 7)	+ 16 (9)
October ...	– 0·7	– 8·5 (30)	+ 9 (5)
November ...	– 6·5	– 15·0 (27)	+ 7 (9)
December ...	– 15·2	– 22·0 (23, 24)	– 5 (6)
1875, January ...	– 17·8	– 27 (23, 24, 27, 28)	– 5 (26)
February ...	– 19·6	– 35 (19)	– 5 (13)
March	– 17·9	– 33 (8)	+ 2 (19)
April	– 8·1	– 20 (5, 6)	+ 3 (20, 21)
May	– 3·7	– 13 (1, 2, 3)	+ 10 (17)
June	+ 4·2	0 (1, 9, 14, 17, 18, 20, 23)	+ 15 (24)
July	+ 5·2	+ 1·5 (20, 22, 26, 28)	+ 17 (19)
August	+ 7·0	+ 1·0 (28, 31)	+ 16 (4, 20)
September ...	+ 2·3	– 7·0 (28)	+ 15 (1)
October ...	– 2·0	– 7·5 (30)	+ 4 (1, 8)
November ...	– 6·1	– 21 (26)	+ 2 (7)
December ...	– 15·4	– 31 (28)	+ 2 (6)
1876, January ...	– 20·5	– 30 (17)	– 7 (11)
February ...	– 19·1	– 32 (8)	+ 3 (25, 26)
March	– 14·8	– 34 (6)	+ 1 (23)
April	– 2·3	– 21 (4)	+ 10 (9)
May	+ 1·7	– 8 (7)	+ 10 (15)
June	+ 3·5	– 9 (0)	+ 11 (10, 30)
July	+ 6·5	+ 0·5 (7)	+ 20 (27)

The annual means resulting from these tables are—

For the first year, from August, 1874, to July, 1875, – 5°·51,
,, second ,, ,, 1875 ,, 1876, – 4°·93.

The annual extremes are—

In the first year, min. – 35°, Feb. 19; max. + 23°, Aug. 8,
,, second ,, – 34°, March 6; ,, + 20°, July 27.

The first year was sensibly more cold than the second; if we compare their means to those of the two preceding years—

1872 to 1873, – 4°·0; 1873 to 1874, – 5°·3;[1]

their results as the mean of the four consecutive years counting from the month of August, – 4°·93.

The observations were made at Rama by M. and Mme. Weiz, Moravian missionaries, with a thermometer sent from the observatory of Geneva. A barometer was recently sent from Kew to Naïn, another station of Labrador, to solve the question of the variations of atmospheric pressure in these regions.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

LONDON.—Prof. Morris, who for more than twenty years has held the chair of geology and mineralogy at University College, has resigned his appointment. The Rev. T. G. Bonney, fellow of St. John's College, Cambridge, and Prof. H. G. Seeley, of King's College, are mentioned as candidates.

BIRMINGHAM.—We have received the programme of the Birmingham and Midland Institute, which is quite as satisfactory as usual. Prof. Tyndall, the president, will deliver the inaugural address on the evening of Monday, October 1.

UPSALA.—The celebration of the 400th anniversary of the foundation of the University of Upsala was brought to a termination on Saturday. The celebration, which lasted nearly the whole week, seems to have been thoroughly successful, and was enthusiastically joined in by all, both "town and gown," from the king downwards. Many foreign delegates were present. We hope to be able to give a detailed account of the event shortly. It is stated that the king has made the University a donation of 40,000 crowns, the yearly revenue from which is to be distributed as premiums to young authors of scientific works.

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[1] NATURE, vol. xiii. p. 60.

SCIENTIFIC SERIALS

American Journal of Science and Arts, August.—Discovery of oxygen in the sun by photography, and a new theory of the solar spectrum (with plate), by H. Draper.—Action of certain organic substances in increasing the sensitiveness of silver haloids, by M. C. Lea.—Critical periods in the history of the earth and their relation to evolution, by J. Le Conte.—Notes on the internal and external structure of palæozoic crinoids, by C. Wachsmuth.—Chemical composition of Hatchettolite and Samarskite, by O. D. Allen.—Relations of the geology of Vermont to that of Berkshire, by J. D. Dana.—A proposed new method in solar spectrum analysis, by S. P. Langley.—Note on the exactitude of the French normal fork, by R. König.

Annalen der Physik und Chemie, No. 7, 1877.—The polarisation of refracted light, by M. Fröhlich.—Note on the dispersion-curve of certain media with more than one absorption-band, by M. Ketteler.—On a new photometer, by M. Glan.—On electric induction on non-conducting solid bodies, by M. Wüllner.—On the electric behaviour of metals immersed in water or salt solutions in radiation from sun or lamplight, by M. Hankel.—Note on a change in the direction of the polarisation-current after passage of alternately opposite galvanic currents, by M. Hankel.—On vapour tensions of homologous series and Kopp's law of constant differences of boiling-point, by M. Winkelmann.—On the absorption of gases by salt solutions, by Mr. Mackenzie.—On the theory of the action of cylinder spirals with variable number of windings, by M. Wallentin.—On diamonds, by M. von Baumhauer.—On the history of the invention of the telescope, by M. Wolf.—Note relating to natural science among the Arabs, by M. Wiedemann.

No. 8.—Experimental investigation of weakly magnetic bodies, by M. Silow.—On a general proposition with reference to electric induction, by M. Clausius.—On the electric conductivity of electrolytes, by M. Berggren.—Determination of the electric conductivity of liquids with constant current, by M. Tollinger.—On the so-called unipolarity of flame conduction, and on truly unipolar electric phenomena, by M. Herwig.—Further remarks on the action of cylinder-spirals with variable number of windings, by M. Wallentin.—Contributions to an adequate determination of the plane of vibration of polarised light, by M. Ketteler.—On the specific heat of water according to experiments of M. von Münchhausen, by M. Wüllner.—On the physical nature of articulate sounds, by Mr. Grassmann.—On a convenient form of the mercury-pump on Sprengel's principle, by M. Hüfner.—Bunsen in a tellurium mineral, by M. Krenner.

Journal de Physique, August.—Researches on photography, by M. Angot.—On attractive and repulsive forces, and the action of the medium, by M. Jannery.—New electric lamp with oblique circular rheophores, by M. Reynier.—Polarising microscope, by M. Nodot.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, September 3.—M. Peligot in the chair.—M. Villarceau gave an outline of his and M. De Magnac's new work, entitled "Nouvelle navigation astronomique."—The following papers were also read:—On the combinations of chlorhydrate of ammonia with the chlorides of potassium and of sodium (extract from memoir), by M. Chevreul. He was led to study these from having found in guano cubical crystals formed of chloride of sodium and chlorhydrate of ammonia; also a similar compound in a piece of sealskin taken from this guano. Some peculiarities in crystallisation are mentioned.—Considerations on the interpretation which should be given to the conditions of maxima relative to calculations of electro-magnetic forces, by M. Du Moncel (a reply to M. Raynaud).—On the discovery of a terrestrial plant in the middle part of the Silurian system, by M. De Saporta. He was shown at Caen a plate from the slatey schists of Angers, presenting evident traces of a large fern. The vegetable substance had been replaced by sulphide of iron, and much of the contour was interrupted and lacerated as if the plant had been long under water. The fern appears to rank among the Neuropterideæ; it recalls *Cyclopteris* and *Talæopteris*, observed in the Upper Devonian. The Silurian system in Europe having only furnished, hitherto (of plants) algæ, and of somewhat problematic nature, this fern may be regarded as the oldest terrestrial plant yet found on our continent; and it indicates a floral already comparatively rich and complex, therefore distant probably from the first origin of plants. M. Lesquereux has also, quite lately, found terrestrial plants in the Silurian system in America (at the base), and M. De Saporta assigns priority in this discovery to him.—Researches on the phosphoric acid of arable lands (extract from memoir), by MM. Corenwinder and Contamine. In forty-eight hours a saturated solution of carbonic acid sufficed to render assimilable a quantity of phosphoric acid greater than that furnished to the soil by introducing 1,000 kilogrammes of super-phosphate. The phosphates disseminated in arable land are not in the same degree soluble in water charged with carbonic acid. Their capacity depends on their molecular state and the source whence they come. The phosphates pre-existing in liquid manures are probably more attackable than others.—On the invariability of the great axes of planetary orbits, by M. Haretu. This invariability, which several geometers, and Poisson himself, believed to be quite general, exists only for the first and second powers of the masses.—On an insect destructive to phylloxera, by M. Laliman. This larva, or worm, which (the author says) might be called the *cannibal* of phylloxera, devours the latter most voraciously; in ten minutes he saw ninety-five disappear. He found it in the interstices or tissue of galls on the leaves of the vine.—Remarks on M. Laliman's communication, by M. Balbiani. The observation is not wholly new; the larva is that of a dipterous insect belonging to the genus *Syrphus*, or an allied one. All the larvæ of Syrphi are aphidiphagous; their habits have been fully studied by M. Reaumur, who remarked their voracity and the indifference of taste they showed for all kinds of pucerons. M. Balbiani recommends a continuation of these researches, and cultivation of the insect.—Invasion by phylloxera of the vineyards in the environs of Vendome, by M. Prillieux.—Satellite of Mars observed at the observatory of Paris, by MM. Paul and Prosper Henry.—New stellar system in rapid proper motion, by M. Flammarion. This is perhaps still more important than the former; for it consists of two couples of stars carried along in space by the same movement of translation, and with a velocity much above the average of ordinary proper motions. The two couples are those of 17 χ Cygnus and 2576 Σ. The motion is almost perpendicular to the direction of that of the sun in space.—On the wind system in the region of the Algerian chotts, by M. Angot.—Study of some derivatives of ethylvinyle, by M. Nevolé.—On a mode of transmission of the disease ergot, by M. Duplessis. The ergot appeared in a part only of a field of winter wheat. The previous crop having been a weeded crop and the field having borne, before that, trefoil and vetch (plants in which ergot has not hitherto been observed), M. Duplessis infers that the ergot must have been imported by a natural vehicle; probably the waters of the Loire, which overflowed this spring, brought it from some fields further up, which were affected by the disease last year.

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THURSDAY, SEPTEMBER 20, 1877

THE WORK OF THE IRON AND STEEL INSTITUTE

IT cannot be denied that of late years the component parts of that great aggregation of the contributions of workers in a thousand different fields, and which is known by the name of Science, have arrived at their common destination as much through the paths opened up by the development of the applied sciences as through those of original research or the seeking after scientific truth for the truth's sake.

In this country the institutions set apart for the furtherance of the applications of science to the use and convenience of man form collectively a very powerful body. Their influence upon the greatest industries of the country renders them indispensable to trade and finance, and that connection places at their disposal large means, both monetary and influential, without which many of the most important scientific researches could never have been attempted. The mere mention of the names of the Institution of Civil Engineers, the Institution of Mechanical Engineers, the Institute of Naval Architects, the Society of Telegraph Engineers, and the Iron and Steel Institute, suggests at once to the mind wealth, and influence, and scientific progress; and it is not too much to say that the wealth and influence of all those institutions has been brought to bear upon the advancement of applied science with remarkable success.

The Iron and Steel Institute ranks second to none for the importance of its objects to the welfare of the country, for the scientific value of its papers and discussions, and for the influence of its members. It was founded rather more than eight years ago for the purpose of advancing the knowledge of the manufacture of iron and steel, by bringing before its members the latest inventions and methods of working adopted in different establishments, for the encouragement of scientific research bearing upon the manufacture of iron and steel; whether in the domains of geology, metallurgy, chemistry, mechanics, or physics, and for the improvement of the operations of mining, smelting, and working the ferruginous ores of the country so as to obtain the highest perfection of the products at the smallest expenditure of capital and labour.

The field which the Iron and Steel Institute set itself to cultivate was partly covered by the domains of the institutions devoted to the advancement of the above sciences respectively, but a very large portion of it was never reached by any of them; and even the knowledge derived from discussions in other societies was too general to be of much practical value to the iron and steel manufacturer. No greater proof could be given of the need of such an institution in such an iron-working country as this than the very rapid rise and progress which it has made. It includes in its list of members, we believe without exception, all the leaders in the iron and steel trade of this country, as well as many of the eminent workers on the continent of Europe and in the United States of America; and among its most active members are the leading metallurgists and several of the first chemists, physicists, and engineers.

There cannot be any doubt that with so influential a body of members, with such varied and important objects, bound up as they are with the largest commercial interests of this great country, and with so many exceptional advantages, the Iron and Steel Institute has many and great responsibilities. Its only connection with trade is a scientific one (for the commercial interests of the iron and steel industries are represented by the British Iron Trade Association), and commercial matters have no right to be introduced into its discussions except in so far as accurate scientific knowledge, sound technical experience, and correct mechanical manipulation, by producing perfection in the products, and economy and certainty in the various processes employed, enable a manufacturer possessing these advantages to outdo those who do not, and by the guidance of scientific truth to command the market for his productions.

The iron and steel trade, represented in its scientific aspect by the Iron and Steel Institute, owes to pure science a deep debt of gratitude; but through that valuable institution it is year by year repaying that debt, at one time advancing the science of metallurgy, at another that of mechanics, at another adding to chemical knowledge, and so on. And we have little doubt, judging of its future by its past, that it will make still greater additions to scientific knowledge, in consequence of its increasing scientific organisation and the habit of scientific thought which it engenders among manufacturers and others by its meetings and published transactions.

The work of the Iron and Steel Institute is necessarily very varied. First and foremost it should promote, by every means in its power, the technical education required to make its various processes and operations understood by those who are entering the profession, and who in a few years will be its representatives. The importance of this can hardly be over-estimated. And it should be given in no grudging or half-and-half spirit; the days of the monopoly of knowledge, more especially of such as is based upon philosophical research, are long past, and though there must necessarily be some trade secrets, the results of knowledge and experience, still the knowledge which led to them is patent to all; and a technical society which has the best interests of its members at heart must be foremost in encouraging the dissemination of the scientific and technical knowledge required for advancing the interests which it represents. Another point for the consideration of such an institution is economy in fuel, and in the various processes employed in the making of iron and steel. In these days of close competition, when not only individuals but whole countries are doing their utmost to undersell one another, and a saving of a few shillings here and a few pence there may mean the difference between a profitable or a losing adventure, it becomes of the utmost importance to be working in a direction which is guided by scientific knowledge and proved by experiment and practical experience.

Then again the encouragement of the investigation of the physical and chemical laws which come into play within the smelting, puddling, and converting furnaces is a most important duty for the Institute to discharge, for if the amount of fuel consumed in proportion to the amount of metal produced can be reduced by only one per cent. an important step is gained and in large works a large addition to the returns is insured. The knowledge, however, necessary for such improvements can only be obtained through long experience, patient investigation, and the collection of data and information furnished by many workers towards the same end but in different paths.

The questions of the proper application of the hot blast and of the utilisation of waste heat are of the utmost importance, and with regard to the former the knowledge of the laws of radiant and specific heat has taught the engineer that economy in blowing and other engines can only be obtained by high piston speed, and that far greater advantages are to be had from small engines working at a high velocity than from engines of larger capacity working slowly.

No one has done more to advance the science of the iron and steel manufacture than the distinguished president of the Iron and Steel Institute, Dr. C. W. Siemens, F.R.S. His regenerative gas furnace, which is now made use of in so many industries in this country, in America, and on the Continent, is at the same time one of the most beautiful applications of science to industry that has been made in this or any other age, and one of the most, perfect economisers of fuel that has ever been invented. Then again the process of steel manufacture worked out by Dr. Siemens, and which bears his name, is the only equal competitor with the Bessemer process over which it possesses one great advantage, and that is that the progress of the operation may be watched and tested from commencement to conclusion, and may be arrested at any moment.

It is, of course, needless to point out the vital importance to the steel manfacturer of a knowledge of the chemistry of the blast, converting, and heating furnaces, and of the chemical properties of various qualities of iron, of various fuels, of spiegeleisen and ferro-manganese, and of various "*fettlings*," or furnace-linings. In connection with this subject the Iron and Steel Institute has done and is doing most valuable work, and the names of Mr. Edward Riley, of Mr. Isaac Lowthian Bell, M.P., F.R.S., of Dr. Percy, F.R.S., of Dr. Siemens, F.R.S., of Mr. Mushet, of M. Gautier, of Mr. A. L. Holley, and others, who have given to the Institute valuable communications both in papers and in discussion, will always stand foremost in this branch of scientific labour.

Again the construction of furnaces and the various mechanical systems of puddling, manipulating, rolling, and working the product in its various stages of manufacture, all call for the aid of science, and the Iron and Steel Institute is in no way deficient in recognising the importance of science to this branch of the profession, many interesting papers having been read and valuable discussions held in connection with this subject.

One great result brought about by the Institute is the establishment in nearly all the principal works of the country of experimental work and laboratory practice. It has, ever since its establishment eight years ago, been dispelling the clouds of darkness and of the "rule of thumb," and by allowing the light of science to illuminate the road has spread far and wide a desire for accurate scientific knowledge based upon practical experiment. But there is much yet to be done. There are still many manufacturing firms turning out many tons of metal in the course of the year who have exceptional advantages for the promotion of scientific knowledge, but who make no use of it; the advantages, however, which such an institution as the Iron and Steel Institute brings to all manufacturers must in time be recognised by all, and we cannot but believe that with all the national resources and with the advance of knowledge promoted by such institutions, this country will still be able to hold her own against foreign competition.

Much has yet to be learnt about the behaviour of many of the elements during the various processes of the iron manufacture, whether in the form of alloys or merely as substances present in the converter or puddling furnace. It would be well if the Iron and Steel Institute were to appoint a committee composed of practical analytical chemists and influential manufacturers of iron and steel, for the purpose of investigating the effect of various metallic alloys upon iron and steel as regards their tensile or compression strength, malleability, brittleness, &c. We feel sure that very interesting results would be obtained, and that the reports of this "Alloy Committee" would be found of great practical value to manufacturers, and would well repay the expenses incurred. It is well known that the metals chromium and tungsten form alloys with iron possessing valuable physical and mechanical properties; and the influence of carbon, silicon, boron, phosphorous, sulphur, arsenic, aluminium, and antimony, have been more or less examined; but there is very much yet to be learnt with regard to this subject.

The value of spectroscopic research in connection with the investigation of this subject can hardly be overestimated. It is, without exception, at once the most infallible and the most delicate test that has ever been placed in the hands of the chemist, and, when employed in conjunction with quantitative chemical analyses and with mechanical tests, cannot fail to clear away many of the mists with which that most mysterious substance, or compound of elements, which we know generally by the name *Iron*, is enveloped. Spectroscopic research presents several collateral advantages to the iron and steel manufacturer. There are many instances in which much valuable information may be obtained by its means without the progress of the various processes being disturbed. Nearly all other systems of testing require the taking of samples, and necessitate either the stopping of an operation at a critical time or the waiting until the process is perhaps too far advanced for the information gained to be of practical utility. The spectroscope, on the contrary, peering through the smallest crack, can detect all that is going on which concerns itself, and makes a report in unmistakable language, and before it is too late to be taken advantage of.

There is no better way of obtaining reliable results than by the systematic investigation by a committee which should certainly include in its list of members the names of Dr. Siemens, Mr. Riley, Dr. Percy, and Mr. Lowthian Bell.

There are so many important branches, both scientific and technical, of the iron and steel manufacture which come under the legitimate cognisance of the Iron and Steel Institute, the importance of which is every day increasing, that we cannot help thinking that such subjects as mining, mine ventilation, pumping and winding machinery may safely be left to the Institutions of Civil and Mechanical Engineers, as well as the consideration of the *applications* of iron and steel. The subject of corrosion is one, however, which, though rather appertaining to the finished product than to its manufacture, is one which should not be altogether overlooked, for if, by any variation in the process of production, the effects of corrosion may be diminished or modified, a new value will be given to the finished product.

There is one thing, however, which, as we hinted at the beginning of this article, has no place in the deliberations of a body holding a scientific position such as the Iron and Steel Institute, and that is the consideration of commercial questions as such and apart from the influence of science upon the cheapening of the cost of production or the lessening of labour. There must be, no doubt, in a society composed to so great an extent of men largely interested in the commercial aspect of the manufacture of iron and steel, a great temptation and tendency for the discussions occasionally to diverge into commercial questions; but it will be the duty of the president for the time being to check such digressions and to keep the discussions within legitimate channels; and it will be one of the objects of the council to allow no paper to come on for reading or discussion which is not calculated to advance the technical and scientific interests of the Institution over which it has been called to preside.

With the present council, and under the presidency of so distinguished a worker in science as Dr. Siemens, there is every prospect of the Iron and Steel Institute keeping up its high scientific character, and we cordially wish it every possible success. C. W. C.

COHN'S BIOLOGY OF PLANTS

Beiträge zur Biologie der Pflanzen. Herausgegeben von Dr. Ferdinand Cohn. Zweiter Band. Erstes Heft. (Breslau, 1876: J. N. Kern.)

THE first part of the second volume of Cohn's Beiträge contains five papers, two of them being illustrated with three plates each. The first paper is by Dr. Leopold Auerbach, "Cell and Nucleus," remarks on Strasburger's work, "Ueber Zellbildung und Zelltheilung." It is a critical paper, and hardly admits of any condensation. He tries to controvert the statements of Strasburger, and sums up thus:—1. The longitudinally striated body in the interior of the cell is not the "nucleus," but the middle part of the so-called "karyolitic figure," and therefore a product of the mixing of the special substance of the nucleus with the surrounding protoplasm; and 2, That the young nuclei do not develop by the fission of the mother nucleus.

The second paper is one of great importance, dealing as it does with one of the carnivorous plants. It is by Dr. A. Fraustadt. "Anatomy of the Vegetative Organs of Dionæa muscipula, Ellis," with three plates. As Dr. Fraustadt gives a very useful summary of results, we may here quote them. Each half of the lamina is slightly bent in a sigmoid manner, and forms a cavity to retain an insect, while the petiole is broadly winged and flattened. The cells of the epidermis, as well as those of the ground tissue, are elongated in the direction of the long axis of the leaf, in the petiole and midrib of the lamina, but in the transverse direction in the rest of the lamina. The cells forming the epidermis contain chlorophyll grains. The epidermis forms numerous stomata and stellate hairs on the upper and lower surface of the petiole, and under-surface of the lamina, but glandular hairs only on the upper surface of the lamina. The glands are placed in depressions in the epidermis, and are formed of a two-celled basal portion, a two-celled short stalk, and a round secreting part of two layers of cells convex on the upper side. The stellate cells are similarly constructed, except that the cells of the top layer grow out in radiating straight arms, giving the whole a star-like appearance. The stellate hairs appear early and are completely developed before the glands begin to form. The stellate hairs and glands are homologous structures. The lamina bears on its margin numerous (from fifteen to twenty) teeth or marginal setæ, and usually six spiny hairs (central setæ) on the upper surface. The marginal setæ are slender, triangular, pyramidal, and have stellate hairs and stomata on all sides. A fibro-vascular bundle is present running nearer the upper than the under side of the structure. Between each of the marginal setæ a single stellate hair is placed sometimes elevated on the top of a small projection, which, however, receives no fibro-vascular bundle. The central setæ consist of two parts, the lower forming a joint, and receiving an axile cellular string; the upper part is conical, contracted below, and has no cellular string. The cells of the central setæ show aggregation of the protoplasm (as described by Darwin in Drosera), as well as those of the glands. In the green parts of the petiole (above ground), and in the midrib of the lamina, the cells of the ground-tissue increase in length and in size of cavity from without inwards, the superficial cells, and those near the fibro-vascular bundles are green, the others colourless. In the lamina, with the exception of the midrib, the inner cells of the ground tissue are colourless, very broad, with sinuous walls and small intercellular spaces. The epidermal cells of the upper side of the lamina and the ground-tissue cells below it, are larger than those of the under side. The chlorophyll grains contain abundance of starch before the leaf has obtained any organic (animal) nourishment. The starch diminished after the reception of organic (animal) matter by the leaves, and lastly disappears entirely from the parts of the plant above ground. The bases of the petioles are dilated into colourless sheath-like portions developed underground and together forming a kind of bulb. The ground tissue consists entirely of equally broad and long cells completely filled with starch, as well before as after the reception and absorption of organic (animal) matter. The starch grains in the part above ground of the petiole and lamina are oval; in the basal sheathing part of the petiole, on the other hand, the grains are cylindrical or rod-like.

The living cells of the lamina and petiole contain a colourless substance dissolved in the cell-sap, precipitated by bases in the form of dark grains which are redissolved by acids. The glands contain no starch. The red colouring matter of the glands becomes converted into green by the action of strong bases as ammonia and potash, but is again restored by the action of acids. The colour seems, therefore, to be identical with the red colouring matter of plants so fully described by Prof. A. H. Church in a recent number of the *Journal* of the Chemical

Society, under the name of Coleine. Colourless glands become coloured artificially after the absorption by the leaves of red-stained albumin. The fibro-vascular bundles, as far as the petiole, are also coloured, thus rendering the absorption very evident. After death black granules form in the tissues of the leaf, visible as black specks on the surface.

In the midrib of the petiole a well-developed axile fibro-vascular bundle runs; from it straight branches proceed into the wings in a "curved veined" manner. The branches split into smaller and smaller twigs, the branching, however, not being symmetrical. In the midrib of the lamina there is a large axile fibro-vascular bundle, which gives off branches (at a right angle) running parallel towards the margin, where they fork and again unite. One bundle formed by the union of the fork-branches runs into each marginal seta of the leaf.

The phloem of the fibro-vascular bundle consists of soft bast; the xylem, in the lamina, chiefly of spiral vessels; in the petiole there are other vessels in addition.

In the youngest leaves the petiole and lamina are not separable. The part first formed, and springing from the flat vegetative cone represents the origin of the lamina, but it remains rudimentary for a long time, during which the petiole is rapidly developing at its base. The lamina at first forms a straight continuation of the stalk, then bending through an angle of 180° bends itself over into the grooved petiole. Afterwards it just reverses the process and straightens itself as it expands.

The margins of the lamina are in the early stages rolled inwards. Afterwards the petiole expands in a plane, and last of all the lamina becomes fully developed.

The stem is short and thick with a ring of xylem. The bundles pass transversely, so that one enters each leaf and each root.

The lateral rootlets are long and strongly developed, but never branch. The cells at the apex are red in colour, the cortical cells become brown in centripetal order and die in as far as the sheath of the fibro-vascular bundle. The vessels develop first at the periphery of the axile bundle, increase in a centripetal direction, and ultimately form an eight-rayed star.

The third paper is by Dr. J. Schroeter, "On the Development and Systematic Position of Tulostoma, Pers." It describes the structure of a very interesting little fungus which passes part of its life below ground, then the stalk elongates, and the open periderm, with a capillitium, appears above ground. The plant described is *Tulostoma pedunculatum*, L. (*T. brumale*, Pers., *T. mammosum*, Fries. Cooke). The sporocarps are developed at a depth of from two to three centimetres below the surface of the ground. They spring from a white, branched, thread-like mycelium, running between grass-roots, and old moss-plants. The mycelium gives rise to fusiform structures of varying thickness, and these pass over by all gradations into true sclerotia. The sclerotia give rise to the sporocarps, but the development was not observed; apparently, however, they bud out from a spot on the surface of the sclerotium. At first the sporocarp is like a small bovista. The spores are developed on remarkable basidia. These form four elongations springing from the sides at unequal heights, and each develops a spore. The basidia only last a short time, and a capillitium is developed in the interior of the peridium, the spores lying between the meshes. Tulostoma has been placed among the Gasteromycetes, in the Lycoperdaceæ, but the peculiar development of the basidia at once separates it from the Lycoperdaceæ, and Schroeter proposes to place it in a new group of the Gasteromycetes, the Tulostomaceæ. The curious genus, Pilacre, seems also to Schroeter to belong to the same division, and he further suggests the possibility of the remarkable genus Batarea being also related.

The fourth paper is one of the highest interest and deals with a most remarkable group of plants. Exceedingly simple in structure, they attack many algæ and water-plants, and seem not unfrequently to have been described as the fruits of algæ by certain algologists. It is by Dr. Leon Nowakowski: "Contributions to the Knowledge of the Chytridiaceæ," and is illustrated by three plates. The genus Chytridium, which gives its name to the group, consists of only one cell. Rhizidium consists of two cells, the lower forming a root-like or branched mycelium-like structure, while Synchytrium consists of a group of cells. Zygochytrium and Tetrachytrium of Sorokine are the most highly developed, and in them the zoosporangia are produced as a branched bearer. Nowakowski describes a new genus, Cladochytrium, in which a branching mycelium is developed in the tissues of the host-plant. Another new genus, Obelidium, has a stalk to the zoosporangium and a well-developed mycelium. The spores of Chytridium are formed by free-cell formation in the zoosporangium, and generally possess a very highly refracting nucleus. The zoospores exhibit, as first pointed out by Schenk, peculiar amœboid modifications of form. The zoospores have only one cilium either before or behind. Conjugation of zoospores has not been observed. When the spores germinate the nucleus gradually disappears and the whole spore either at once grows into a new zoosporangium, or a sort of mycelium is formed. Resting spores have been observed in Chytridia as in Rhizidium, and probably occur in others. The position of these plants is at present doubtful, but probably they are allied to Saprolegnia.

Nowakowski has described certain new forms and carefully-observed forms already described but not fully studied. The following is a synopsis of the forms described:—

I. CHRYTIDIUM, A. Br.

1. *C. destruens*, nov. sp., occurs in cells of a new green gelatinous alga, developing zoospores, and described by Nowakowski under the name of *Chætonema irregulare*.

2. *C. gregarium*, nov. sp., in the ova of a Rotifer; found among the gelatinous matter of *Chætophora endiviæfolia*.

3. *C. macrosporum*, nov. sp., also in the ova of a Rotifer; found among the gelatinous matter of *Chætophora elegans*.

4. *C. coleochaetes*, nov. sp. In the oogonia of *Coleochaete pulviratum*, and never in the vegetable cells.

5. *C. microsporum*, nov. sp., a specimen of *Mastigothrix æruginea*, Ktzg., found in gelatinous matter of *Chætophora elegans*.

6. *C. epithemiæ*, nov. sp., an *Epithemia zebra*, one of the Diatomaceæ.

7. *C. mastigotrichis* nov. sp., a *mastigothrix æruginea*, as in No. 5.

II. OBELIDIUM, nov. gen., Now. The one-celled zoosporangium is elevated on a more or less developed bearer from the middle of a star-like dichotomously-branched mycelium, which radiates in a single plane. The zoosporangia are separated from the mycelia by a transverse wall. The zoospores are developed in small numbers, and escape by a lateral opening.

1. *O. mucronatum*, nov. sp. In the empty skin of a gnat-larva.

III. RHIZIDIUM, A. Br.

1. *R. mycophilum*, A. Br., is fully described and figured, and the resting-spores traced through their long period of repose.

IV. CLADOCHYTRIUM, nov. gen., Now. The zoosporangia are either developed as intercalar swellings of the one-celled mycelium in the tissue of the host-plant, and separated by transverse walls, or they are terminal at the end of single mycelium threads. The zoosporangia dehisce either by the opening of a long neck, or by a lid. Secondary zoosporangia are developed either in rows or in the interior of old empty zoosporangia.

1. *C. tenue*, nov. sp., in the tissues of *Acorus calamus*, *Iris pseudacorus*, and *Glyceria spectabilis*. Closely related to *Protomyces menyanthis*, De Bary found in the leaves and petioles of *Menyanthes trifoliata*.

2. *C. elegans*, nov. sp., in the gelatinous substance of *Chætophora elegans*.

The last paper is by Prof. Cohn himself—"Remarks on the Organisation of Certain Swarm-Cells." It is chiefly devoted to an account of *Gonium tetras*, A. Br., and certain subjects suggested by the examination of that plant, such as the nature of the "amylum kern," or starch nucleus, the inner organisation of swarm-cells, the cavities and contractile vacuoles in such cells, and the comparison of swarm-cells with one-celled animals. The whole number is one of great interest and will well repay perusal.

W. R. MCNAB

OUR BOOK SHELF

Physiography and Physical Geography. By the Rev. Alex. Mackay. (Blackwood and Sons.)

IN his preface the author draws attention "to the peculiar character of the present work," and quotes by way of explanation two paragraphs from the Directory of the Science and Art Department. He remarks that "the student will at once perceive that the author has discussed all the subjects embraced in the new syllabus" of the department. The spirit of this discussion and "the peculiar character of the work" will be best appreciated from a few extracts.

"The combined result of various experiments gives to the earth a density of 5·66 times that of water. But more reliance should be placed on the number indicated by the Great Pyramid, which in this as in so many other great cosmical data, has anticipated modern science by more than 4,000 years." "The sacred volume declares that in the days of Noah the whole world was inundated by a flood, which covered the highest mountains, and that, with the exception of one family, the entire human race was destroyed. A change in the inclination of the earth's axis would certainly produce such a catastrophe—a catastrophe which was accompanied with direful results to all future generations; the alternations of heat and cold became so rapid as to affect the longevity of man, which has from that date gradually shortened from nearly a thousand years to three-score years and ten." "Why the planets move in elliptical orbits" is the title of a paragraph, which, containing no reference to nor explanation of the *ellipticity* of the planetary orbits, is embellished with a diagram to show why the orbits are *circular*. "Mountain-chains of the same geological formation are believed to be of the same antiquity; and, however widely separate, are parallel to one another." "The slow increase in the saltness of the ocean may account for the otherwise inexplicable fact that frequently since the ocean became inhabited, its varied population became wholly or almost wholly extinguished." "The antiquity of the human species as indicated by geological evidence, no doubt conflicts with the chronology of Usher, founded on our modern Hebrew text. In the matter of antediluvian chronology, however, the Hebrew text has, in all probability, been tampered with, as we have shown at large in a separate work ('Facts and Dates,' p. 62-69). The Septuagint translation—a translation sanctioned by our Lord and his Apostles—assigns to our race an antiquity of nearly 1,500 years more than Usher does. Science is giving its emphatic verdict, in this particular, in favour of the Septuagint; and though the extended chronology may fail in meeting all the difficulties of the case, it will certainly meet many of them . . . Geologists are too apt to toy with millions of years as if they were playthings, and to show no regard to moderation or common sense. Science has not hitherto been able to determine the actual antiquity of the planet, and probably never will."

The Book of Algebra. By A. T. Fisher, B.A. (London: Stewart's Local Examination Series, 1877.)

MR. FISHER has aimed at writing a short work on algebra for students who have no intention of reading high mathematics. He has done his task well, and the result is a compact and carefully put together little book. The limit he has set himself is to enable a reader to understand all that is required as preliminary to the solution of higher simultaneous equations; hence we have nothing on the Progressions, Notation, Permutations, &c. On a perusal of the work we have been especially struck with the care taken by the author to bring out a book burdened as little as possible with mistakes. For three-fourths of the book he has been assisted by the printers, but in the chapters on surds, indices, and higher equations we have noticed a plentiful crop of typographical errors. Most of these are, however, easily corrected. There is an unfortunate mistake of + for × twice on p. 47; of − for ÷ in Ex. 14, p. 52.

Some readers would require a larger number of examples; those that are given are, on the whole, very well chosen, and there are some useful problems neatly solved. It is possibly an objection, certainly in the elementary parts, that the answers immediately follow the questions. The book is neatly, and for the most part carefully, printed.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iii. No. 2. (Washington, 1877.)

THE second number of the above *Bulletin* contains three important entomological articles from the pens of Messrs. Osten Sacken, Uhler, and Thorell.

The first memoir, from the pen of that distinguished Dipterist, Baron C. R. Osten Sacken, bears the modest title of "Descriptions of New Genera and Species of Diptera from the Region West of the Mississippi, and especially from California," but he who takes up the paper expecting to find nothing but bare descriptions will be agreeably surprised to find it interspersed with analytical tables of the Diptera of the United States, with diagnoses and critical notes on many species already known, with remarks on their geographical distribution, synonymy, and in fact anything that could in any way contribute towards rendering this order of insects clear of comprehension or attractive to the student.

The second article, by Prof. P. R. Uhler, is a report on the insects collected by himself during the exploration of 1875, including monographs of the hemipterous families *Cydnidæ* and *Saldæ*, and an account of the hemiptera collected by Dr. A. S. Packard, jun. The monograph of the *Cyanidæ* commences with an excellent conspectus of the genera of that family, followed by detailed descriptions of both genera and species; two well-executed uncoloured plates accompany the paper.

In the third memoir Dr. T. Thorell gives an account of the *Araneæ* collected in Colorado in 1875 by Dr. Packard; the descriptions of the species are drawn up in the author's usual careful and exhaustive style, and leave nothing to be desired but illustrations, the absence of which we cannot but deplore; an appendix by Mr. J. H. Emerton (the well-known American arachnologist) describes two additional species of the genera *Epeira* and *Drassus*, with which two woodcuts are given.

A. G. BUTLER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Temperature of Moon's Surface

IN a recent number of *Les Mondes* (tome xliv., No. 1, September 6, 1877), M. l'Abbé F. Raillard puts forward a theory to explain the reddish tinge acquired by the moon during a total eclipse, attributing it possibly to the great elevation of temperature caused by the continuous exposure of its surface for many days previous to the solar rays, which he thinks may be adequate so to raise its temperature as to render it self-luminous.

In support of his theory he refers to my experiments with the thermopile, and states that I have found the lunar surface to acquire under solar radiation a temperature of more than 500° Centigrade. Now in a paper published in the *Proceedings* of the Royal Society, No. 112 (1869), I estimated the radiation to be equal to that of a lamp-blacked surface 500° Fahrenheit higher in temperature at full moon than at new moon, but on repeating the experiments[1] with more care, 197° Fahrenheit, or 100° Centigrade, was found to be a far more probable value, a large error having crept into the former result. It is, moreover, shown in my last paper that near the middle of the partial eclipse of 1872, Nov. 14, the radiant heat was only about one-half of what it had been two hours before, having kept pace in its diminution with the light. Observations made during the recent eclipse, so far as they go, fully confirm this result, and I much doubt if five per cent. of the heat acquired since new moon is retained till the middle of a total eclipse; heat, too, which we have shown from its low mean refrangibility as compared with that of the direct heat of the sun to have been truly *absorbed* by the lunar surface.

M. l'Abbé Raillard appears to be mistaken in supposing it to be the generally received theory that the red tinge is due to dispersion rather than to simple refraction and preponderant absorption of the more refrangible rays in passing through the earth's atmosphere.

There appears to be, therefore, no ground for supposing that the difference between the "lumière cendrée" of the unilluminated surface of the new moon and its reddish hue during a total eclipse is to be ascribed to a difference of temperature, and I think that we must fall back on the usual explanation.

It may also be expected that independently of any tinge due to unequal absorption by the earth's atmosphere the preponderance of blue and green on the terrestrial surface may not be without influence on the colour of the "earth-light" which gives rise to the "lumière cendrée" and may contribute to an appreciable degree towards forming a contrast between its hue and that acquired by the moon when totally eclipsed.

September 15 ROSSE

Rainfall and Sun-Spots in India

As Prof. Balfour Stewart says the true test of a physical cycle is its repetition, and since he evidently regards the tendency to repetition which he has shown to exist in the rainfall of Madras as a favourable indication of the presence of a physical cycle such as that claimed by Dr. Hunter, I may perhaps be allowed to supplement my former statements regarding the tendency of the winter rainfall in many stations of Upper India to vary in a cycle corresponding inversely with the solar spots, by exhibiting a similar tendency to repetition in the rainfall of Calcutta. The following table represents the winter rainfall of Calcutta from 1833 to 1876.

The rainfall is taken for the months of January, February, March, and April in each year, together with that for December of the preceding year. The November fall is excluded chiefly because experience and an inspection of the register show that it properly belongs to the summer monsoon rainfall, occurring almost entirely in those years in which the summer monsoon rains are either very heavy or prolonged, and in fact being nothing else than the last drop they shed before they take their departure. The real winter rains commence in the Christmas week, so December really includes their actual first appearance. As the summer rains seldom begin before the second week in June, we are well within correct limits in taking the rainfall from December to April inclusive. The following table is arranged after the model of that given by Prof. Stewart in his letter to NATURE (vol. xvi. p. 161):—

Years employed.	Years of Series—Calcutta.											
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	
1833–43	4·34	7·34	3·12	2·97	2·16	1·98	3·19	1·24	5·11	7·49	6·69	A.
1844–54	4·51	9·21	6·30	3·85	1·77	6·75	5·79	7·28	9·50	1·60	9·54	B.
1855–65	5·54	3·91	2·76	1·80	7·26	2·56	1·75	5·51	3·83	3·42	8·58	C.
1866–76	7·46	3·21	5·86	8·41	4·83	6·47	5·08	3·02	8·68	5·45	7·49	D.
Whole period average.	5·46	5·91	4·51	4·25	4·00	4·44	3·95	4·26	6·78	4·49	8·07	

The years of minimum sun-spot occur in the first and second series, and the years of maximum sun-spot in the fifth and sixth series. The series of heaviest average winter rainfall are 9, 10, 11, 1, 2, and those of lightest average rainfall are 5, 6, 7, 8. Taking the mean of the averages of the five series of heaviest rainfall we get 6·14 inches, and taking the mean of the averages of the four series of lightest rainfall the result is 4·16 inches. The same result is exhibited by each cycle individually, thus:—

	Max. group, inches.	Min. group, inches.
Cycle A	6·19	2·14
,, B	6·87	4·31
,, C	5·05	4·27
,, D	6·45	4·85

The evidence of repetition is thus quite as manifest as in Dr. Hunter's case, the only difference being that in the present case the years of minimum sun-spot are those of heaviest, and maximum sun-spot those of lightest, rainfall. In order to render it still more apparent that the cyclical connection with the sun-spots is not the result of accident I will exhibit the difference between the rainfalls in years of absolute minimum and maximum sun-spot:—

Years of minimum sun-spot.	Total of the five months.	Years of maximum sun-spot.	Total of the five months.
	inches.		inches.
1834 1844 1856 1867	18·97	1837 1848 1860 1870	11·32
Average for each year	4·74	Average for each year	2·83

The same connection is maintained when the fall in April is left out or that in November included, so that it is evidently not due to the effect of any particular month, but as may reasonably be inferred, an indication of the presence of some physical cause tending to increase the rainfall in years of minimum and diminish it in years of maximum solar maculation.

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[1] *Philosophical Transactions*, 1873.

Bankipore, Patna E. D. ARCHIBALD

The Australian Monotremes

IT is certainly news to me, and I believe to most other European naturalists, that *Tachyglossus* and *Ornithorhynchus* occur in Northern Queensland. Perhaps W. E. A. will kindly state, for our information, the exact spots where they have been discovered and their extreme *northern* limit, so far as this has been ascertained.

W. E. A. speaks of an adult female *Echidna* (*sive Tachyglossus*) having “a fine healthy young one in the pouch.” Is there not some error here, as the monotremes have, strictly speaking, no marsupial pouch? P. L. S.

WITH reference to the existence of *Tachyglossus* (*olim Echidna*) in North Australia, and the recent discovery of one (or possibly two) species in New Guinea, the following account, which I lighted on a few evenings ago, when looking over an old volume of the *Field*, seems to be of sufficient interest to warrant its transfer to the pages of NATURE. The account in question occurs in an article “A Week at Plain Creek, Queensland,” by Mr. E. B. Kennedy, which appeared in the issue of that journal for September 20, 1873. It runs as follows:—“ . . . Whilst so engaged we heard our dogs making a tremendous noise, high up the bank in the scrub, and upon going to ascertain the cause found them scratching, yelling, and pulling at a porcupine which was half imbedded in a hole; we were at least ten minutes digging him out with sharp-pointed sticks, such was his tenacity in holding on and burrowing. The quills were not nearly so long as the Cape of Good Hope species (of course a true *Hystrix*), and he differed from that quadruped in having a sort of beak instead of a regular jaw.” It is to be regretted that Mr. Kennedy did not preserve his specimen, which was ultimately cooked and eaten! I should have mentioned that Plain Creek lies in 21 lat. S., so that this is certainly the northernmost locality on the Australian continent, where we have certain knowledge that the *Echidna* occurs. As we now know that many North Australian species of birds range also into southern New Guinea, it would hardly be surprising if the *Tachyglossus* of the Fly River and south New Guinea were nothing more than the well-known *Tachyglossus hystrix*. It is to be hoped that this point may soon be solved by the arrival of specimens from both localities. W. A. FORBES

English Names of Wild Flowers and Plants

To all who are interested in the history of the English language the derivations proposed for the vernacular names of many plants in the Rev. W. Tuckwell's lecture (see NATURE, vol. xvi. p. 385) will be highly appreciated. And even in the few cases where the etymologist may feel doubtful as to the verisimilitude of the suggested pedigree it will for the most part be difficult to propose another with any great confidence.

There is, however, one of these doubtful cases, the derivation of woodruff from wood-roue, in lieu of which I have to offer a conjecture which appears to need no lengthy argument to insure its acceptance.

Is not the ruff of woodruff identical with the riff of sherriff? Is not, in short, the woodruff the wood-reeve, just as the sherriff is the shire-reeve? That the German wald-meister has the same connotation and is applied to the same plant is evidently a striking confirmation of this view, and it would be interesting to know whether the word wald-graf (*i.e.*, wald-ge-raf = wood-y-reeve), or any equivalent form, is to be met with in high or low German literature.

I used to be told by a school-fellow that the way to spell woodruff was—

Double U, double O, double D, E,
Double R, double O, double F, E.

Even under the disguise of woodderrooffe, however, the origin of the word is perceptible.

As regards the main purpose of the Rev. W. Tuckwell's paper, I feel strongly that scientific accuracy is compatible with a much freer use of vernacular words than is customary amongst us, and that their adoption by science teachers would remove a great stumbling-block from the path of learners.

Manningham, September 10 J. WILLIS

Some of the Troubles of John O'Toole respecting Potential Energy

“IT is the people's *right* to demand of their teachers that the information given them shall be, at least, definite and accurate as far as it goes,” and “whenever there appears to be a confusion about fundamental principles it is the *duty* of a scientific man to endeavour by all means in his power to remove it.” These are the words of one of the teachers.[1] I am one of the people—as indeed, my name testifies, Toole (*Tuathal*) being the Irish equivalent of the Latin *Publius*—and I would now, on behalf of myself and every brother Publius, assert our above “right” in respect of the matter now in hand, and demand the performance by the doctors of their corresponding “duty.” Now there is much “confusion about the fundamental principles” of physical Energy in the minds of the public who care about such things; and it is principally, though I admit not entirely,[2] the doctors who are to blame for this. Their own ideas on the subject being so clear and correct they are superior to the phraseology they use respecting it, and they are not injuriously affected thereby; but those who are dependent on that phraseology for their knowledge are in very different case. Let me, as one of the latter, point out some of the perplexities under which we labour from no fault of our own, and which we should be spared if our teachers would only condescend to use their words discreetly and consistently. It may be well to premise that we know the definition of physical Energy, which is—“the power or capacity of performing work;” and that we are not now making any confusion between Energy and force.

The word “potential” has two very different meanings—(1) Of, or belonging to, potency or power; (2) Existing *in posse*, or in possibility, as opposed to existing *in esse*, or in actuality; and the expression, “potential Energy,” can have no less than three references or meanings, which we shall mark with A, B, and C; and each meaning has its own proper inconveniences independent of the perplexities arising from their mutual relations.

A.—*Potential E., as meaning “Energy existing* in posse.”

The phrase “potential E.” is in the first place very generally intended to mean E. existing *in posse*, according to one proper signification of the word “potential.” The phrase was first used by Rankine,[3] and apparently in this sense; he contrasted “potential” and “actual” E. This antithesis is still very generally implied and sometimes expressed. Clerk Maxwell tells us[4] (the statement being *repeated* only last year[5]) that “potential E.” “signifies the E. which a system has not in actual possession, but only has the power to acquire.” Wormell says[6] —“It has been aptly called possible or *potential* E., because it represents the power the body has of acquiring actual or kinetic E.” Many of our doctors use the phrase “potential E.” without explaining it, and of course, unless there be some particular reason to the contrary, such must be understood to give it, as one of its significations, at least, the original meaning intended by its proposer (or if not they are guilty of a very misleading omission, *utrum horum mavis accipe*); and this is especially undeniable in the case of those who apply the title “actual” to the other type of E. Balfour Stewart, though he seems to have quietly dropped the name “potential,”[7] has really retained the idea implied thereby, for he still habitually calls the other type of E., that of motion, “actual E.,” as its *distinguishing* title. Moreover, this idea is involved in other statements, &c., of our teachers. For instance, we occasionally find language used which seems to imply that potential E. must first emerge as actual E. before it can produce work, as by Deschanel,[8] by Dunbar Heath,[9] and by Balfour Stewart.[10] Observe, also, the expression “E. of actual motion,” which is frequently used by the last-mentioned doctor,[11] and accepted at least by Tait.[12] “E.

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[1] Tait Evening lecture during meeting of Brit. Assoc at Glasgow in 1876. NATURE, Sept. 21, 1876.

[2] That brother Publius who wrote the article on Tyndall's “Heat,” in *Blackwood's Mag.*, December, 1863, was partly responsible for his own confusion about Energy.

[3] *Phil. Mag.*, February, 1853. He says: “All conceivable forms of E. may be distinguished into two kinds, actual or sensible, and potential or latent.”

[4] “Theory of Heat,” p. 91, 1871.

[5] “Matter and Motion,” p. 81, 1876.

[6] “Dynamics,” p. 185.

[7] At least it never occurs in his “Conserv. of Energy,” 1874, though frequently in “Elem. Physics,” 1870.

[8] “Nat. Phil.,” p 78, edition of 1870.

[9] “Energy,” p. 64. [10] “Elem. Physics,” pp. 104-106. But see p. 360

[11] “Conserv. of Energy,” p. 25, and elesewhere.

[12] “Unseen Univ.” (last edition), p. 109, twice.

of actual motion," as a *distinguishing* title, cannot mean any-thing else than this—that the other E., potential E., is E. of about-to-supervene motion, or that it does not perform work except through the resulting E. of motion. We do not say that these doctors intended what we have mentioned, but their words unquestionably go to convey that impression; and what makes this so particularly mischievous is that poor Publius is already so susceptible to that impression, being prepared for it by the titles "potential" and "actual" E.

I should not be at all surprised if some would try to argue that the phrase "potential E." need not be taken to mean more than simply this, viz., that the E. so called exists in possibility *relatively to the body or system* that may be in question, that its potentiality merely implies that it is absent from and acquirable by that body or system, and not that it is altogether out of actual existence.

1. Now even supposing this to be true, though I have never seen any evidence of it, and even if we should grant this to be a right usage when the *body* or *system* is followed through the history of its changes, it is a wrong usage when, as in a book or chapter on E., Energy is the subject, when it is the conserved E. *itself* which is to be followed through its migrations. Why should this grand conserved E. be stigmatised as merely potential when it does not happen to be in a certain mass? Relatively to that mass it may be some times potential, but relatively to itself it is, as we shall see, always actual.

But we cannot concede that the potentiality of this mode of E. implies merely the above. I believe it is usually intended to mean much more; and, at any rate—whatever those who use the word may intend—it logically involves much more; and this is what poor Publius is chiefly concerned with. Now if we consider the words of Clerk Maxwell quoted above, we shall see that in the case of a separate unconnected system, such a statement coming from him cannot possibly mean that the said E. is in actual existence outside of the system, and is waiting there until the system takes possession of it. If it is not in the system there is nowhere else where it can be; therefore it is not in actual physical existence at all, although connected with existence by some inconceivable paraphysical link. The acquiring of it is a kind of creation of it. Curiously enough Stewart and Tait[1] speak of the "creation" and "annihilation" of both types of E.

2 Here, then, comes our second complaint. E. is "the power of performing work;" therefore potential E., which is intended to be the power of acquiring E., is the power of acquiring the power of doing work. E. is already a potentiality; therefore potential E. is a potentiality which, itself, exists only in potentiality. There is here a double remotion from tangibility, which may be gratifying to the metaphysicians, who rejoice the more the harder the nuts you give them to crack; but poor Publius finds *bonnes bouches* of this sort rather trying to his molars.

3. Potential E., in the present sense, being, as we have seen, undeniably out of actual physical existence, poor P. does not feel that he has gained much when he learns that the sum of the actual and potential E.s of the universe is a constant quantity—for this is the form in which the grand principle of the conservation of E. is usually, or at least frequently, presented to him by the doctors. A rigid physicist, who himself believes in nothing but the physical, teaches poor P. something which compels *him* to stand with one foot on the land of physics and the other in the sea of metaphysics, in order to reach it all. This *teacher* forces poor P. to recognise the metaphysical, while he scorns to do so himself. The combination of the two characters of conductor and of pure finger-post, in the same person puzzles Publius a good deal. Sometimes, when poor Publius thinks that he has grasped the principle in the above form, it seems to him to turn out only a truism, after all; and indeed no less a man than Sir John Herschel sympathised with him in this idea.[2] I am not sure that they are right; they seem to overlook that this potential E., though undeniably out of physical existence, is by some mysterious paraphysical operation, recoverable in its former quantity. However, P. and Sir John are right, so far, that the doctors will sometimes inadvertently allow themselves to present a physical principle of E., which is very far from self-evident, in a form which has all the appearance of a logical truism; *e.g.* when we are told that "the E. exerted is equal to the work performed." P. says I could have told you that from the definition of E., which is "the power of performing work."

4. Potential E. being that which is not had in actual possession by the body (or system) in question, how can that body be "a store of potential E.?"[1] How can the body contain that which is not in it? The doctors should explain this. However, I am glad to find that my cousin Barney was not so wrong, after all, when he complained that Ireland was swarming with absentee landlords.

5. But to pass now from *à posteriori* objections to the phrase "potential E." in the present sense. This potential E. is so called to *distinguish* it from actual E. so called, and yet it is just as immediately and directly efficient in performing its work as actual E. itself, and, therefore, as truly actual as any E. can be. When a certain quantity of potential E. is followed by its equivalent actual E., what is the actual E. of the body but the *direct work* of the potential E. done against the inertia of the body? It is from the doctors themselves, of course, that I learn this. And yet it is very curious to observe how often they shrink from directly stating this, and how ingeniously they will avoid it (one doctor actually denies it). They will sometimes tell you that the potential E. is "*transformed*" into the subsequent actual E. and *v. v.* Sometimes, when they feel that this evasive euphemism is unsuitable to their immediate purpose, they will use what I, with the utmost deference as well as difference, hold to be the proper word, viz., "transfer"; but having made this concession they refuse to proceed further, and shirk telling us from what or to what the transference is made (more of this presently). As we have said the kinetic E. of the moving body is the direct work of the equivalent potential E. that preceded it; and if the work be, as it is, actual, the E. must be so too; as long as we remain in the realm of physics.

But more than this; the potential E. of a mass, as it is expressed, can do other direct work than that of producing actual E. in the mass concerned. Take the case of a clock weight, which is so often adduced, though never, as far as I know, for the purpose of illustrating its own proper lesson. When wound up it has, as we are told, potential E.; but in its descent, while working the clock, it never acquires more than the indefinitely small quantity of actual E. which is due to its excessively slow motion; and this actual E. is doing no work during the descent, since the velocity of descent is uniform. The only work that this actual E. performs is to produce an infinitesimal amount of heat at the instant of the weight's reaching the lowest point of its descent; that is to say, when the clock has stopped. This is only one instance of a whole class of cases in which, as it is expressed, a mass does work by means of its potential E. which exists only in possibility, without ever having any actual E. which it can apply to that work! Moreover there are cases outside of molar physics in which it is not yet known, for certain, whether the E. present is conventionally actual or potential; and yet, in either case, the work is done immediately and directly; and therefore the E. is truly actual whether conventionally so or not. Therefore "potential E.," in the present sense, is a wrong title for this or any mode of E., and this being so, "actual E.," as the *distinguishing title* of the other E., is wrong too; since both are actual.

6. There are a very few of our doctors who use the name "potential E." with another reference solely, and who, as it would appear, designedly abstain from giving it the meaning of "E. existing *in posse*," probably on account of some of the inconveniences we have mentioned; and yet they will use epithets which at least tend somewhat in the same direction. They speak of it as being "E. of repose"[2] (meaning of course reposing E.), as being "of a quiet nature,"[3] "dormant,"[4] "quiescent,"[5] "tranquil,"[6] and "passive"[7] (!), in opposition to the other type of E., which they correspondingly call "active"[8] and "living."[9] Now poor Publius is strongly inclined to think that if *he* had spoken thus they would have said that he had not yet got hold of the precise scientific meaning of E. It seems to him, though he trembles to say it, that although in popular usage the phrases, "quiet," "dormant," &c., and "active energy" may do very well, and convey a correct meaning, viz., that intended by the

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[1] "Unseen Univ.," p. 114.

[2] "Familiar Lectures," p. 469. See Rankine's answer to Herschel, *Phil. Mag.*, February, 1867.

[1] Thomson and Tait (*Nat. Phil.* p. 178) virtually say this, but with them pot. E. does not mean E. existing in possibility. So they are all right in doing so.

[2] Balfour Stewart, "Cons. of E.," pp. 27, 143. [3] *Op. cit.*, p. 23.

[4] Stewart and Tait, "Uns. Univ.," p. 109; also Tait's "Glasgow Lect."

[5] *Op. cit.*, p. 111. [6] Do., p. 147. [7] Tait, "Glasgow Lecture.

[8] "Uns. Univ.," p. 111; Tait, "Glasg. Lect.," and Tyndall, "Heat," 2nd edition, p. 140. [9] Stewart, "Cons. of E.," p. 27.

speaker, yet that physical E., according to its definition, is not capable of having such epithets applied to it, except in senses which are not intended by those doctors. Active E. would not be E. or "the *power of performing* work," it would be rather the *performing* of that work. "Active E." being thus incorrect, its above antithetics, or approximate antithetics, are incongruous expressions, or else have meanings different from what is intended. If "reposing," "dormant," "quiescent" E. have any meaning, it is that of "*unavailable* E." If "quiet" and "tranquil" E. have any meaning, it is that of E. spending itself slowly and equably. Poor P. thinks that the expression, "passive E.," would sound very like a bull, whether used in a tap-room or in a lecture theatre. He dares not entertain the suspicion that these expressions had their origin in a momentary, latent, unconscious confusion between kinetic E. and action in the minds of the writers; but he knows that they are eminently calculated to cause a chronic intentional muddling of them both together in his own brain-pan.

Dublin (*To be continued.*) X.

On the Supposed Action of Light on Combustion

THE results obtained in the experiments mentioned by M. C. Tomlinson are to be attributed to the elevation of temperature of the candles exposed to solar *light* and *heat.*

The influence of light on combustion has been mistaken for the action of heat, which, in this instance, seems to have accelerated combustion, and in other instances *retards it* by increasing *the heat of the air* and diminishing the *draft.* That is why the sun shining over chimney-pots is said to cause smoke; it diminishes the ascensional speed of the air through the pipe.

Jersey, September 2 G. SAVARY

[On referring to Mr. Tomlinson's paper we find that out of four trials, with a number of candles to each, there was a greater consumption of material in the first and fourth trials in the light than in the dark; and in the second and third trials the consumption was greater in the dark than in the light; but in any case the difference was so small, amounting only to from two to seven grains per hour, that it may fairly be referred to accidental circumstances, such as differences in temperature, in currents of air, and in the composition and matter of the candles. Some of the trials were made in the diffused light of day, and in all the trials the differences in temperature between the dark and the light spaces were but small.—ED.]

OUR ASTRONOMICAL COLUMN

THE SATELLITES OF MARS.—The following ephemeris of the outer satellite is deduced from the elements given in this column last week, except that the daily motion is corrected to 285° 5′·147 by observations to Sept. 16:—

	At 8h. om.		At 10h. om. G.M.T.	
	Pos.	Dist.	Pos.	Dist.
	°	″	°	″
Sept. 24	247	75	237	61
,, 25	280	49	276	68
,, 26	52	54	26	34
,, 27	83	70	75	77
,, 28	192	29	129	31
,, 29	252	76	243	68
,, 30	297	35	274	55
Oct. 1	60	63	44	44
,, 2	90	59	80	71
,, 3	217	38	167	25
,, 4	257	72	249	70
,, 5	328	26	286	41

For the inner satellite the following elements may be taken as representing closely the Washington measures, August 17–20:—

Passage of Ascending Node, August 17·89788 G.M.T.

Longitude of node	82° 48′
Inclination of orbit	25 24
Period of revolution	0·31841 days.
Log. radius of orbit at mean distance of Mars from the sun	0·9286

They show the following differences of the calculated angles from those observed:—

Aug. 17	− 0°·1	Aug. 19	+ 1°·9
,, 18	− 0·7	,, 20	+ 0·1

The outer satellite has been observed on several nights by A. A. Common, Esq., of Ealing, with an 18-inch silver-on-glass reflector. On the 15th and 16th inst. excellent measures were made with this instrument, by means of which the period of revolution was corrected before calculating the above ephemeris. Mr. Common has stated to the writer that the satellite is ruddy, even more deeply-coloured than the body of the planet. It has also been observed on several occasions by M. M. Henry at the Observatory of Paris.

Employing Kaiser's value of the mean diameter of Mars at distance unity (9″·472) it results that the inner satellite is distant from the centre of the primary 2·730 and the outer one 6·846 semi-diameters. As seen from the inner satellite the globe of Mars will subtend an angle of 40°, and as seen from the outer satellite, one of about 16°. The orbital motions per minute are respectively seventy-nine miles and fifty miles. Our own moon has a mean orbital velocity of thirty-eight miles per minute.

THE SATELLITE OF NEPTUNE.—The subjoined ephemeris is derived from Prof. Newcomb's tables:—

At 13h. Greenwich M.T.

	Pos.	Dist.		Pos.	Dist.
	°	″		°	″
Sept. 30	30	15·2	Oct. 9	25	13·6
Oct. 1	314	5·7	,, 10	101	6·3
,, 2	231	14·9	,, 11	46	16·2
,, 3	209	14·7	,, 12	23	13·0
,, 4	122	5·7	,, 13	294	7·5
,, 5	49	15·4	,, 14	225	16·5
,, 6	27	14·2	,, 15	201	12·3
,, 7	291	5·9	,, 16	86	7·5
,, 8	228	15·8	,, 17	44	16·7

The motion of this satellite is *retrograde* both with reference to the equator and to the ecliptic, and thus it presents the most decided case of retrograde motion in the planetary system; the motion of the satellites of Uranus, though retrograde upon the ecliptic, is direct upon the equator. For 1877 we have for the satellite of Neptune, from Prof. Newcomb's investigation—

	For Equator.	For Ecliptic.
Node	183° 3′	184° 33′
Inclination	121 42	145 13

Adopting the mean of Mr. Lassell's and Mr. Marth's measures of the diameter of Neptune, taken at Malta in 1864-65, as the most reliable value hitherto published, we find that Prof. Newcomb's mean angular distance of the satellite from Neptune corresponds to a true distance of 14·552 semi-diameters of the primary (or about 219,000 miles), which will therefore present an angular diameter of rather less than 8° as viewed from the satellite. The period of revolution being 5·8769 days, the mean orbital velocity of the satellite is 162 miles per minute.

THE BINARY α CENTAURI.—Mr. Gill has found time to measure this fine star with Lord Lindsay's heliometer at his present station, Mars Bay, Ascension, on four nights between July 22 and August 5. The distance of the components was then little over two seconds, the bright star preceding. The measures are evidently difficult from the magnitude and closeness of the stars, the separate night's results differing by more than 10°; but Mr. Gill will doubtless establish an important epoch, and we may hope at the end of the year to have something like reliable elements of this the most interesting of all the revolving double stars.

METEORIC ASTRONOMY.—The second part of the publications of the Royal Observatory at Münster has appeared, and is entitled, "Resultate der in den 43 Jahren 1833-1875 angestellten Sternschnuppen-Beobachtungen, von Dr. Eduard Heis." It was close upon completion at the time of Dr. Heis's decease on June 30, the revision of the final sheets having been undertaken by one of his pupils. The work contains the times of occurrence and the points of first and last appearance of some 13,000 meteors observed by Heis and the various colleagues who assisted him from time to time, followed by partial discussion of the results and catalogues of radiant points. A fuller account of this valuable publication must be reserved for a future note. The first number of this series contains Heis's long-continued observations on the zodiacal light.

A New Comet.—M. Coggia, of the Observatory at Marseilles, discovered a faint comet on September 14. Its position at 14h. 38m. 8s. mean time at Marseilles, was in R.A. 8h. 32m. 3·1s.; N.P.D., 41° 45′ 59.″ Daily motion in R.A. – 45 seconds, in N.P.D. + 18 minutes. The comet has a central condensation, with trace of a tail.

CHEMICAL NOTES

Action of Organic Substances increasing the Sensitiveness of certain Silver Salts.—Mr. M. C. Lea, of Philadelphia, has criticised, in a short paper, the theory brought forward by Poitevin and Vogel, that increased sensitiveness was imparted to the halogen compounds of silver by certain organic substances in virtue of their affinity for hydrogen. From experiments he has made he is led to the conclusion that these organic substances do not form substitution products as might be expected if they possessed an affinity for hydrogen, but that they all act as reducing agents. The natural view, therefore, of their action which Lea deduces is that the affinity of the organic substance for oxygen assists that of halogen for hydrogen, and, under the influence of light, a molecule of water becomes decomposed. That, in the case of tannin and silver iodide for instance, the tannin is oxidised, the iodine converted into hydriodic acid, and the silver salt more or less reduced. According to this theory traces of free acid would be found instead of the iodine substitution product. His experiments have confirmed this supposition, and he concludes, therefore, that the increase of sensitiveness produced by organic substances takes place in virtue of their affinity for oxygen promoting the decomposition of water by the halogen employed.

Heat of Combustion of Oxygen and Hydrogen in Closed Vessels.—In a recent number of the *Journal* of the German Chemical Society there are some experiments on the above subject communicated by Than. He has modified Bunsen's ice calorimeter, so as to make it available for heat determinations in chemical action, and by this means he has obtained accurate results of the heat of combustion of electrolytic gases in closed vessels. The terms "heat of combustion," or "total difference of energy," are used by Than to express the quantity of actual energy evolved when the combining gas, in the case of oxygen and hydrogen at 0° and 760 mm., is completely converted in a closed vessel into water. Taking the atom of hydrogen as unity he finds that a gramme of hydrogen uniting with the requisite quantity of oxygen in a closed vessel to form water, produces 33·982 units of heat, which number agrees closely with that found by Andrews, viz., 33,970.

On Vapour Volumes in Relation to Avogadro's Law.—In the same journal (*Ber. chem. Ges.*, x.) there is a paper by Troost, detailing experiments made to determine the accuracy of Avogadro's theory that "equal volumes of substances in the state of vapour contained the same number of molecules," *i.e.*, that the volume of the molecule of hydrogen being called 2, the volume of all other molecules must also be 2; instead of as happens in certain cases, apparently 4, 6, or 8. The method of experiment adopted was to introduce into the vapour of chloral hydrate a salt containing water having a dissociation-tension nearly equal to that of chloral hydrate; if the chloral hydrate vapour undergoes dissociation, and consists of equal volumes of chloral and aqueous vapours, then the vapour volume will remain constant; but if chloral hydrate is volatile as such, its vapour will be free from water, and on introducing the salt it will give up water, and the volume of vapour will increase till the dissociation-tension is reached. The salt used was potassium oxalate, containing one molecule of water. Troost has found that the volume increases on the addition of the oxalate, leading him therefore to the conclusion that chloral hydrate undergoes volatilisation without decomposition.

Chemical Constitution of the Minerals Hatchetolite and Samarskite, from North Carolina.—Mr. O. D. Allen has lately had an opportunity of making some further experiments on the above minerals lately described by Mr. J. L. Smith, and of which a short note was given some time ago in Nature. His analytical results confirm those of Mr. Smith, and from these he deduces a ratio among the elements closely corresponding to that of $\overset{II}{R}_2\overset{V}{R}_3 2H_2O$, or

$$\overset{II}{R}_2\overset{y}{R}_2O_7 + 2\overset{II}{R}\,\overset{V}{R}_2O_6 + 4H_2O,$$

when $\overset{II}{R}$ represents one atom of a bivalent radical, or two of sodium, and $\overset{V}{R}$, tantalum or columbium. The investigations of Rammelsberg point to the conclusion that three columbates (having columbium replaced by tantalum) occur in minerals, viz., $\overset{II}{R}Cb_2O_6$, $\overset{II}{R}_2Cb_2O_7$, and $\overset{II}{R}_3Cb_2O_8$, which, singly or combined with each other, constitute mineral species. Mr. Allen regards hatchetolite as composed of the first two, with a small quantity of normal titanate. He also thinks that it may have resulted from the alteration of a mineral possessing essentially the same composition as pyrochlore, by hydration, and removal of alkaline fluorides. From his analysis of samarskite a ratio is obtained closely approximating to that required by the formula $\overset{II}{R}_2\overset{V}{R}_2O_7 + \overset{II}{R}_3\overset{V}{R}_2O_8$. From this it would appear that samarskite closely resembles fergusonite in chemical constitution, the formula of that latter body, deduced by Rammelsberg, being $R_3(Cb, Ta)_2O_8$.

On a New Class of Bodies termed Platoiodnitrites.—Nilson has lately described in the *Ber. Berl. chem. Ges.* x., a series of bodies to which he has assigned the above name, and which he prepares by acting on potassium or barium platinonitrite with an alcoholic solution of iodine. Aldehyde is evolved on heating the mixture, which latter, originally brown, becomes of an amber colour. The platoiodnitrite is deposited in crystals. He has prepared the potassium salt in four-sided amber prisms, and assigns to it the formula $K_2N_2O_4I_2Pt(H_2O)_2$. He represents the reaction by the following equation:—

$$K_2(NO_2)_4Pt + I_2 + (C_2H_6O)_3 = K_2N_2O_4I_2Pt + C_2H_4O + (C_2H_5ONO)_2 + 2(H_2O).$$

He describes also a new acid obtained by decomposing barium platinonitrite with sulphuric acid and evaporating in vacuo. The first product consisted of an acid corresponding to the platotetranitrosylic acid of Lang, but on evaporating the solution to dryness, after removal of the first crystals, a permanent residue of a brownish-green colour remained, which, after drying over sulphuric acid, gave on analysis the following composition: $H_4(NO_2)_8Pt_3O \cdot 2(H_2O)$. The author calls this triplato-octinitrosylic acid.

A New Acid.—At the last meeting of the Niederrheinische Gesellschaft für Natur und Heilkunde of Bonn Prof. Mohr announced the discovery of a new acid of phosphorus and oxygen, by Herr Th. Salzer, of Worms. The new acid stands between phosphorous acid and phosphoric acid, and consists, according to old notation, of one atom of phosphorus and four atoms of oxygen. It has been named hypophosphoric acid. It forms a rather insoluble soda salt. Herr Salzer found that the "acide phosphatique" described by Pelletier consists of a mixture of phosphorous and hypophosphoric acids.

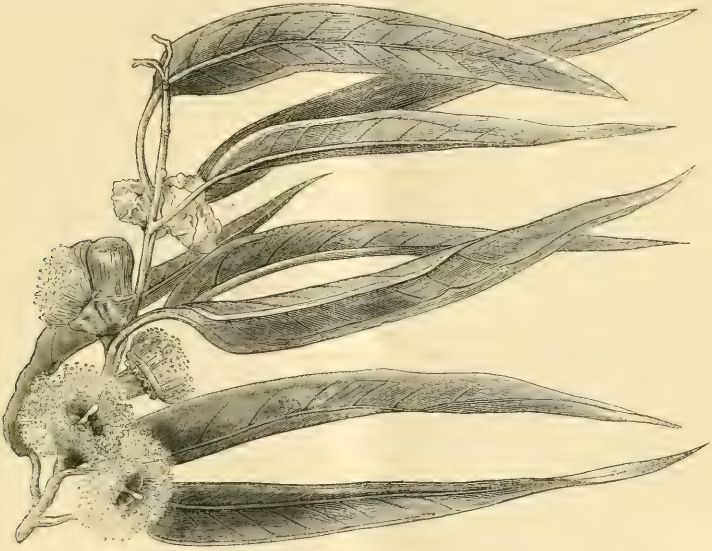
REMARKABLE PLANTS

IV.—THE BLUE GUM TREE (*Eucalyptus globulus*, Labil.).

SO much attention has been directed during the last few years to the various remarkable virtues attributed to this tree, that an exaggerated idea of its value may exist in many minds. Sufficient has, however, been established on irrefragable authority to justify a brief account, in this series of papers, of the known properties and qualities of the *Eucalyptus*. We rely for a considerable proportion of our facts on a lecture delivered before the Royal Botanic Society of London in 1874, by Prof. Bentley, and on the account of the tree in Bentley and Trimen's "Medicinal Plants," part 15, our illustration being also, to a considerable extent, copied from that in the latter work.

The genus *Eucalyptus* is a large one, numbering about 150 species, and belongs to the natural order Myrtaceæ, distinguished by the number of trees and shrubs included in it which yield aromatic properties. The species are all, with a few doubtful exceptions, natives of Australia or Tasmania, and are known in the Colonies as "gum-trees" and "stringy-bark trees." They are all evergreen trees, several of them of enormous height. The one we are describing, a native of Tasmania and temperate Australia, is perhaps the most gigantic of them all, not unfrequently attaining a height of upwards of 300 feet.

The leaves vary remarkably according to the age of the plant; when it is young they are large, sessile, and opposite, of a bluish glaucous-white colour, and placed at right-angles to the branches on which they grow, while on older plants they are much narrower (as shown in the drawing), alternate, bluish green, and, by a twisting of the petiole, appear as if placed obliquely, or in the same plane as the branches, with their flat surfaces lateral. The flowers are large and not very unlike those of the myrtle, with a very large number of stamens, but differing in the absence of a corolla, the limb of the calyx becoming detached when the flower opens in the form of a lid or "operculum."



Eucalyptus globulus (Blue Gum Tree). Branch with flowers and older leaves (reduced)]

The rapidity of the growth of this tree is one of its most remarkable and valuable features. Although not introduced into this country till the year 1856, and not perfectly hardy here, except perhaps in the extreme south-west, trees of a considerable size are not unfrequently seen. A specimen only two years old has flowered this year in the Economic House at the Regent's Park Botanic Gardens. In its native country it is stated that in a grove planted only sixteen years, the average height of the trees is seventy-two feet, and the girth of the stems six feet; while a tree ten years old presents the development of a well-grown oak of a century. In fifty years they are said to attain a height of from 160 to 200 feet, and the trunk a circumference of from 50 to 60 feet at the base. Even where the *Eucalyptus* is not indigenous, well-authenticated instances of a rapidity of growth almost equalling this are on record in favourable climates. Mr. Thomas Hanbury states that near Mentone a seedling planted in March, 1869, was then three feet high; in 1874 it had reached forty-eight feet, and the circumference of the trunk was three feet at three feet above the ground. In Algeria the growth is no less astonishingly rapid. The gigantic size of the trunk is combined with a peculiarity of growth which greatly adds to the value of the timber. It rarely sends out a branch till the stem is 100 feet high, and Prof. Bentley states that planks have frequently been cut 160 feet long, twenty inches broad, and six inches thick. The timber is stated to be at the same time remarkable for its hardness and durability.

This rapid growth renders the *Eucalyptus* an invaluable tree for planting in countries where deforesting has been carried to so great an extent as to prejudicially diminish the rainfall; and it has now been more or less successfully cultivated for this purpose in France, Spain, Portugal, Greece, Italy, Corsica, Algeria, Egypt, St. Helena, Palestine, the uplands of India, Natal, other parts of

South Africa, Cuba, and various parts of North and South America. It has already been stated that it is not hardy in this country, a temperature below the freezing point—or even in some cases a little above it—appearing to kill it. Another useful quality of the tree is that, in consequence of its deciduous bark, it is not attacked by parasites. Baron von Mueller, the director of the Botanic Gardens at Melbourne, states that the ashes of the wood of this and of other species of *Eucalyptus* contain a very large proportion of potash, in some cases as much as twenty-one per cent.

The medicinal properties of the *Eucalyptus globulus* are due to the presence, so common in trees and shrubs belonging to the Myrtaceæ, of a volatile oil, in various parts of the plant, but especially in the mature leaves. This oil may readily be obtained by distillation with water, is of a yellow colour when freshly distilled, and resinifies by exposure to the air. Its principal constituent was found by Cloëz to be a colourless liquid boiling at 347° F., which he regarded as analogous to camphor, and to which he gave the name Eucalyptol; more recent investigations have shown this to be a mixture of two substances, a terpene and a cymol, the essential oil containing other substances in addition to these. Older statements that the leaves of *Eucalyptus* contain, besides this essential oil, quinia or some other of the well-known cinchona alkaloids, have been shown, by the researches of Broughton, to be altogether without foundation.

The value of the leaves as a febrifuge, especially in cases of intermittent fever, has been attested by many medical practitioners, English, Italian, and French; and in Australia the leaves have long had a popular reputation in the treatment of fevers. They are best administered in the form of an alcoholic tincture, which is also useful as a stimulant and antispasmodic. As an external dressing for wounds it is stated by M. Gimbert that the balsamic nature of the leaves not only has a curative effect, but removes all the unpleasant odour. The oil is also used as a disinfectant and antiseptic.

But the point to which the most interest attaches in connection with the *Eucalyptus* is its alleged anti-malarial properties, in consequence of which it has been called the "fever-destroying tree." On this subject Prof. Bentley says that "the evidence that has been adduced from Australia, the native country of the tree, and from all parts of the world where it has been introduced, and which are favourable to its growth, in testimony of its anti-malarial properties, is so strong that, allowing for exaggeration in some cases, it can scarcely be doubted that this tree does produce a most beneficial effect by destroying the fever-producing miasm of marshy districts; and that it should consequently be introduced into all countries and districts where the climatic influences are favourable for its development, and where such miasmatic emanations are to be found." Special interest attaches to the introduction of the blue gum tree into Italy for this purpose, and it is confidently hoped that by its means the problem may at length be solved of destroying the noxious malaria which has in recent times rendered the level country round Rome so unhealthy in the summer season. The chief difficulty is with the occasional frosts to which Northern Italy is subject. Of a large number of trees planted at one time by the Roman Railway Company along the line from Rome to Naples, only those in the neighbourhood of Naples survived the first winter. It is possible, however, that if they became established through a succession of mild winters, and attained a good size, they might then be able to resist slight frosts.

The mode in which the trees thus act in influencing the climate is open to somewhat more controversy. The popular idea is that the efficient cause is the odorous and antiseptic emanations from the leaves. It is quite likely that some influence is exerted in this way, but it seems most probable that the chief effect produced is by the action of the roots on the soil. This function of trees is often greatly overlooked. The effect of the planting of forests in decreasing the rainfall is frequently erroneously stated to be due to the attractive force of the trees on the moisture in the air, similar to that exerted by a range of mountains; but it is difficult to conceive that the small mass of the entire foliage of a forest can exert any appreciable influence in this direction. The mode in which trees mainly act is by their roots arresting the rainfall, which would otherwise escape by the natural drainage of the country; the combined forces of capillarity, osmose, and transpiration then cause the ascent through the tissues of the tree of the water thus arrested, and the larger portion is eventually given off into the air through the stomata of the leaves. In this way a forest tree will in a very short time give off into the air its own weight in water, which must eventually condense, and be again deposited as rain or dew. It is quite possible, however, that the effect of the planting of trees may be apparently the reverse of this in swampy countries where there is no natural drainage. The water then accumulates in the soil; and, if the country is bare of timber-trees and the sun powerful, a rapid decomposition takes place of the herbaceous vegetation, with the consequent emanation of malarial vapours. The effect of the planting of trees under such conditions will be to supply artificial drainage; the accumulation of water in the soil and the consequent noxious effluvia will be diminished and finally prevented, and the atmosphere will be rendered, if not drier, at all events more wholesome. This is the mode in which it is hoped that the malarial fevers of the Campagna may ultimately succumb to the influence of the *Eucalyptus*. In no quarter of the world have the beneficial effects of the planting of this tree been more distinctly seen than in Algeria, where it has been carried on to a considerable extent for some years, mainly through the exertions of private individuals, French and English, aided by the Government. All the good things that have been said about it are there found to have been realised.

A. W. B.

MANTEGAZZA ON THE RELATIVE LENGTHS OF THE INDEX AND "RING" FINGERS

THE curious and suggestive researches made about two years ago by Prof. Ecker, of Freiburg University, in the Breisgau, into the comparative lengths of the index and ring fingers, the results of which were embodied in an article contributed to this journal (vol. xiii. p. 8), entitled, "A new Palmistry," have, in the meantime, been further followed out by Prof. Mantegazza, of Florence.[1]

With the aid of another observer the Florentine professor has made several hundred observations, almost all upon Italians, the subjects being for the most part Romans, Tuscans, and natives of Lombardy. The results are classified in the following table:—

Out of two hands the index longer than the "ring" finger.		Out of two hands the index the shorter.	
Men	27	Men	309
Women	64	Women	194
Total	91	Total	503
Men :: 6·7 : 100		Men :: 76·67 : 100	
Women :: 20·71 : 100		Women :: 62 78 : 100	
Total :: 12·77 : 100		Total :: 70·65 : 100	

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[1] "Della Lunghezza relativa dell' Indice e dell' Anulare della Mano umana. Nota del Professore Paolo Mantegazza." *Archivio per l'Antropologia e la Etnologia*, vol. vii. p. 19. Firenze, 1877.

In one hand the index longer. In the other shorter or = "ring" finger.		Index of same length as "ring" finger in both hands.	
Men	57	Men	10
Women	45	Women	6
Total	102	Total	16
Men	:: 14·14 : 100	Men	:: 2·48 : 100
Women	:: 14·56 : 100	Women	:: 1·94 : 100
Total	:: 14·32 : 100	Total	:: 2·25 : 100

The following instances of this "carattere oscillante" (*schwankender character*, Ecker) of the human hand are taken from what the professor terms "our feminine Olympus":—

1. A pretty Piedmontese girl, with the most lovely hands. In both the index longest.
2. A Jewess of Modena, very lovely, and with beautiful hands. Index much shorter than "ring" finger on both sides.
3. A handsome lady of Imola, with pretty hands. The index a little shorter than the "ring" finger.
4. A Tuscan lady with a most lovely hand. Index the longest of the two digits in question.
5. A lady of Rimini, with a lovely and very small hand. Index longer on both right and left sides.
6. A Neapolitan lady with a wonderful face and figure, and with handsome but large hands. Index shorter than the "ring" finger on both sides.
7. A Ferrarese lady, pretty, and with a hand of rare beauty. Index the shortest in each hand.
8. The prettiest lady in Meldola, with lovely hands. Index the longer on both sides.
9. A lady with the most lovely face and figure and with beautiful hands. Index the shortest in both hands.
10. A Jewess of Livorna, handsome, and with the most lovely hands. The right index the longer, the left the shorter.
11. A lady of Cremona, with a wonderful face and figure, and with large but beautiful hands. Index longest on both sides.
12. A Venetian lady, very beautiful, and with "divine" hands. Index slightly longer on both sides.

Prof. Mantegazza considers that his observations partly confirm and in part check the conclusions of his German colleague. To the examples taken by Prof. Ecker from the domain of art, the former adds the following interesting passage from Prof. Casanova ("Mémoires de Casanova," tome vi., p. 252; Bruxelles, 1871), relative to an argument between this author and the celebrated painter Rafaele Mengs, on the subject of the two digits in question:—

"I remember that one day I took the liberty, in the course of viewing his pictures, of calling his attention to the fact that the hand of a certain figure seemed out of drawing. In fact the fourth finger was shorter than the second.

"'A pretty observation,' he replied; 'look at my hand,' and he held it out.

"'See mine,' I answered, 'I am convinced it does not differ from that of other sons of Adam.

"'From whom, then, would you have me descend?' he replied.

"'Ma foi!' I said, after examining his right hand, 'I do not know to what species to refer you, but you certainly don't belong to mine.'

"'Then your species is not a human one, for the form of the hand of man and woman is just like that.'

"'I bet you a hundred pistoles that you are mistaken,' said I.

"Furious at my contradiction, he throws aside palette and brush, rings for his servants, and makes them all show their hands. His rage was great when he discovered that in all of them the ring finger was longer than the index. Feeling however, the absurdity of his conduct, he ended the scene by the following *mot*:—

"'I am delighted, at any rate to be, to a certain point, unique of my kind.'"

Sig. Paolo Lioy—evidently a trustworthy observer—having been asked by Prof. Mantegazza to direct his attention to the subject in question, returned the following answer:—"I have examined about two hundred individuals but it is remarkable that only in *one man* and in the *left hand* have I seen the index longer than the ring finger. In all the rest, and in both sexes, the ring finger is always the longest, and, with the exception of nine persons, in whom it is but a little longer, it is generally much so; in this, too, in hands fairly beautiful—'in manine assai belle.' It is, therefore, remarkable that, as far as I have been able to see, painters and sculptors give the index the greater length. This I have noticed in all the designs of Canova, the most painstaking and purest idealiser of beauty; as I have been able to verify in certain figures of Titian and Ary Scheffer." Sig. Lioy, thus confirms, as Prof. Mantegazza remarks, the observations of Dr. Ecker.

With regard to the transmission from parent to offspring of the peculiarity of the hand which forms the subject of this article, Prof. Mantegazza states that in many cases he has been enabled to verify the heredity of these characters in certain families in which the father and mother differed as to the relative lengths of the two fingers in question; the children exhibiting the digital proportions of that parent to which they bore the greatest resemblance.

This interesting paper concludes with the following remarks:—"If, however, I have been mistaken in the interpretation of the æsthetic value of the Eckerian character ('del carattere eckeriano'), it would be difficult to find a judge more impartial than myself, in that nature has given me a left hand with an index almost as long as the ring finger, and a right hand with the index shorter than the ring digit. But if artists wish to deduce a practical lesson from this very brief dissertation, I would advise them to give to the more perfect creations of their tool or pencil an index somewhat longer than the ring finger, without, however, wishing to deny to human nature the liberty of making very beautiful hands with a 'ring' finger longer than the index."

J. C. GALTON

NOTES

FILIPPO PARLATORE, Director of the Museo di Fisica è Storia Naturali, at Florence, and of the Botanic Gardens, died suddenly, of a fit, on Sunday, September 9. He elaborated the Gnetaceæ and Coniferæ, for the sixteenth volume of De Candolle's "Prodromus," and was author of a partly completed work on the Italian flora.

WE regret to announce the death of Prof. Jacob Nöggerath, lecturer on Mineralogy at Bonn University, who died at the advanced age of 90 years, on Thursday last the 13th instant.

SIR JOSEPH HOOKER and Prof. Asa Gray, who, as our readers know, are accompanying Dr. Hayden on a scientific tour in Western America, had, the *American Naturalist* states, collected, previous to August 1, nearly 400 species of rare plants, being thus enabled to study critically in their native habitats the species they had during past years described from dried specimens brought in by expeditions. Both Sir J. Hooker and Prof. Gray will prepare reports on the botany of the West for the Eleventh Report of Hayden's Survey.

Mr. R. S. Newall, F.R.S., telegraphing to the *Times*, from the Observatory, Gateshead, last Thursday night, states that on August 23, during the total eclipse of the moon, he observed that Mars is surrounded by a whitish envelope, the diameter being about twenty times that of the planet. He saw it again on September 7, and again last night distinctly. It has a well-defined edge, and is densest nearest to Mars. Small stars were seen though it. It is easily visible, Mr. Newall states, in the 61½-inch finder.

The regular proceedings of the Iron and Steel Institute commenced at Newcastle on Tuesday. According to the report of the secretary the effective strength of the Institute is now close on 1,000 members. After a discussion on two papers read at the last London meeting Mr. Lowthian Bell, M.P., read a paper on the separation of carbon, silicon, sulphur, or phosphorus in the refining and puddling furnace and in the Bessemer conversion. The afternoon was devoted to visits and excursions, as we announced last week.

Herr Bruijn's last expedition to New Guinea, which started from Ternate in January last, returned to that island on June 15, having accomplished good results. Examples of both sexes of the wonderful new monotreme, *Tachyglossus bruijnii*, were obtained in the mountains on the north coast of New Guinea at an elevation of about 3,500 feet. The expedition was commanded by M. Leon Laglaize, a young French naturalist, who, with the rich collections he has made, is expected to return to Paris by the next French mail.

Mr. A. Boucard, the well-known naturalist and collector, has just returned from a successful expedition to Costa Rica, where he passed some four months at the commencement of the year. Mr. Boucard has formed a good series of Costa Rican birds, comprising examples of about 200 species. Amongst these are several new to science. Mr. Boucard has also obtained the female of the rare and little known Cotingine bird described a short time ago by Mr. Salvin as *Carpodectes nitidus*.

Letters have been received from Mr. Everard F. im Thurm, announcing his safe arrival at Georgetown, British Guiana, where he has accepted an appointment as Curator of the British Guiana Museum. Mr. im Thurm will shortly proceed upon an expedition into the interior of the Colony to obtain specimens for the collection under his charge.

We have received the Daily Programme of the meeting of the American Association at Nashville. Judging from the number of members registered and elected the attendance must have been large, considering the almost tropical heat that prevailed. Eighty-seven papers were entered for reading, all of them on points of scientific importance. Prof. Newcomb gave an evening lecture on the two important astronomical discoveries recently made in America, viz., that of oxygen in the sun by Prof. Draper, and that of the satellites of Mars by Prof. Hall. Prof. Pickering, vice-president and president of Section A, was unable to be present, but an address by him was read, in favour of the endowment of research. He described what he thought would be a suitable building and arrangements for a physical laboratory. Prof. Pickering gave elaborate details of his project, and pointed out the numerous advantages that might be expected to result when those facilities were afforded to investigators. The plan includes the appointment of a presiding officer and a staff of assistants. From the reports in the local paper, the *Daily American*, the meeting as a whole seems to have been well managed and successful. Without doubt the prominent feature of the meeting was Prof. Marsh's address on the Succession of Vertebrate Life in America, which we are happy to be able to print elsewhere,

The days of meeting of the third annual conference of the Cryptogamic Society of Scotland, at Dunkeld, have been changed to October 17, 18, and 19.

In the *Scientific and Literary Review* for September, 1877, there is a notice of the Spined Soldier-bug as a *newly-discovered* enemy to the Colorado beetle. In the "Fourth Annual Report of the Noxious, Beneficial, and other Insects of the State of Missouri (1872)," Mr. Riley speaks of this insect as "now so well known for its efficiency in thinning out the ranks of our potato pest." Mr. Riley also figures two other Hemiptera of the family *Scutelleridæ* as enemies to this beetle—the Ring-banded Soldier-bug and the Dotted-legged Plant-bug (*Euschistus punctipes*).

News from Naples has been received announcing an increased activity of Mount Vesuvius. The glow of fire in the crater is so intense that it can be distinctly seen from Naples at night.

In our number for August 9 we briefly noticed the ascent made by Mons. Wiener of the mountain Illimani, one of the highest—if not the highest—of the Bolivian Andes, which forms a noble object from the city of La Paz, and was formerly reputed (on the authority of Mr. Pentland) to have an altitude of no lesss than 24,200 feet. M. Wiener, however, makes its height only 20,112 feet, while Mr. Minchin, as we have already observed, places its altitude at 21,224 feet. If the latter estimate be correct, Mons. Wiener has, we believe, not only made the highest ascent which has been made in the Andes, but has attained a greater altitude than has hitherto been reached *on* the earth out of Asia, and in Asia has only been beaten by Mr. Johnson, who some years ago got to a height of 22,300 feet in Cashmere. As the recorded ascents to the height of 21,000 feet are extremely few, we shall be glad to hear further particulars respecting Mons. Wiener's exploit, and more especially whether he experienced much exhaustion through the rarefaction of the air. Practised mountaineers who have climbed to a height of 17,000 to 18,000 feet have been of opinion that even at such altitudes there is a very important and perceptible diminution of the bodily powers, and think it probable that the height of 25,000 or 26,000 feet will be found to be about the limit which will ever be reached on foot. As a set-off to this opinion we may mention the facts that hunters in the Himalayas frequently pursue their game at heights exceeding 20,000 feet without experiencing any notable inconvenience from the low barometric pressure, and that natives living on the base of Demavend, near Teheran, often ascend to its summit to gather sulphur from its crater without any great difficulty. The height of this mountain, there is reason to believe, also exceeds 20,000 feet, although it has never been accurately determined. If, therefore, severe work can be done with impunity at such elevations, it seems not unreasonable to suppose that much greater heights might be attained by men who had previously accustomed themselves to life at high altitudes. Aëronauts, anyhow, have proved that life can exist at 30,000 feet above the level of the sea, and that at 25,000 feet, and upwards, one may positively be comfortable if sufficiently warmly clad. That such is the case is sufficiently remarkable, for "travellers in the air" have to sustain incomparably more rapid variations of pressure and temperature than mountain-climbers. Mr. Glaisher, on his memorable ascent on September 5, 1862, left the earth at 1 P.M., and in *less than an hour* shot up to a height of 30,000 feet. At starting the temperature of the air was 59°, and at its greatest altitude it was sixty-one degrees lower! Mountaineers experience no such extreme variations as these. They rarely ascend more rapidly than 1,000 feet per hour, never so much as 15,000 feet in a *day*, and become to some extent acclimatised as they progress upwards. On the whole we are inclined to think that man will not rest until he has at least attempted to reach the loftiest summits on the earth, though we will venture to assert that it will be long before anyone crushes down the snow on the summit of Mount Everest.

SINCE we last noticed the progress of the great Government Map of Switzerland several further instalments of it have been issued. In all 108 sheets have been published out of the 540 which will compose the map. Amongst the more recently-published sheets the following will especially be found useful by English travellers in Switzerland:—La Chaux de Fonds, Thun, Engelberg, Wassen (embracing the Titlis district), Guttanen (with the basin of the Gauli glacier), and St. Mauritz (giving the country round Pontresina). The whole of the sheets as yet published are most admirably drawn, and reflect the highest credit on those who have been concerned in their production; and the celerity with which they are issued makes us desire that a little more life could be infused into our own topographical departments.

PETERMANN'S *Mittheilungen* for October will contain a long paper, embodying the results of considerable research, on the German and Latin Elements among the Population of the South Tyrol and Venetia. A new map of a large portion of Costa Rica will show the results of the surveys of Gabb, Collins, and Martinez. A letter from Dr. Schweinfurth describes his journey through the Arabian Desert of Egypt, from Heluan to Keneh, between March 28 and May 18 of this year. He has obtained, besides important topographical data, much valuable information as to the geological and botanical conditions of the region.

THERE is on view at present at the Alexandra Palace an interesting collection of fourteen Nubians with a number of animals, comprising six ostriches, six giraffes, five elephants, twenty-one racing dromedaries, three rhinoceroses, two hunting dogs, two Abyssinian spotted donkeys, four buffaloes, two zebus, monkeys, &c. Some specimens of *Cynocephalus gelada*, which are said to live from 7,000 feet to 11,000 feet high in the Abyssinian hills are expected to follow. The European who organised and accompanied the caravan for Messrs. Rice and Hagenbeck, says that the men from the different tribes speak different patois, so that he very often cannot understand them, and they cannot understand one another. The different districts in which the various animals were captured does not, therefore, seem to be known to the present owners. As to the men, they have been interrogated as to their ages and the tribes to which they belong. There are four Hadendoes aged fifteen, twenty-three, twenty-three, and twenty-five. The characteristic manner of dressing the hair is well seen in the three men, but the lad does not seem to have adopted yet the artificial arrangement. They are all tall, fine men. There are two Hallengas from Cassala, aged twenty-two and twenty-four. The general style of trimming the hair is much the same as of the Hadendoes, but the "fringe" is much longer and stands away from the head more. There are three of the Beni-Amer tribe, one of whom, aged twenty-four, having fallen ill in Paris, had his hair cut off; the "old man," aged thirty-two, wears a close white cap, and he alone of the party can read and write; while the third retains his hair in its original state. Of the remaining six men of the fourteen each represents a different tribe, and they all differ in appearance and style of hair arrangement from those tribes already mentioned. There is a Djaalein, aged twenty-six; a Homran, aged nineteen, who has the three rhinoceroses under his special care, and which follow him and lick his hand like pet lambs; a man, aged twenty-seven, from Amara, near Suakim; and a Takrouri, twenty-five, who has twice been to Mecca, the only one of the party who has; and the Bara "boy," who claims to be twenty-one. He has the negro hair and lips, and a very contented look. All the men seem happy, and sing, laugh, smoke, and go through the mimic war, dromedary racing, and their representation of crossing the desert with great delight.

IN a letter to yesterday's *Times* Mr. Henry Jeula, of Lloyd's, gives some interesting data to show that there is probably some connection between sunspots and the number of wrecks posted each year on Lloyd's Loss Book. His data are for two complete cycles of eleven years, 1855–1876, and the results Mr. Jeula has worked out along with Dr. Hunter. Dividing the eleven years as nearly as the number will allow, into three parts, and taking the percentages of losses posted, Mr. Jeula finds a coincident minimum period of four years at the extremities of the cycle, a maximum period of three years in the centre of the cycle, and an intermediate period of the four years lying between the maximum and minimum periods. Mr. Jeula expresses the hope that the great practical importance of the theory of the connection between sunspots and weather will lead to a full and exhaustive examination of all the evidence bearing upon it.

THE exhibition of the Photographic Society of Great Britain will be opened by a *conversazione* on Tuesday evening, October 9, at 5, Pall Mall, East

IN a paper presented recently to the philosophical faculty at Heidelberg University, Herr Richard Boernstein has published his investigations on the influence of light upon the electric resistance of metals. Mr. Willoughby Smith had found that the electric resistance of selenium, and in a much smaller degree that of tellurium also, decreases under the influence of light. Herr Boernstein has now made the interesting discovery that this property also belongs to platinum, gold, and silver, most probably to all metals, in fact. The electric current, according to Herr Boernstein, diminishes the electric conducting power, as well as the sensitiveness towards light, of its conductor, but after cessation of the current, both gradually return to their former values.

ROCK crystal seems to be growing more and more in favour amongst technical men on account of the stability of its physical properties. At the August meeting of the Bonn Society of Naturalists it was reported that the directors of the Imperial Mint of Germany have recently ordered of Herr Stern, at Oberstein, several absolutely correct normal weights made of rock crystal, which are to be used for the control of gold coins. These weights have the great advantage that it is unnecessary to determine the specific gravity of every weight, and in the case of measures to find the thermal co-efficient of expansion of every measure, as both are as near constant as possible. They have been found the same in all the specimens of rock crystal yet examined, viz., specific gravity at 0° C. = 2,6506 (reduced to water at 4° C.); coefficient of expansion for 1° C., parallel to the axis, 0·00000750 inch, *i.e.*, seventy-five ten-millionths of an inch.

AT the same meeting Prof. vom Rath read a report from Dr. Th. Wolf, the state geologist to the South American republic of Ecuador, on the province of Esmeraldas (the northernmost province of the republic), and on the rain of ashes which, coming from the north-east, *i.e.*, from the volcanic interior, fell along the whole *littorale* of Guayaquil between June 26 and June 30. Dr. Wolf, after giving a general geological description of Esmeraldas (in the auriferous sands of which he discovered platinum), adds the following general remarks:—Of all provinces of Ecuador Esmeraldas is the most uniform in its relief and geological structure. It reaches from the coast of the Pacific to the foot of the Andes. A great part of the province is quite flat, particularly in the north; another part is traversed by low mountains, the highest points of which scarcely reach 500 or 600 meters; the average height of the hills, however, is only 50–70 meters. The province is a magnificent country, and especially rich in the finest timber and many other vegetable products — none utilised. The only article which finds its way abroad is caoutchouc, and enormous quantities of this have been exported during the last ten years, but the export begins gradually to decrease, since the workmen, instead of only tapping the trees, destroy them completely. The province is inhabited by a population of only 10,000 natives, who live along the coast. The whole of the interior is covered by one gigantic virgin forest, and accessible only in canoes upon the rivers. Of the three months of my journey I spent more than two in canoes, which are rather small and hardly comfortable, or adapted for a travelling naturalist; the last twenty-three days I spent uninterruptedly in a canoe on the Esmeraldas River and its tributaries. The rivers are very rapid and not without dangers; but then my journey was made during the middle of the rainy season, when the rivers are very much swollen. On the Cayapas River I made the acquaintance of the wild Cayapas Indians, a very interesting tribe with a language and customs of their own. They keep in perfect isolation from other tribes, living in forests, hunting and fishing, going almost naked, and painting their bodies; on the whole they are very harmless, and may be some 2,000 in number."

IN the *Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen*, Herr Edmund Hoppe gives an account of some experiments he made with a view to determine the resistance offered by flames to the galvanic current. He arrived at the following results: (1) In each flame the greater galvanic conducting power depends on the greater heat and the greater quantity of burning gas. (2) With different flames the conducting power depends on the burning substances; the salts of potassium, sodium, barium, strontium, lithium, thallium, and copper in particular increase greatly the conducting power of the hydrogen flame. (3) Ohm's law applies perfectly to flames.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. A. S. Percival; an Osprey (*Pandion haliaëtus*) from Yorkshire, presented by Mr. W. H. S. Quintin; a Common Hangnest (*Icterus vulgaris*) from South America, presented by Mr. Hamilton Dunlop; a West African Python (*Python sebæ*) from West Africa, presented by Mr. Francis Lovell; two Guilding's Amazons (*Chrysotis guildingi*) from St. Vincent, a Violet Tanager (*Euphonia violacea*), a Yellow-winged Blue Creeper (*Cœreba cyanea*), a Common Boa (*Boa constrictor*) from South America, deposited; three Capybaras (*Hydrochœrus capybara*) from South America, purchased.

INTRODUCTION AND SUCCESSION OF VERTEBRATE LIFE IN AMERICA[1]

THE origin of life, and the order of succession in which its various forms have appeared upon the earth, offer to science its most inviting and most difficult field of research. Although the primal origin of life is unknown, and may perhaps never be known, yet no one has a right to say how much of the mystery now surrounding it science cannot remove. It is certainly within the domain of science to determine when the earth was first fitted to receive life, and in what form the earliest life began. To trace that life in its manifold changes through past ages to the present is a more difficult task, but one from which modern science does not shrink. In this wide field every earnest effort will meet some degree of success; every year will add new and important facts; and every generation will bring to light some law, in accordance with which ancient life has been changed into life as we see it around us to-day. That such a development has taken place no one will doubt who has carefully traced any single group of animals through its past history, as recorded in the crust of the earth. The evidence will be especially conclusive if the group selected belongs to the higher forms of life, which are sensitive to every change in their surroundings. But I am sure I need offer here no argument for evolution; since to doubt evolution to-day is to doubt science, and science is only another name for truth.

Taking, then, evolution as a key to the mysteries of past life on the earth, I invite your attention to the subject I have chosen: The Introduction and Succession of Vertebrate Life in America.

In the brief hour allotted to me I could hardly hope to give more than a very incomplete sketch of what is now known on this subject. I shall therefore pass rapidly over the lower groups, and speak more particularly of the higher vertebrates, which have an especial interest to us all, in so far as they approach man in structure, and thus indicate his probable origin. These higher vertebrates, moreover, are most important witnesses of the past, since their superior organisation made them ready victims to slight climatic changes, which would otherwise have remained unrecorded.

In considering the ancient life of America it is important to bear in mind that I can only offer you a brief record of a few of the countless forms that once occupied this continent. The review I can bring before you will not be like that of a great army, when regiment after regiment with full ranks moves by in orderly succession, until the entire host has passed. My review must be more like the roll-call after a battle, when only a few scarred and crippled veterans remain to answer to their names. Or rather, it must resemble an array of relics, dug from the field of some old Trojan combat, long after the contest, when no survivor remains to tell the tale of the strife. From such an ancient battle-field a Schliemann might unearth together the bronze shield, lance-head, and gilded helmet of a prehistoric leader, and learn from them with certainty his race and rank. Perhaps the skull might still retain the barbaric stone weapon by which his northern foe had slain him. Near by the explorer might bring to light the commingled coat of mail and trappings of a horse and rider, so strangely different from the equipment of the chief, as to suggest a foreign ally. From these, and from the more common implements of war that fill the soil, the antiquary could determine, by patient study, what nations fought, and perhaps when and why.

By this same method of research the more ancient strata of the earth have been explored, and in our western wilds, veritable battle-fields, strewn with the fossil skeletons of the slain, and guarded faithfully by savage superstition, have been despoiled, yielding to science treasures more rare than bronze or gold. Without such spoils, from many fields, I could not have chosen the present theme for my address to-night.

According to present knowledge, no vertebrate life is known to have existed on this continent in the Archæan, Cambrian, or Silurian periods; yet during this time more than half of the thickness of American stratified rocks was deposited. It by no means follows that vertebrate animals of some kind did not exist here in those remote ages. Fishes are known from the upper Silurian of Europe, and there is every probability that they will yet be discovered in our strata of the same age, if not at a still lower horizon.

In the shore deposits of the early Devonian sea, known as the Schoharie grit, characteristic remains of fishes were preserved, and in the deeper sea that followed, in which the corniferous limestone was laid down, this class was well represented. During the remainder of the Devonian fishes continue abundant in the shallower seas, and, so far as now known, were the only type of vertebrate life. These fishes were mainly ganoids, a group represented in our present waters by the gar-pike (*Lepidosteus*) and sturgeon (*Acipenser*), but, in the Devonian sea, chiefly by the placoderms, the exact affinities of which are somewhat in doubt. With these were elasmobranchs, or the shark tribe, and among them a few chimæroids, a peculiar type, of which one or two members still survive. The placoderms were the monarchs of the ocean. All were well protected by a massive coat of armour, and some of them attained huge dimensions. The American Devonian fishes now known are not as numerous as those of Europe, but they were larger in size, and mostly inhabitants of the open sea. Some twenty genera and forty species have been described.

The more important genera of placoderms are *Dinichthys*, *Aspidichthys*, and *Diplognathus*, our largest palæozoic fishes. Others are, *Acanthaspis*, *Acantholepis*, *Coccosteus*, *Macropetalichthys*, and *Onychodus*. Among the elasmobranchs were, *Cladodus*, *Ctenacanthus*, *Machæracanthus*, *Rhynchodus*, and *Ptyctodus*, the last two being regarded as chimeroids. In the

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[1] Lecture delivered at the Nashville meeting of the American Association, August 30.

Chemung epoch the great dipterian family was introduced with *Dipterus*, *Heliodus*, and possibly *Ceratodus*. Species of the European genera, *Bothriolepis* and *Holoptychius*, have likewise been found in our Devonian deposits.

With the close of the Devonian came the almost total extinction of the great group of placoderms, while the elasmobranchs, which had hitherto occupied a subordinate position, increase in numbers and size, and appear to be represented by sharks, rays, and chimeras. Among the members of this group from the carboniferous were numerous cestracionts, species of *Cochliodus* of large size, with others of the genera *Deltodus*, *Helodus*, *Psammodus*, and *Sandalodus*. Of the Petalodonts there were *Antliodus*, *Chomatodus*, *Ctenoptychius*, *Petalodus*, and *Petalorhynchus*; and of the hybodonts, the genera *Cladodus*, *Carcharopsis*, and *Diplodus*. These elasmobranchs were the rulers of the carboniferous open sea, and more than one hundred species have been found in the lower part of this formation alone. The ganoids, although still abundant, were of smaller size, and denizens of the more shallow and confined waters. The latter group of fishes was represented by true lepidostidæ, of the genera *Palæoniscus*, *Amblypterus*, *Platysomus*, and *Eurylepis*. Other genera are, *Rhizodus*, *Megalichthys*, *Ctenodus*, *Edestus*, *Orodus*, *Ctenacanthus*, *Gyracanthus*, and *Cœlacanthus*. Most of these genera occur also in Europe.

From the permian rocks of America no vertebrate remains are known, although in the same formation of Europe ganoids are abundant, and with them are remains of sharks, and some other fishes, the affinities of which are doubtful. The palæozoic fishes at present known from this country are quite as numerous as those found in Europe.

In the mesozoic age the fishes of America begin to show a decided approach to those of our present waters. From the triassic rocks ganoids only are known, and they are all more or less closely related to the modern gar-pike, or *Lepidosteus*. They are of small size, and the number of individuals preserved is very large. The characteristic genera are *Catopterus*, *Ischypterus*, *Ptycholepis*, *Rhabdolepis*, and *Turseodus*. From the Jurassic deposits no remains of fishes are known, but in the cretaceous ichthyic life assumed many and various forms; and the first representatives of the teleosts, or bony fishes, the characteristic fishes of to-day, make their appearance. In the deep open sea of this age elasmobranchs were the prevailing forms, sharks, and chimæroids being most numerous. In the great inland cretaceous sea of North America true osseous fishes were most abundant, and among them were some of carnivorous habits and immense size. The more sheltered bays and rivers were shared by the ganoids and teleosts, as their remains testify. The more common genera of cretaceous elasmobranchs were *Otodus*, *Oxyrhina*, *Galeocerdo*, *Lamna*, and *Ptychodus*. Among the osseous fishes, *Beryx*, *Enchodus*, *Portheus* and *Saurocephalus* were especially common, while the most important genus of ganoids was *Lepidotus*.

The tertiary fishes are nearly all of modern types, and from the beginning of this period there was comparatively little change. In the marine beds sharks, rays, and chimæroids maintained their supremacy, although teleosts were abundant, and many of them of large size. The ganoids were comparatively few in number. In the earliest eocene fresh-water deposits it is interesting to find that the modern gar-pike, and *Amia*, the dog-fish of our western lakes, which by their structure are seen to be remnants of a very early type, are well represented by species so closely allied to them that only an anatomist could separate the ancient from the modern. In the succeeding beds these fishes are still abundant, and with them are siluroids nearly related to the modern cat-fish (*Pimelodus*). Many small fishes allied apparently to the modern herring (*Clupea*), left their remains in great numbers in the same deposits, and with them has been recently found a land-locked ray (*Heliobatis*).

The almost total absence of remains of fishes from the miocene lake-basins of the west is a remarkable fact, and perhaps may best be explained by the theory that these inland waters, like many of the smaller lakes in the same region to-day, were so impregnated with mineral matters as to render the existence of vertebrate life in them impossible. No one who has tasted such waters or has attempted to ford one of the modern alkaline lakes which are often met with on the present surface of the same deposits, will doubt the efficiency of this cause, or the easy entombment of the higher vertebrates that ventured within their borders. In the pliocene lake-basins of the same region remains of fishes were not uncommon, and in some of them are very numerous. These are all of modern types and most of them are cyprinoids related to the modern carp. The post-pliocene fishes are essentially those of to-day.

In this brief synopsis of the past ichthyic life of this continent I have mentioned only a few of the more important facts, but sufficient, I trust, to give an outline of its history. Of this history it is evident that we have as yet only a very imperfect record. We have seen that the earliest remains of fishes known in this country are from the lower Devonian; but these old fishes show so great a diversity of form and structure as to clearly indicate for the class a much earlier origin. In this connection we must bear in mind that the two lowest groups of existing fishes are entirely without osseous skeletons, and hence, however abundant, would leave no permanent record in the deposits in which remains of fishes are usually preserved. It is safe to infer from the knowledge which we now possess of the simpler forms of life, that even more of the early fishes were cartilaginous, or so destitute of hard parts as to leave no enduring traces of their existence. Without positive knowledge of such forms, and considering the great diversity of those we have, it would seem a hopeless task at present to attempt to trace successfully the genealogy of this class. One line, however, appears to be direct, from our modern gar-pike, through the lower eocene *Lepidosteus* to the *Lepidotus* of the cretaceous, and perhaps on through the triassic *Ischypterus* and carboniferous *Palæoniscus*; but beyond this, in our rocks, it is lost. The living chimæra of our Pacific coast has nearly allied forms in the tertiary and cretaceous, more distant relatives in the carboniferous, and a possible ancestor in the Devonian *Rhynchodus*. Our sharks likewise can be traced with some certainty back to the palæozoic; and even the *Lepidosiren*, of South America, although its immediate predecessors are unknown, has some peculiar characters which strongly point to a Devonian ancestry. These suggestive lines indicate a rich field for investigation in the ancient life-history of American fishes.

The amphibians, the next higher class of vertebrates, are so closely related to the fishes in structure, that some peculiar forms of the latter have been considered by anatomists as belonging to this group. The earliest evidence of amphibian existence, on this continent, is in the sub-carboniferous, where foot-prints have been found which were probably made by labyrinthodonts, the most ancient representatives of the class. Well preserved remains are abundant in the coal-measures, and show that the labyrinthodonts differed in important particulars from all modern amphibians, the group which includes our frogs and salamanders. Some of these ancient animals resembled a salamander in shape, while others were serpent-like in form. None of those yet discovered were frog-like, or without a tail, although the restored labyrinthodont of the text-books is thus represented. All were protected by large pectoral bony plates, and an armour of small scutes on the ventral surface of the body. The walls of their teeth were more or less folded, whence the name labyrinthodont. The American amphibians known from osseous remains are all of moderate size, but the foot-prints attributed to this group indicate animals larger than any of the class yet found in the Old World. The carboniferous amphibians were abundant in the swampy tropical forests of that period, and their remains have been found imbedded in the coal then deposited, as well as in hollow stumps of the trees left standing.

The principal genera of this group from American carboniferous rocks, are, *Sauropus*, known only from foot-prints, *Baphetes*, *Dendrerpeton*, *Hylonomus*, *Hylerpeton*, *Raniceps*, *Pelion*, *Leptophractus*, *Molgophis*, *Ptyonius*, *Amphibamus*, *Cocytinus*, and *Ceraterpeton*. The last genus occurs also in Europe. Certain of these genera have been considered by some writers to be more nearly related to the lizards (*Lacertilia*) among true reptiles. Some other genera known from fragmentary remains or foot-prints in this formation have likewise been referred to the true reptiles, but this question can perhaps be settled only by future discoveries.

No amphibia are known from American permian strata, but in the triassic, a few characteristic remains have been found. The three genera, *Dictyocephalus*, *Dispelor*, and *Pariostegus*, have been described, but, although apparently all labyrinthodonts, the remains preserved are not sufficient to add much to our knowledge of the group. The triassic foot-prints which have been attributed to amphibians are still more unsatisfactory, and at present no important conclusions in regard to this class can be based upon them. From the Jurassic and cretaceous beds of this continent, no remains of amphibians are known. A few only have been found in the tertiary, and these are all of modern types.

The amphibia are so nearly allied to the ganoid fishes, that we can hardly doubt their descent from some member of that group. With our present limited knowledge of the extinct forms, however, it would be unprofitable to attempt to trace in detail their probable genealogy.

The authors to whom especial credit is due for our knowledge of American fossil fishes and amphibians, are Newberry, Leidy, Cope, Dawson, Agassiz, St. John, Gibbes, Wyman, Redfield, and Emmons, and the principal literature of the subject will be found in their publications.

Reptiles and birds form the next great division of vertebrates, the sauropsida, and of these the reptiles are the older type, and may be first considered. While it may be stated with certainty that there is at present no evidence of the existence of this group in American rocks older than the carboniferous, there is some doubt in regard to their appearance even in this period. Various foot-prints which strongly resemble those made by lizards, a few well preserved remains similar to the corresponding bones in that group, and a few characteristic specimens, nearly identical with those from another order of this class, are known from American coal measures. These facts, and some others which point in the same direction, render it probable that we may soon have conclusive evidence of the presence of true reptiles in this formation, and in our overlying permian, which is essentially a part of the same series. In the permian rocks of Europe, true reptiles have been found.

The mesozoic period has been called the age of reptiles, and during its continuance some of the strangest forms of reptilian life made their appearance, and became extinct. Near its commencement, while the triassic shales and sandstones were being deposited, true reptiles were abundant. Among the most characteristic remains discovered are those of the genus *Belodon*, which is well known also in the trias of Europe. It belongs to the thecodont division of reptiles, which have teeth in distinct sockets, and its nearest affinities are with the crocodilia, of which order it may be considered the oldest known representative. In the same strata in which the belodonts occur, remains of dinosaurs are found, and it is a most interesting fact that these highest of reptiles should make their appearance, even in a generalised form, at this stage of the earth's history. The dinosaurs, although true reptiles in all their more important characters, show certain well marked points of resemblance to existing birds of the order *Ratitæ*, a group which includes the ostriches; and it is not improbable that they were the parent stock from which birds originated.

During triassic time, the dinosaurs attained in America an enormous development both in variety of forms and in size. Although comparatively few of their bones have as yet been discovered in the rocks of this country, they have left unmistakable evidence of their presence in the foot-prints and other impressions upon the shores of the waters which they frequented. The triassic sandstone of the Connecticut Valley has long been famous for its fossil foot-prints, especially the so-called "bird-tracks," which are generally supposed to have been made by birds, the tracks of which many of them closely resemble. A careful investigation, however, of nearly all the specimens yet discovered, has convinced me that there is not a particle of evidence that any of these fossil impressions were made by birds. Most of these three-toed tracks were certainly not made by birds: but by quadrupeds, which usually walked upon their hind feet alone, and only occasionally put to the ground their smaller anterior extremities. I have myself detected the impressions of these anterior limbs in connection with the posterior foot-prints of nearly all the supposed "bird-tracks" described, and have little doubt that they will eventually be found with all. These double impressions are precisely the kind which dinosaurian reptiles would make, and as the only characteristic bones yet found in the same rocks belong to animals of this group, it is but fair to attribute all these foot-prints to dinosaurs, even where no impressions of fore-feet have been detected, until some evidence appears that they were made by birds. I have no doubt that birds existed at this time, although at present the proof is wanting.

The principal genera of triassic reptiles known from osseous remains in this country are, *Amphisaurus* (*Megadactylus*), from the Connecticut Valley, *Bathygnathus*, from Prince Edward's Island, *Belodon* and *Clepsysaurus*. Other generic names which have been applied to foot-prints and to fragmentary remains, need not be here enumerated. A few remains of reptiles have been found in undoubted Jurassic rocks of America, but they are not sufficiently well determined to be of service in this connection. Others have been reported from supposed Jurassic strata, which are now known to be cretaceous. It will thus be seen that, although reptilian life was especially abundant during the triassic and Jurassic periods, but few bones have been found. This is owing in part to the character of most of the rocks then formed, which were not well fitted for preserving such remains, although admirably adapted to retain foot-prints.

(*To be continued.*)

ON NOCTURNAL INCREASE OF TEMPERATURE WITH ELEVATION[1]

TILL the year 1862, when my first experiments were made by the use of the balloon, our knowledge of the temperature of the air was almost entirely confined to within four or five feet of the earth's surface, and the theory that the temperature was always lower at high elevations, and that the decrease of temperature with increase of elevation was at the rate of 1° Fahrenheit for every 300 feet of elevation, was generally received and acted upon. These theories were found not to be at all times true, and the assumption of the decrease of 1° of temperature in every increase of 300 feet of elevation was proved to be erroneous in every balloon ascent I have made; in some a decrease of 1° and more than 1° was experienced within 100 feet, and there is no doubt that, considering the quickness of motion on leaving the earth, the decrease at such times was really 2° or 3°, or more, within the space of 100 feet.

In some of the ascents a difference of 10° was met with within 1,000 feet of the earth, whilst in others but little or no difference was experienced even to heights exceeding 1,000 feet.

Towards the end of my balloon experiments it was evident that a very large number more were necessary, and in my last report I said:—

From all the experiments made it would seem that the decrease of temperature with increase of elevation is variable throughout the day, and variable in different seasons of the year; that at about sunset the temperature varies but very little for a height of 2,000 feet; that at night with a clear sky the temperature increases with elevation; that at night with a cloudy sky there was a small increase of temperature as the height increased; that in the double ascent of May 29, 1866, the one just before and the other after sunset, it would seem that after radiation from the earth began, the heat passes upwards till arrested where the air is saturated with vapour, when a heat greater by 5° was experienced after sunset than at the same elevation before sunset.

This was the state of our knowledge when M. Giffard most kindly placed the great "Captive" balloon, located at Ashburnham Park, Chelsea, near London, at my disposal for a series of experiments.

This balloon could ascend to the height of 2,000 feet on a calm day; its rate of ascension could be regulated at will; it could be kept stationary at any elevation, and experiments could be repeated several times in the day.

On two different days I ascended nine times on each day; there was a decrease of temperature with increase of elevation at every ascent, but, different in amount at every hour, being less and less as the day advanced towards sunset. The results of the experiments are shown in the following table, showing the amount of decrease of temperature per 100 feet of elevation, at different hours of the day with a clear sky, and a cloudy sky, as found by experiments with M. Giffard's captive balloon.

Height above the ground.		Clear Sky.						Cloudy Sky.				
From	To	10 A.M. to 11 A.M.	3 P.M. to 4 P.M.	4 P.M. to 5 P.M.	5 P.M. to 6 P.M.	6 P.M. to 7 P.M.	About 7 P.M.	3 P.M. to 4 P.M.	4 P.M. to 5 P.M.	5 P.M. to 6 P.M.	6 P.M. to 7 P.M.	7 P.M. to 8 P.M.
feet.	feet.	°	°	°	°	°	°	°	°	°	°	°
0	100	1·0	1·4	1·2	0·9	0·5	0·0	1·2	1·2	0·8	0·6	0·5
100	200	1·0	0·8	0·7	0·7	0·5	0·1	0·9	0·7	0·6	0·6	0·5
200	300	0·9	0·7	0·6	0·7	0·5	0·2	0·9	0·6	0·6	0·6	0·5
300	400	0·9	0·6	0·5	0·7	0·5	0·3	0·7	0·5	0·6	0·5	0·5
400	500	0·8	0·6	0·4	0·6	0·5	0·3	0·5	0·4	0·5	0·5	0·5
500	600	0·7	0·5	0·4	0·5	0·5	0·3	0·5	0·4	0·5	0·5	0·5
600	700	0·6	0·5	0·4	0·4	0·5	0·4	0·4	0·4	0·5	0·5	0·5
700	800	0·6	0·5	0·4	0·4	0·5	0·4	0·4	0·5	0·5	0·5	0·5
800	900	0·5	0·5	0·4	0·4	0·4	0·4	0·4	0·4	0·4	0·5	0·5
900	1000	0·5	0·5	0·4	0·4	0·4	0·4	0·4	0·4	0·4	0·5	0·4

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[1] Abstract of a paper read at the Havre meeting of the French Association by Mr. James Glaisher, F.R.S.

This series of experiments proved that which was only indicated in the ascents with a free balloon, viz., that the change of temperature with increase of elevation has a diurnal range, the change being the greatest at about midday and the early afternoon hours, decreasing till about sunset, at which time, when the sky was free from clouds, there was little or no change of temperature up to the height of several hundred feet. I was not able, by means of M. Giffard's balloon, to take any observations at about noon and early afternoon hours, nor any observations after sunset, as the balloon never ascended at these times; but such observations were greatly needed, as there seemed to be at this time a very high probability that the temperature of the air at night must increase with elevation.

A thermometer was placed at the height of 22 feet, sufficiently protected from the effects of radiation, and a second one at the height of 4 feet, and eleven years' observations of these instruments have been taken daily at 9 A.M.; noon; 3 P.M.; and 9 P.M.

These observations were reduced by taking the difference between the readings of the two thermometers and affixing the sign + to that difference when the temperature was higher at the higher elevation; and the sign − when lower. By taking the mean differences for each month between the temperatures at 22 feet and 4 feet, it was found that at all hours of the day during the months of January, February, November, and December, in the afternoon hours of March, September, and October, and night hours throughout the year, the sign was + and that it was − at all other times, clearly indicating the fact of an increase of temperature with increase of elevation during the night hours throughout the year.

THE HEAT PHENOMENA ACCOMPANYING MUSCULAR ACTION

THE fact that in the living muscle heat always appears when the muscle does work (Heidenhain having shown that of two muscles equally weighted and undergoing equal contractions, one doing external work, while the other does none, the former gives out more heat than the latter), is an exception to the general rule in mechanics, that heat disappears when work is done. It is not, however, in contradiction to the general principle of the conservation of energy, but shows that in the living muscle, when stimulated to action, molecular processes occur, which, along with the doing of work, cause a development of heat. The relation of the heat developed to the work done had not been determined with any satisfactory accuracy, probably owing to the want of sufficiently delicate apparatus, though it might naturally be expected to help to an understanding of the phenomena. The subject has been taken up by M. Nawalichin, who, favoured by the experimental means at hand in M. Heidenhain's Physiological Institute, made a careful examination of the development of heat in the active muscle. The experiments were very difficult and tedious, and by reason of the smallness of the values to be measured, required very great foresight and care in the experimental arrangements. The full account of this investigation is given in *Pflüger's Archiv.*

The first series of experiments bore on the question of the production of heat when a particular muscle of the frog is excited, through the nerve, by stimuli of increasing strength to increasing contractions. As, during the experiments, the excitability of the preparation varies, the relation to the strength of stimulus was left out of account, and only the ratio between development of heat and height of contraction examined. The height of contraction was indicated graphically by the muscle itself on a smoke-blackened plate. The development of heat was measured by the deflection of a fine thermo-multiplier, and the stimulation of the nerve was effected by accurately measurable electric actions. The observations were only made when the needle was entirely at rest, which was very difficult to secure, so sensitive was the apparatus.

The tabulated numbers from experiment show: (1) that the sum of the *vis viva*, liberated in the muscle by increasing stimuli, increases only so long as the lifting-heights (*Hubhöhen*) increase. With a certain amount of stimulus when produced by the sending of a constant current, the height of contraction reaches a maximum, and therewith, too, the production of heat. With a particular method of stimulation there is, under certain conditions, a fresh increase of the amount of contraction above the maximum amount, the so-called "supermaximal" contraction; where this occurred, the heat-production also rose. It may therefore be said that in general the development of heat increases with increased lifting-height, and decreases with decreased lifting-height.

The increase in heat-production, however, does not take place proportionally to the increase in lifting-height, but in much quicker ratio. Of this unexpected result M. Nawalichin assured himself by repeated discussion of the numerical values obtained; but he did not succeed in determining more precisely the law of increase.

This result led to the expectation that the same mechanical work of a muscle would be accompanied by unequal heating when the muscle raised a weight to the same height by several small contractions, and when it raised it by one great contraction. In a great contraction more heat would become free than in several small ones, the sum of which was equal to the great. Experiments (though some were difficult) fully confirmed this, especially after it was ascertained that the cooling during the longer period of the several smaller contractions as against the shorter duration of the great contraction, did not play a part.

It is shown, then, that as the stimulation increases, the temperature of the muscle, and accordingly the exchange of material, increase in much quicker ratio than the mechanical work, and that the stronger the stimulation the less favourable is the relation of the exchange of material to the doing of work.

These facts are in accordance, as M. Nawalichin points out, with the common experience that the climbing of a hill is much less heating and exhausting when we go zigzag than when we go straight up. In the former case a greater number of small liftings of the body result in the same doing of work as occurs in the second case through a smaller number of great liftings. The exchange of material, as the second series of experiments show, must essentially be greater in the second case than in the first; and on the amount of it depends, on the one hand, the development of heat, on the other the exhaustion.

In order to get at the inner connection of the phenomena observed, M. Nawalichin sought first to decide the question whether the accelerated increase in production of heat was due to the increase of the stimulus in itself or to the increase of the contraction produced by the increased stimulation. According to Helmholtz's observations, when a muscle is subjected to two maximum stimuli, one following close on the other, the second stimulus produces an increase of contraction only when, at commencement of the second contraction, the first has already reached a considerable height. If this be not the case, as happens if the interval of the two stimuli be less than $\frac{1}{600}$th of a second, the two stimulations produce no greater contraction than each alone. Now in what way does the production of heat occur in this latter case? Experiment showed that also with double stimulation of the nerve, an increase of the heat-development only occurred when it had as result an increase in the height of contraction; the increase of the stimulus in itself is thus without influence on the amount of heat-production. Hence the cause of the quicker increase of the heat-production. That of the amount of contraction must be sought in conditions operative during the course of the contraction.

To determine these conditions the author made experimental inquiry into the relation of heat-development to the states of tension of the muscle during the progress of contraction. He found that the muscle developed less heat the less its tension before action; with which may be mentioned that this tension of the muscle, weighted and stretched by the weight, is smaller the more it has, through contraction, approximated to the natural length. Experiments, also, as to the relation of the heat-production to the change of state of tension during the act of contraction showed an influence of this, such that in each moment of action the quantity of heat depends on the tension. This suggested the idea that the greater heat-production with increasing stimulation is perhaps a consequence of the longer duration of the stronger contraction. The experiments proved, however, that this idea is not justified, for the muscle made small and great contractions in the same time.

As to the nature of the internal processes in the muscle, which may be the basis of the phenomena observed, M. Nawalichin offers the following remarks:—

"We know that the contracting muscle is a body of variable elasticity; with increased contraction its elastic force becomes less, its extensibility greater. When the muscle raises a given weight about four millimetres, the external work for each millimetre of the lifting-height is indeed the same but nevertheless the doing of the same external work for every successive unit-length of the lifting-height will require a larger sum of contractile forces than for every earlier one, since the muscle, even with progressive contraction, varies as to its elastic properties in the direction of an increase of its extensibility. Upon the weight hung to the muscle act, when contraction occurs, both the contractile and the elastic forces of the muscle. . . . In the sum $(c + e)$ of the contractile (c) and the elastic forces (c), e becomes at first (during the contraction) smaller, with the natural unweighted length of the muscle equal to *nil*, and later, even negative. If the weight, then, be lifted a number of units of length, the value of c must increase with increasing contraction. . . . . But an increase of the contractile force is only possible through increased transformation of elasticity into *vis viva*, that is, through exchange of material, which finds its expression in the increased formation of heat which I have observed. Thus, if I mistake not, the facts discovered by me connect themselves with other relations already known, and will find their application in a future theory of muscular forces."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—It is stated that the Home Secretary has appointed as joint secretaries to the Oxford University Commission the Rev. Thomas Vere Bayne, Censor and Student of Christ Church, and Thomas Francis Dallin, late Fellow of Queen's College, and Public Orator of the University of Oxford.

CAMBRIDGE.—The death is announced of Dr. Geldart, Master of Trinity Hall, Cambridge, in the eighty-first year of his age. He had held the mastership twenty-five years, having succeeded Sir Herbert Jenner Fust. Dr. Geldart graduated as seventeenth wrangler in 1818.

LONDON.—Besides those already announced, the Rev. J. F. Blake and Mr. Lebour are, we believe, candidates for the vacant geological chair in University College.

NOTTINGHAM.—The ceremony of laying the foundation-stone of the University buildings at Nottingham has been fixed for Thursday, the 27th inst. The ceremony will be performed at noon by the Mayor, and subsequently there will be a public luncheon in the Albert Hall, at which Mr. Gladstone is expected to be present. The cost of the buildings, including the land, will be 60,000*l.* Of this sum an anonymous donor has contributed 10,000*l.*, and the remainder will be found by the Corporation, who have given the site. There will be lecture and class-rooms for the promotion of the Cambridge University Extension Scheme, which, it will be remembered, was first incorporated at Nottingham, and which has since been carried out successfully in several centres of industry. There will also be class-rooms, laboratory, &c., for the use of the students in the science classes in connection with the local Mechanics' Institution, as well as rooms for the Free Library and the Natural History Museum.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, September 10.—M. Peligot in the chair.—Experimental researches on the mechanism of the formation of sugar in the liver, by M. Cl. Bernard.—Referring to the preceding paper, M. A. Trécul then read a treatise on the formation of starch and of cellulose in plants.—M. Th. du Moncel then presented to the Academy a copy of his "Recherches sur les meilleures conditions des électro-aimants."—On the variation of atmospheric pressure at different altitudes, determined at the Puy-de-Dôme Observatory, during the cyclones of last winter, by M. Alluard. The author found, on comparing the barometrical readings at the Puy-de-Dôme Observatory with those of Clermont Observatory, that the most remarkable discrepancies existed, the barometer having frequently risen several millimetres at Clermont, when at the same time it fell considerably on the Puy-de-Dôme. He asks whether the supposition is justified that, while a cyclone passes over the land, other smaller cyclones are situated inside of it and remain at different heights, without reaching the ground? Or does the strange phenomenon result from local reasons which appertain to the relief of the Dôme's chain and to the relative position of the two observatories? In all cases this phenomenon shows the necessity of studying the atmosphere in different layers and the great importance of the Puy-de-Dôme Observatory.—On a process of preserving the flesh of fish (extract from a note), by M. R. M. d'Amélio.—On the presence of phylloxera in the department of Loir-et-Cher, by M. J. Duplessis. The writer has found that the pernicious insect has now penetrated as far as Villebarron, and the district infested near Orleans now has the shape of a vast equilateral triangle of 60 kilometres side.—M. Ed. Prillieux then read a note on the causes which have brought about the invasion of phylloxera into the Vendôme district.—M. J. Maistre in a letter to M. Dumas speaks of the effects of sulphocarbonates against the insects.—The Minister for Agriculture and Commerce wrote a letter to M. Dumas on the same subject.—M. Faye then drew the attention of the Academy to some interesting results obtained at Washington Observatory by the observation of the two satellites of Mars recently discovered. It appears from a communication made by Admiral J. Rodgers, that in the telegram first sent to Europe by the Smithsonian Institution at Washington there was a mistake, viz., in ascribing to the inner satellite a distance of fifty seconds; half of the major axis of its orbit amounts only to thirty-three seconds of arc.—A letter was then read by the president from M. Ch. Lamey on some observations he made during the winter of 1864–65, and which caused him to believe that Mars is surrounded by a ring of asteroids of all sizes, and as a whole resembling, in some respects, the ring of Saturn. M. Lamey had observed an uncertain reddish light on each side of the disk of the planet and corresponding nearly to its equator. He directs the attention of the observers of the two new satellites to this phenomenon.—M. Leverrier then announced the discovery of another new planet in the zone between Jupiter and Mars, by Mr. Watson, at Ann Arbor, on the 3rd instant, R.A. 23·10m. Dec. + 0° 45′. Daily motion in R.A. 55s.; in Decl. − 1′; magnitude 11.—On the theory of the small motions of a weighty point on a fixed suface which is described round a vertical axis of revolution, by J. Boussinesq.—On locomotives of the compound system, by M. A. Mallet.—On the specific heat and the melting heat of platinum, by M. J. Violle. In the course of experiments made by this gentleman he found the true melting-point of pure silver at 954° C.—A note by M. V. Neyreneuf on the specific induction power.—On nitroso-guanidine, by M. Jousselin.—On the methods which the ancients must have employed to lift and transport the great Celtic or Gallic monoliths, by M. E. Robert.—A note by M. L. Hugo on some curves representing certain elements of the planetary system.

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THURSDAY, SEPTEMBER 27, 1877

URBAN J. J. LEVERRIER

OUR readers will not have been altogether surprised at the announcement of the death of the Director of the Paris Observatory. We have had frequent occasion recently to refer to the unsatisfactory state of M. Leverrier's health, and only a fortnight since we announced his return to his post. He died, however, on Sunday last at the age of sixty-six years, singularly enough on the thirty-first anniversary of the discovery of Neptune.

The gigantic extent and utility of Leverrier's work has been such that it is not possible at such short notice to present anything like a worthy estimate of it. We shall therefore in the present notice content ourselves with referring to a few of the chief events in his career.

Urban Jean Joseph Leverrier was born at St. Lô, March 11, 1811. He was educated at the École Polytechnique, where he so distinguished himself as to be allowed to choose what branch of the public service he wished to enter. He selected a post under the "Administration des Tabacs," and while in that position published his first paper—a chemical one. But he soon gave up chemistry to devote himself almost entirely to mathematical astronomy. In 1839 he contributed two papers to the Paris Academy on the secular variations of the orbits of the planets. These papers attracted the attention of Arago, who requested Leverrier to calculate afresh the perturbations of Mercury with reference to the attraction of other bodies. This may be said to have been the beginning of the great work which he carried on till his death, and with which his name will be for ever associated.

The nature and the stupendous extent of this work were most admirably stated by Prof. Adams, in presenting last year to Leverrier the gold medal of the Astronomical Society. This address is referred to elsewhere. Though drawn aside for a little into the political arena by the events of 1848, he never discontinued his work in connection with the planetary orbits. While in the Legislative Assembly, as member for his native department, La Manche, he gave his attention mainly to subjects connected with public education and scientific discoveries. He has had much influence in these directions, and it is largely owing to him that the École Polytechnique has attained its present high organisation. In 1852 Leverrier was a Senator and Inspector-General of Superior Education. On the death of Arago, Leverrier, as might have been expected, was appointed to succeed him as head of the Paris Observatory. That institution he found in a most disorganised and altogether unsatisfactory condition, and Leverrier set himself earnestly to raise it to the level which it ought to occupy. A work of this kind he could not accomplish without giving offence to many interested in the continuation of the old ways. Indeed, Leverrier's rigid rule caused such discontent among his staff, that the Government were actually compelled to dismiss the great astronomer from his post in 1870; he was, however, restored again in 1873.

While Leverrier's great work was in the sphere of mathematical astronomy, he by no means neglected other departments of science connected with the work of an observatory. To him is mainly due the organisation of the existing meteorological service in France, which depends largely upon local effort. He was ever ready to afford facilities to others to carry on their own researches within the Observatory precincts, and he saw with pleasure the erection of new Observatories both in Paris itself and in the provinces.

"If on no other than selfish grounds," to quote the *Times* notice, "England, as a maritime country, cannot fail to pay a tribute of respect to a man whose work has been of the utmost practical importance in the construction of tables used in guiding ships across the seas. Nor has England been, in fact, niggardly in rendering him honour. On four occasions living words of respect and friendship from England have been addressed to M. Leverrier by presidents of the Royal Society and the Royal Astronomical Society when presenting medals, which are by tradition regarded as the highest tribute the societies can offer of their appreciation of the value of work done. In 1846 the Royal Society, under the presidency of Lord Northampton, presented to him the Copley medal. In 1848 the Royal Astronomical Society, under the presidency of Sir John Herschel, awarded a testimonial; in 1868, under the presidency of the Savilian Professor, the gold medal; and again in 1876, under the presidency of Prof. Adams, M. Leverrier's 'rival' in the discovery of Neptune, a second gold medal. Two years ago the University of Cambridge, at the suggestion of Prof. Adams, conferred on him the honorary degree of LL.D. Perhaps the most valued, because most practical, recognition that could be offered was the fact that for years past his tables have been employed in our 'Nautical Almanac,' superseding all others for the computation of the places of the planets."

Leverrier's work has been of such value to a maritime nation that a mark of appreciation on the part of our Government, as well as on the part of our societies, would not have been out of place. At the funeral on Tuesday, English science was represented by Mr. Hind, the distinguished superintendent of the *Nautical Almanac*—we hope in an official as well as in his private capacity.

M. Leverrier was Inspector-General of Universities, one of the highest dignitaries in the Legion of Honour, and a member of almost every Academy and order of merit in the world. Of his two sons one died two years ago, and the other is an engineer in the Ponts et Chaussées. Madame Leverrier has not been well for a long period, and no doubt the shock of her husband's death will tell upon her constitution.

Owing to the great loss sustained by science in the death of Leverrier, the Paris Academy of Sciences closed its session on Monday immediately after the letter from M. Tresca announcing the sad event had been read. M. Tresca was able to state that the great life-work of Leverrier had just been completed.

RECENT BOTANICAL BOOKS

Text-book of Structural and Physiological Botany. By Otto W. Thomé. Translated and Edited by Alfred W. Bennett, M.A., B.Sc. (London: Longmans, Green, and Co., 1877.)

THERE is a manifest want at the present time of a text-book of moderate size which would supply English students with a general view, not running too far into details, of the distinct types of organisation existing in the vegetable kingdom. Such a book should, as far as possible, draw its illustrations from plants which the teacher could without any very great difficulty place before his pupils. After intelligently comparing the descriptions with as many of the structures described as opportunity afforded, a student should have a tolerably clear idea of the leading facts in the comparative vegetable physiology and morphology of plants. Sachs's "Text-book" is a perfect mine of information of the most accurate and recent kind on all manner of special points, but it is rather a book for the advanced student to consult than for the beginner to study. It is often difficult in it "to see the wood for the trees," to keep well in view the firm grasp of fundamental principles of organisation which the distinguished author undoubtedly possesses, but which the richness of the materials that he has on the whole so ably marshalled cannot, especially on a first reading, but very considerably obscure. Huxley and Martin's "Elementary Biology" is undoubtedly very useful for first breaking ground, but the vegetable types are of course treated in relation to the animal and with the view of bringing out certain general laws applicable to the whole of organised nature. The commencing botanist of course wants something more specialised than this, and for a long time past there has been perhaps little better in English than the able and philosophical sketch of the vegetable kingdom which is contained in the opening chapter of Dr. Carpenter's "Comparative Physiology."

At first sight it seemed as if the desideratum had been supplied by Thomé's "Lehrbuch der Botanik," of which an English translation has recently been published by Mr. A. W. Bennett. An examination of its pages is, however, disappointing. There is an entire want on the part of the author of any definite grasp of his subject, and this, combined with a good deal of vagueness and inaccuracy in the facts, preclude the book being regarded as possessing any higher value than a mere compilation which is only not very bad because recourse has been had to fairly good sources of information. All through there are evidences that the author has not a practical familiarity with the subject on which he is writing. A few instances will suffice to illustrate this defect. On p. 10 we are told "Protoplasm . . . which is inclosed in a cell-wall has . . . no power of escaping from its envelope;" but the author confutes himself on p. 284 by stating *à propos* of Myxomycetes: "The germinating spore, now provided with a cell wall, allows the whole of its protoplasmic contents to escape." On p. 24 there is the extraordinary statement that in some *Muscineæ* starch-grains "are points of crystallisation around which the mass of chlorophyll has been deposited." And a little further on, speaking of the chlorophyll corpuscles of *Metzgeria*, we are told "They multiply also by division, splitting up into two new bodies, each capable of independent life;" yet chlorophyll-corpuscles are incapable of life independent of the cell of which they are specialised constituents. On p. 29 it is stated that the "purpose of the formation of starch is that it may be stored up in the cells as a reserve food material," which is about as just a view of this important process of nutrition as if we were to say of wheat that the purpose of its cultivation was to fill granaries. After this we may pass over as comparatively unimportant, the queer statement that starch is "deposited in especially large quantities in . . pollen-grains" of all things in the world.

On p. 32 we are told that cell-division commences by "the protoplasm . . . contracting into a spherical form." The figure on the opposite page tacitly corrects this error.

On p. 44 we learn that "*Periderm* consists of tabular cells with thicker walls, which, when looked at vertically, have a regular polygonal or stellate appearance." This is far from clear, and certainly not universally true. On p. 52 we are informed:—

> "The laticiferous vessels and the true vessels together have been compared to the venous and arterial blood-vessels of animals; but since a direct connection between them has not been proved, and the mature vessels are normally filled with air, this comparison cannot be maintained."

In an educational book surely we may ask that the *débris* of effete hypothesis should be left to slumber in its appropriate oblivion. Any one who has ever examined students will shudder to think how often this quaint relic of the phytotomy of two centuries ago will be trotted out triumphantly when far more important things are altogether forgotten.

Passing over the histology—the treatment of which cannot be regarded as satisfactory—we find a section on "The external form of plants" which, under this apparently philosophical heading, simply conceals the old dreary sterility of the descriptive terminology of flowering plants. Even this is wanting in accuracy. Thus, to take a single page (79): "scandent" is given as a synonym of twining, the fact being that it includes every form of climbing *except* twining; again, Solomon's seal is said to develop its flowering stems from terminal buds, while it is obvious that they are axillary, and therefore lateral.

The "Special Morphology" is far better planned, and if thoroughly revised and published separately, might make—as it is well-illustrated, though chiefly with borrowed woodcuts—a handy little text-book. There is the same want of severe accuracy, however, to the confusion o students. Thus (p. 192), the suspensor in phanerogams is termed the pro-embryo; p. 315, the same term is applied to the prothallus of ferns; the structures, of course, are morphologically in no way homologous. Merely to mention defects as they catch the eye in turning over the pages, on p. 274, ascospores are said to be contained in perithecia, oblivious of the asci; p. 275, saprophytic fungi are mentioned when saprogenous are intended; p. 296, *Hepaticæ* are said to form "a beautiful transition from the Thallogens to the Acrogens," which only shows how easily persons may be deluded by mere "adaptive" characters; on p. 309 the author confuses—which is almost incredible—the vascular bundle-sheath with the sclerenchyma in the fern-stem; p. 337, the North American *Callitris* is mentioned, North African being intended, and the two-lobed anthers of *Taxineæ* are mentioned, when *Taxus*, for example, has usually a six-lobed anther, as he tells us on p. 336.

We have not examined very critically the concluding chapters on Fossil and Geographical Botany, but the latter, at any rate, seems too vague to be very useful.

Altogether, we cannot but feel sorry that Mr. Bennett has spent his labour on a work of such little real value. He seems, indeed, to have had some misgivings as to its shortcomings, and has largely borrowed from Huxley and Martin's "Elementary Biology." Thus the student is left as best he may to harmonize Thomé's account of the antheridium of Characeæ, on p. 293, with Huxley's independent description on p. 294, which is transferred almost bodily.

It can hardly be doubted that Mr. Bennett, with his experience as a teacher, could have supplied us from his own pen with a text-book which would have been much more useful.

OUR BOOK SHELF

Natural Geometry; an Introduction to the Logical Study of Mathematics, for the Use of Schools and Technical Classes, with Explanatory Models. By A. Mault. (London: Macmillan, 1877.)

THIS is a good elementary text-book, founded on the work by M. E. Lagout ("Takimetry"), which we have already noticed (NATURE, vol. xvi. p. 226). The ground covered by the work before us is not quite so extensive in one direction as that covered by Dr. Gwynne's translation; but it has an introduction to pure geometry which is likely to be of service to junior pupils. We are disposed to think that some such practical training as that indicated here, with the aid of the accompanying models, and a short course of "practical" geometry would be a capital thing for our junior pupils. Boys who are exceedingly dull and stupid over their "Euclid" often, as we have repeatedly seen, take much interest in these concrete exhibitions of geometrical truths. The book has been very carefully got out; there are a few loose expressions which might be improved. On p. 32 is the statement, "in equal circles equal arcs are those which have equal chords," a distinction should be made between major and minor arcs. Another trifling matter (but some boys would at once notice it) is that some equilateral figures are drawn on p. 33, which are not equilateral by scale. There are two parts—geometry by sight, which treats of the measurement of flat surfaces and of solids, and scientific geometry, or reasoning helped by sight. The latter is concerned with the measurement of accessible and inaccessible things and with the incommensurable (as the circle, sphere, cylinder, and cone). We can recommend the book for school use.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Cycle of Sun-spots and Rainfall

MAY I venture to ask insertion for the following remarks on the cycle of sun-spots and rainfall? I frequently receive inquiries regarding the meteorological aspects of the Indian famine and the prospects of Madras during the coming monsoon. But I beg to state that the object of my investigations is not at present to predict the future, but simply to ascertain the past, facts. When we are quite sure of the data it will be time enough to apply them. In order to secure a stable basis it has been necessary to work up a vast accumulation of meteorological returns which have never been previously collated, and to make further references to India, Germany, and America. Some time must still elapse before the results can be presented as a whole. Meanwhile I should be obliged if you would give insertion to the facts with regard to two points which seem at present to have a special interest to the public.

In the first place, I think I may now safely say that the coincidence of the cycle of rainfall with the sun-spot period is not confined to Madras, but is common to various points around the great ocean tract on which Madras lies. The three such points of observation for which materials exist during the most considerable period of years are Madras, Bombay, and the Cape of Good Hope. I have made up the following table from the monthly, and, when available, from the hourly, returns of these stations; and as there are a few errors (probably clerical) in a return recently given for Bombay by the meteorological reporter at Calcutta, I ought perhaps to add that these returns have been tested by a trained computator in the Scottish Meteorological office. The sun-spot column was worked out from an old list, the only one available to me in India, and will hereafter be revised from the more complete returns issued this year by Dr. Rudolph Wölf. The differences do not, however, affect the general results. My cycle of eleven years starts back from 1876; and the *minimum* group of my cycle, namely, the eleventh, first, and second years, include all the years of *minimum* sun-spots from 1877 to 1810. It will be seen that the coincidence in the cycle at Madras and the Cape of Good Hope is very strongly marked, while that at Bombay is less so, and somewhat lags behind the other two. An explanation exists for this, but it would trespass too much on your space to enter into that side of the question.

TABLE I.—*Eleven Years' Cycle of Rainfall and Sun-spots shown in Periods of Two Years.*

		Average of rainfall in inches, registered at Madras. (1813-76.)	Average of rainfall registered at Cape of Good Hope. (1842-70.)	Average of rainfall registered at Bombay. (1817-76.)	Average relative number of sun-spots (Wölf's old list). (1810-60.)
Min. group.	Eleventh series of years in the cycle of eleven years ...	37·03	21·19	70·32	10·92
	First and second series of years in the cycle of eleven years	42·07	20·98	63·02	10·02
	Av.	40·39	21·05	68·78	10·32
	Third and fourth series of years in the cycle of eleven years ...	49·12	23·92	67·36	39·89
	Fifth and sixth series of years in the cycle of eleven years ...	54·64	28·11	71·22	73·44
	Seventh and eighth series of years in the cycle of eleven years	52·36	27·80	79·34	53·78
	Ninth and tenth series of years in the cycle of eleven years ...	49·02	23·26	76·42	33·54
	Eleventh series of years in the cycle of eleven years	37·03	21·19	70·32	10·92

The cyclic coincidence may be tested in another way. If it really exists there should be a well-marked minimum group at the extremities of the cycle (in the eleventh, first, and second years), and a well-marked maximum group in the middle of the cycle (the fifth and following years). The years on both sides of the central maximum group, *i.e.*, between it and the minimum group at the two extremities, should yield intermediate results, and, when taken together, should form an equally well-marked intermediate group. I therefore divided the cycle (so far as the number 11 permitted) into three equal groups. The "minimum group" is formed by the three series of years at the extremities of the cycle, which include *all* the years of minimum sun-spots in this century from 1810 to 1877. The "maximum group" embraces the four central years from the true maximum year of the rainfall and sun-spot cycle (the fifth) to the second maximum in the sub-cycle of sun-spots in the eighth year. The "intermediate group" consists of the two series of years on both sides of the central maximum group, namely, the third and fourth years on the one side, and the ninth and tenth years on the other side.

TABLE II.—*Cycle of Rainfall and Sun-spots shown in the Minimum, Intermediate, and Maximum Groups.*

	Average of rainfall in inches, registered at Madras.	Average of rainfall in inches, registered at Bombay.	Average of rainfall in inches, registered at Cape of Good Hope.	Average relative number of sun-spots (old list).
Minimum group: eleventh, first, and second years... ...	40·39	68·78	21·05	10·32
Intermediate group: third and fourth, with tenth and ninth years	49·07	71·89	23·59	36·71
Maximum group: fifth, sixth, seventh, and eighth years ...	53·50	75·23	27·95	63·61

I regret that I have not the materials for showing the rainfall for Bombay and the Cape during the whole sixty-four years for which the returns exist for Madras.

The other point on which I venture to trouble you at present has reference to a different class of atmospheric phenomena. I think that it may now be affirmed that a cycle of wind-disturbances exists and is coincident with, although slightly lagging behind, the cycle of sun-spots. M. Poëy called the attention of the French Académie des Sciences to this subject five years ago, and published, as far back as 1873, a list of hurricanes in the West Indies from 1750 to 1873 in support of his views. Dr. Meldrum has worked out the same question, as regards the [East] Indian Ocean, with admirable patience and success. On the publication of my cycle of Madras rainfall it struck Mr. Henry Jeula, Honorary Secretary to the late Statistical Committee at Lloyd's, that the subject might have a practical bearing upon under-writing and marine risks. He collected from Lloyd's Loss Book the statistics for the two last eleven-year cycles (1876-66, and 1865-55), the only ones for which materials were available; and conjointly, we have tabulated the results. We found that the percentage of losses on registered vessels of the United Kingdom was 17½ per cent. greater during the two years of maximum sun-spots (the eleventh and first of the cycles) than during the two years of minimum sun-spots (the fifth and sixth of the cycles). In the same way we found that the percentage of the total losses (calculated on the eleven years) posted on Lloyd's Loss Book was 15 per cent. greater during the two years of maximum than during the two years of minimum sun-spots. We further found that the increase and decrease of losses follows a cycle, closely following (although for sufficient reasons somewhat lagging behind) the cycle of sun-spots. These results can be tested from the succeeding tables.

TABLE III.—*Percentage of Losses posted on Lloyd's Loss-Book, compared with the Eleven-Year Cycles of Sun-spots and of the Rainfall at Madras.*

		On registered vessels of the United Kingdom.	On total losses posted in eleven years.	Average of rainfall at Madras. Inches.	Average relative number of sun-spots (Wölf's lists).
Minimum group.	Eleventh series of years of the cycles	9·93	7·64	37·03	10·9
	Average of first and second years of the cycles	11·91	9·33	42·07	10·0
Average of third and fourth years of the cycles		11·05	8·64	49·12	39·8
Average of fifth and sixth years of the cycles		12·21	9·31	54·64	73·4
Average of seventh and eighth years of the cycles		12·82	9·81	52·36	53·7
Average of ninth and tenth years of the cycles		11·84	9·09	49·02	33·5
Average of eleventh years of the cycles		9·93	7·64	37·03	10·9

Dividing the eleven years as nearly as the number admits by three, into a minimum, an intermediate, and a maximum group, we get the following results:—

TABLE IV.—*Percentage of Losses posted on Lloyd's Loss-Book, compared with the Eleven-Year Cycles of Sun-spots and of the Rainfall at Madras.*

	On registered vessels of the United Kingdom. (1855-1876.)	On total losses posted in each eleven years. (1855-1876.)	Average of rainfall at Madras. Inches (181[illegible]-1876.)	Average of relative number of sun-spots (Wölf's lists). (181[illegible]-1860.)
Minimum Group. Average of first, second, eleventh, and tenth years of the cycles	11·13	8·64	41·58	14·26
Intermediate Group. Average of third, fourth, ninth, and eighth years of the cycles	11·90	9·21	51·37	42·46
Maximum Group. Average of fifth, sixth, and seventh years of the cycles...	12·49	9·53	53·22	64·10

Again, testing the cyclic coincidence, by taking the first four years, the middle or maximum four years, and the remaining three years at the end, a similar result is obtained.

TABLE V.—*Percentage of Losses posted on Lloyd's Loss Book, compared with the Eleven-Year Cycles of Sun-spots and of the Rainfall at Madras.*

	On registered vessels of the United Kingdom.	On total losses posted in eleven years.	Average of rainfall at Madras. Inches	Average relative number of sun-spots (Wölf's lists).
Average of first, second, third, and fourth years of the cycles	11·49	8·93	45·60	24·90
Maximum Group. Average of fifth, sixth, seventh, and eighth years of the cycles	12·52	9·56	53·50	63·55
Average of the ninth, tenth, and eleventh years of the cycles	11·20	8·61	45·02	25·99

I think, therefore, that we are justified in concluding that the periodicity observed by M. Poëy in the hurricanes of the Antilles, and by Dr. Meldrum in the cyclones of the Bay of Bengal is of such a character as to exercise a widespread effect upon the commerce of the world. How far these wind-disturbances may be eventually proved to be general throughout the earth's atmosphere, or throughout any given belt of it, I am not yet prepared to offer an opinion. But that the practical results of such wind-disturbances on maritime commerce are of a general character the foregoing tables now place beyond dispute.

In conclusion, I beg to caution fellow-workers that no really trustworthy results are to be obtained from the old plan of jumbling together a number of unhomogeneous stations in a bag and shaking out averages. The true method is to take certain recognised factors in the rainfall, such as the monsoons, and to examine whether any common periodicity exists between the operation of these factors and the sunspots. This is what I have attempted to do for various points around the great basin which stretches southwards from the Bay of Bengal, and what Mr. Archibald has so carefully done in NATURE for Northern India. I am now conducting a similar inquiry into the American and Australian rainfall, but, as already stated, some time must elapse before the results can be presented. W. W. HUNTER

Allanton House, Lanarkshire, September 20

The Discovery of the Satellites of Mars

As some of the earlier newspaper accounts of Prof. Hall's discovery of satellites of Mars are said to have produced, in some quarters, an impression not fair to him, and as the same accounts may produce the same impression abroad, it seems proper to make the following statement:—

When on the morning of August 17 Prof. Hall showed me his observations, the communication was purely confidential and friendly, and was not made either in the line of duty or because he failed to recognise the significance of his observations, or because any special skill he did not possess would aid in interpreting them. I suggested that, from the few measures he had made, it was possible to estimate the time of revolution of the satellite, if the object really were one; and thus ventured the prediction that it would be hidden during most of the following night, but would reappear toward morning near the position in which it was seen the night before. The fulfilment of this prediction facilitated the establishment of the true character of the object, but, without it, an equally certain hold on the satellite would very soon have been obtained by Prof. Hall alone. The credit of sole discoverer is therefore due to him. Simon Newcomb

The Satellites of Mars

It may interest some of the readers of Nature to know that one of the recently-discovered satellites of Mars appears to have been certainly seen with the six-foot reflector at Parsonstown. My assistant writes me on the 17th instant:—

"On the 8th instant (before receiving the Washington circular) I suspected very strongly at 11·45 P.M., while using the six-foot, that a satellite was visible, following the disc, about 1½ diameters. It appears now from the elements that it must have been the outer one. On the 15th instant, at 11·30 P.M., I saw it *quite distinctly* preceding the planet, however not well enough to measure it, as I lost it again after a couple of minutes, owing to the strong glare of Mars. Last night I saw it again, but only by glimpses, twice or three times."

The unfavourable weather prevented the satellites from being looked for between the 8th and 15th instant.

I may add that it seems probable that the satellites might have been measurable on the 15th instant with the bright line micrometer had it not been in the maker's hands. The low meridian altitude of the planet (25° at Parsonstown as compared with 40° at Washington) is of course a serious drawback to observations at the former place. Rosse

Yorkshire, September 20

The weather in this neighbourhood has been very unfavourable for observation ever since the announcement of the discovery of two satellites of Mars. Last night, however (about 9.30), during little more than half-an-hour's interval of clear sky, the air being extremely steady, and the planet beautifully defined, I succeeded in seeing the outer satellite of the two. With the full aperture of my 18-inch silvered-glass equatorial reflector, and an ordinary achromatic eye-piece with a bar across the field hiding the planet, the satellite was but glimpsed occasionally; with a single double-concave lens (power about 180) it was visible, in spite of the brilliant light of the planet. Had I not known its exact position, however, I question whether I should have seen it at all. It is a most difficult object.

Henry Cooper Key

Stretton Rectory, Hereford, September 19

A Good Suggestion

The approaching meeting in London of librarians representing the most important English, and, I believe, foreign, collections of books, makes the present a suitable time to offer suggestions as to the management of such collections.

It has long seemed to me that an improvement might be made of a very simple nature, but capable of greatly increasing the working value of reference libraries, especially those of the first rank; namely, to provide, somewhat as follows, for their being consulted by those who cannot personally visit them.

Suppose that the authorities of such an institution as the British Museum or the Bodleian designate certain persons, not paid officers of the library, but known to its directors as well-educated, trustworthy, and acquainted with the resources of the particular library; publishing the names and addresses of these gentlemen as willing, and believed to be competent, to undertake researches amongst the books for those who may write to them from a distance; the official authorities assuming no actual responsibility for the work so done, but merely recommending the persons to do it; publishing at the same time a definite statement of the payment expected per day or hour by these persons.

Often when one would desire to consult a great public library in a foreign land, or in a distant part of one's own country, nothing short of a personal visit would be of use, but in very many cases it would be quite possible to obtain all that one desired by a simple business-like correspondence with a proper agent. Sometimes the question is merely whether such or such a book exists in the library, with perhaps an accurate copy of its title; sometimes a special reference to a single page in an old and scarce scientific journal or set of transactions is to be verified; sometimes a few paragraphs are to be copied in the exact words of the author; sometimes a name, date, or number is to be sought out; sometimes a larger amount of work would be needed, but so definitely shaped out that instructions in writing could easily be given for it to an intelligent person on the spot. As it is, the consulting of such a distant library in person is often simply impossible, and even when possible, often involves such expense and delay as to make themselves seriously felt; whereas by the plan proposed, the object in view might often be attained at a cost of time and money altogether trifling. In my own very small experience I once found it necessary to travel some 700 miles, losing three days, and spending about 7*l.*, in order to refer to a book for about ten minutes, while directions for making the same search could have easily been put upon half a sheet of note-paper, and carrying them out would have occupied a person living in the city in which the library was situated altogether not more than an hour or an hour and-a-half.

In the neighbourhood of almost every large library competent men might readily be found to undertake such work as is suggested, and to whom the opportunity of increasing their income, or probably in time earning from this source alone a satisfactory income, would be welcome. The plan would admit of being carried out upon a small or an extended scale; a library of the third or fourth class might afford a field for a single man only, while one of the first class would be likely gradually to enlist the services of a number; if this were so sub-division of labour would be desirable, one person undertaking researches in natural history, another in mathematics, physics, or chemistry, another in classical learning, &c.

While such work could not properly be done by the regular officers of a public library, it would be important that the private individuals who were to enter upon it should have the approval of, and should be recommended by, the library authorities, who might also very properly fix the rate of payment, recommending only those who were willing to accept the rules laid down.

This plan has at least the merit that it might be tested with very little trouble, risk, or disturbance of existing arrangements. I believe that even in England with great libraries situated at comparatively moderate distances from almost every one in the kingdom, it would prove a great convenience; to persons placed as are those who live here in America, with no library of the first rank on this side of the ocean, and with hundreds of miles often separating one from the larger of even those libraries which do here exist, the boon of access by letter to the greatest collections of the world would be inestimable. It would be in a new direction, and a noble one, carrying out the tendency of the most modern civilisation which looks to placing, as far as possible, the resources of the whole earth within the reach of him who lives at any one spot upon its surface J. W. Mallet

University of Virginia, Setember 5

Some of the Troubles of John O'Toole respecting Potential Energy[1]

II.

B.—*Potential E.*, as meaning "*energy related to Potential Functions.*"

We now pass to the second meaning of "potential E." It happens, by a most singular and unfortunate coincidence, that this class of E. can very well be called by that title for a reason quite distinct from that which we have been deprecating. The idea of the potential function, or briefly, potential, was first formed and thus named by Green. It has no reference whatever to existing in possibility; it is concerned with present potency or power; and it happens that potential E. of unit of mass may be very appropriately so termed by some physicists as being E. of potential, or rather E. of difference of potential, or E. which is the complement of potential. This is evidently sometimes in the minds of our teachers: indeed, Clerk Maxwell directly tells us[1] that this is one sense in which the title is suitable; he calls it a "very felicitous expression," because it has the two differently applicable meanings we have mentioned.

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[1] Continued from p. 441.

7. Here, then, is our next gravamen. These two characters of this type of E. are quite heterogeneous and unconnected. Now a *simple name* can only refer to one character of a person or thing; and if it happens, by accident, that the *word* that constitutes the name has two quite different meanings or references, and that both are applicable to the person or thing, if we mentally apply them both to that individual, we are guilty of a sort of punning or verbal skylarking; as if for instance we should call Mr. Smith an upright man on account both of his erect carriage and of his moral probity. This is bad enough; but further, the two characters which might be implied in the name "potential E." are not merely heterogeneous; they are mutually incompatible; they cannot be put together into the same complex idea, at least by ordinary mortals. Surely there is no occasion to stop to prove this.

It is evident that the majority of our teachers feel this and the preceding inconveniences themselves. And I confess that I am now going to bring against them a more serious accusation than that of merely using unsuitable language. There is a most singular and apparently significant omission to be noticed in nearly all the manuals, referring to this subject, into which I have looked; we have already alluded to this.

8. But we must take this opportunity of numbering it as the eighth of our gravamina. It is this, while teaching us about the different classes of E. and telling us that one is called "potential," they abstain from telling us why it is so called! This omission is so remarkable, in itself, as occurring in books intended to impart instruction, and so unlike the ordinary behaviour of our doctors, that there must be some very particular reason for it. A single person might make this omission by pure accidental inadvertence; but when a number of persons do so it cannot be thus accounted for. There is no explanation but this—that they perceive the botherations connected with this confounded, I mean confounding, name, "potential E.," and rather than acknowledge how matters stand, and own themselves to blame, they try to slur the thing over by giving no meaning of the name at all. Rankine, indeed,[2] just alludes to the last meaning of "potential E.," which refers to its connection with potential (function); but that is all; not a word about the incongruity between it and his own original meaning, just as if none existed. Clerk Maxwell, however, as we have just seen, boldly takes the bull by the horns, and tries to make both himself and us believe that he is delighted with this Janus-like name and with the compounding of its two incompatible meanings. It so happens that the writings of that distinguished physicist contain as striking an illustration as could be conceived of the inconvenience of the ambiguity of this "very felicitous expression." We have already mentioned it, but with a different object in view. Having told us in one place that "potential E." is "the E. which the system has not in actual possession," he also tells us elsewhere[3] that "the leaden weight of a clock when it is wound up has potenial E., which it loses as it descends." The weight sets-to and works with E., which it has not in possession, but only has the power to acquire, and which it loses the power of acquiring!![4] In the first statement he was thinking of the first meaning of potential E. and in the second statement of the other meaning.

It might be said that if we discard the first meaning of "potential E." on account of its intrinsic wrongness, we shall, at the same time, abolish this last difficulty, which arises from its relation with the second meaning, and that this second meaning, which is admittedly good *in itself*, will then have nothing against it. But in the first place the associations of the name "potential E." with the first meaning are too strong to be easily got rid of. It would be all but impossible to retain the word and confine it strictly to the second meaning.

9. But besides, "potential E." in this second meaning, though good in itself, has inconveniences independent of this when applied as I believe it universally is, to E. conceived of as existing in *the body* moved; for potential (function) does not appertain to the *body* moved, but entirely to the *force* concerned.

C.—*Potential E., as meaning "Energy of Potency."*

As to the third meaning of "potential E.," it has been said (and indeed Rankine may have had this in view in one place[1]) that it need not be taken to imply anything more than E. of potency or power without reference to Potential (function)

10. But according to this, "potential E." would mean the power of doing work which consists in power; and it would be as great a tautology as "umbrageous shades."

11. And again, if it be the special *distinction* of one class of E. that it is E. of potency it necessarily follows that the other class, observe the so-called actual E., is *E. of impotency!*

12. And besides, there is the same incompatibility between this meaning, C, and meaning A, as there is between B and A.[2]

As to the whereabouts of Potential Energy.

We shall now pass from the perplexities connected with this unlucky name, "potential E." to consider the behaviour of our teachers towards the thing itself. It will conduce to clearness to drop this name now, since our objections are no longer directed against *it*, and adopt another very common one for the same thing, viz., "E. of position."

13. The E. of position is usually regarded and spoken of as belonging to, or being *in the body* in question which may be about to move and acquire E. of motion. This puzzles poor P. terribly; not only on account of the difficulty of grasping the thing mentally and of putting any clear meaning into it, but also because the doctors, both individually and collectively, often display such curious inconsistency respecting it.

But before proceeding to consider directly the undesirableness of this way of viewing E. of position, let us observe that it is the cause of all the above perplexities, which, indeed, seems to be sufficient objection to it; and let us endeavour to find out why the doctors should have had recourse thereto.

The physicists having determined, for the reasons below, to talk of this E. of position as being in the body, and that body being just the same (and, when regarded as attract*ed* or repell*ed*, as is usually done, equally inert) in whatever position it stands, it becomes necessary to provide for this by a little ingenious dodge; for such the phrase "potential E.," as now generally used, really is. "Potential E." plays the same part as a conjuror's empty case or shape, which is made to represent some solid object which is really lying elsewhere, or is perhaps actually doing duty there. Our physical prestidigitateurs tell poor P. that this E. is "in the body," that the body "*has* it," that the body "possesses it," with other similar expressions. But what is it that they are presenting to him all the time? "Potential E.," which sounds to him very fine, and which he thinks must be something very serviceable, but which is in reality only an empty shape, for it is "E. which the body *has not* in actual possession"!! They have adopted the precise inverse of the famous device employed by Ulysses when he told Polyphemus that his name was Οὖτις. We have mentioned and reckoned this grievance already, on its own account; we have returned to it now only to show how the present one necessitates the use of this delusive name "potential E."

Why then is it that our *teachers* (save the mark!) wish thus to make-believe that they have got their E. of position in the body? The principal reason is this—They have to keep straight with the metaphysicians. In these days it is generally perceived that we should, as much as possible, avoid treating force as an objective *something*. When energy does not come prominently forward into discussion they can use the same forms of expression about force as their grandfathers did, though intending them only as such. The term "force" is "very useful," in that "it enables us to abbreviate statements which would otherwise be long and tedious;" and no harm is done by using it when the necessary reservation as to its being only a convenient mode of speech is known to underlie all the statements and discussion. But when Energy, which must be taken as real and objective in some sense, is the subject of their talk, they become extra cautious, and, fearing to put this objective affair into non-objective force, or tension, or stress, they are driven to thrust it into the body, notwithstanding the perplexities and contradictions caused by so doing, and notwithstanding the painful necessity incurred thereby of hoodwinking poor P. in the above manner, and endeavouring to hoodwink the metaphysicians. But there is really no reason why force with the saving reservation should not be introduced as freely into the discussion of E. as into other questions of dynamics; and the physicists often do introduce it thereinto; but then, when frightened at what they have done, they will silently withdraw it again. All the inconsistencies of the doctors, and their capriciously varying moods of freedom and shyness respecting "force," and their stepping up and down from one platform of thought to another, perplex poor P. beyond measure. He knows nothing but what they tell him; and he dares not attribute his difficulties to anything but either the abstruseness of the subject or his own stupidity.

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[1] "Heat," p 91; "Matter and Motion," p. 81:—"Potential E.—A very felicitous expression, since it not only signifies the E. which the system has not in actual possession, but only has the power to acquire, but it also indicates its connection with what has been called (on other grounds) the Potential Function."

[2] *Phil. Mag.*, February, 1867.

[3] "Theory of Heat," p 281.

[4] We have already seen above, that the weight never acquires more than a quite insensible amount of "actual E.," so called.

[1] "Encyclop. Brit." (1857), vol. xiv., article "Mechanics."

[2] In Nicholl's "Cyclop. of Phys. Sciences" potential E. is said to mean E. of *a* power or force, but it is easy to see that this does not mend the matter at all. As a *distinguishing* title it implies that a moving body has no force nor power, no "power of performing work," that is to say no E.

But probably there was another motive, also, for this melancholy idea of putting the E. of position into the body, viz., the desire for simplicity of arrangement. Since E. is E., and the kinetic E. is undeniably in the body, it would seem to be an orderly proceeding to put the other there too. But this would be as if a methodical housekeeper should keep her coals and her blankets in the same "hole" because they are both warming apparatus, though in very different ways. And besides, we shall find that whatever may be the gain in this respect in putting both the types of E. into the body, it is outweighed by a certain loss of true correspondence and clear analogy, which will be mentioned farther on.

We now come to the more direct consideration of the merits of this procedure of putting the E. of position into the body. Let us begin with an interesting little illustration of its character. It is the ordinary and legitimate mode of expression to say that when a stone is projected vertically upwards, the gravitational attraction between the earth and stone draws the stone down again and gives it the kinetic E. with which it strikes the earth. And the gravitation attraction is usually and conveniently conceived and spoken of as being all the earth's: and the stone is usually regarded as being simply attract*ed*. Every doctor will frequently speak thus; and nevertheless he will also, and sometimes in the same breath,[1] tell us that it is the *stone*, say at the highest point of its ascent, that has E. of position due to its height from the ground. So then the connecting attractive force, which is to do the work of drawing the stone down again, and which is therefore one factor of the E. present, is regarded as being in the earth, but the E. as being in the stone! This is *one* way no doubt of teaching poor P. the difference between force and E! Take another illustration. Some of our foremost doctors[2] tell us that when a bow is drawn and about to discharge the arrow or the bolt it is the arrow or the bolt that has E. of position; in this they have at least the merit of consistency. Poor P. generally feels that this conveys no distinct idea at all to his mind; of course he dares not thing it wrong. Then he finds other doctors[3] who tell him (though in so doing they are inconsistent[4] with themselves) that in this case the E. is in the bow. What is to be done now? Is this distracting E. of position "like a bird so that it can be both here and there at the same time"? Or are the doctors on one side—how shall we write it—wrong? At any rate, since the doctors differ, poor P. must needs choose for himself, and in order to escape the above perplexities and also for the following reasons, he elects to conceive of the E. of position as not in the body but in the force or forces concerned which are at least virtually there; it being an ulterior and quite another question, what is force?

The discussion is of course now, as it has been all along, only as to modes of conception or of expression, and not as to the science of our doctors. All agree that if you *spend* E. against the resistance of the inertia of a mass in giving it velocity or acceleration, you have bestowed your E. on the inertia of that body, you have transferred your E. to that inertia. So, in exact correspondence and analogy, if you spend E. against the resistance of the gravitation attraction, for instance, in raising a stone to a certain height you have bestowed your E. on that attraction, you have transferred your E. to gravity. That attraction was beforehand pulling at the stone as hard as it could; but it had no power of doing work, according to the definition of work, *i.e.*, it had no energy according to the definition of E. You have given it E., or the power of performing work by affording it the condition necessary for its doing work, viz., space to work through. Why will not the doctors say this in so many words, when they do say it virtually in various forms? From Newton down they tell us this, that the work done by a force is fs (s being the space through which the force f acts); but the work done is the measure of the preceding E. or power which of course the force had of doing that work; why then will they scarcely ever say that the E. of a force is fs (s being now the space through which the force will have opportunity of acting)? When they do say in substance what we want them to say, they avoid most carefully the direct clear statement of it in so many words.[1] "This kind of E. [potential] depends upon the work which the forces of the system would do if the parts of the system were to yield to the action of those forces." That, of course, means *precisely the same* as the following, which, however, expresses the thing more directly. This kind of E. (potential) is the E. which the forces of the system possess in consequence of the possible displacements of the parts of the system under the action of those forces. Tait himself, both in his Glasgow lecture and in his "Recent Advances," tells us that a wound up spring or bent bow has potential E. Clerk Maxwell tells us the same. If so we have a right to speak of the energy of the gravitation attraction. In a certain respect the cases are different, but not so as to affect the present point.

This, our putting of the E. of position into the forces, instead of into the body or bodies, does not, of course, *explain* the action any more than the other does, but it gives a conception (provisional, if you like) which is much clearer and *in better analogy*, and, as we have said, free from all the above-recounted confusions. Moreover, the expression "E. of a force" has the great advantage of keeping before the mind of poor P. the fact that force and energy are not the same, a distinction which he is slow to apprehend, and which it is of the utmost importance to him that he should get proper hold of.

And now that we have got our E. of position into its most convenient seat, what shall we call it, and how shall we speak of its action? We cannot be dreadfully wrong if we call it by a name suggested by an expression of Helmholtz; let it be "*Energy of Tension.*" Does it not seem more logical to designate it by its essential characteristic than by what is only a condition though an indispensable one; for this latter we do when we call it E. of position or configuration. And as to its action let us say that when E. is being, as it is usually expressed, transformed from potential to actual E., or *vice versâ*, it is *transferred* from the forces to the bodies of the system, or *vice versâ*. If these expressions are unsuitable and erroneous, then let every one abstain from language which is precisely tantamount to them. But our doctors do not do this; and it fortifies us greatly in the belief that we are right to know that our doctors, when they are quite themselves, say the very same in substance, though not in so many words. On the other hand, if these expressions recommend themselves to us, let us use them boldly and consistently without mincing matters. Deschanel seems to have been on the point of using them in one place.[2] However, the fear of his *confrères* suddenly rose before his eyes, and having written (or his translator for him) the word "transferred," he stops short without telling us from what and to what the transference is made; he leaves us to complete for ourselves the sense of the passage, which clearly is that the transference is from the forces to the bodies, and *vice versâ*.

Poor Publius and myself have *several* other complaints to make; but probably we have said enough to excite the sympathy of all considerate persons.

Dublin X.

New Electric Lights

UNDER the above title Mr. Munro describes, in NATURE, vol. xvi. p. 422, M. Lodighin's device for an electric light. This is no novelty but a simple repetition of an invention made by Mr. Starr, a young American, and patented in this country under the title of "King's Patent Electric Light," specification enrolled March 25, 1846. An account of it, with drawings, may be found in the *Mechanic's Magazine*, April 25, 1846, p. 312. To this are appended some editorial remarks in which the novelty of the invention was at that date disputed. Those who care to follow the subject further may find a letter of mine replying to this editorial criticism in the *Mechanic's Magazine* of May 9, 1846, p. 348.

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[1] Clerk Maxwell's "Heat," p. 281 bottom; see Willson's "Dynamics," p. 147.

[2] *E.g.* Balfour Stewart, "Cons. of E.," p. 25 (but see his "Elem. Phys.," p. 106).

[3] Tait, "Recent Advances," p. 18; Willson, "Dynamics," p. 278.

[4] The inconsistency is startlingly exhibited in a single sentence for which two doctors are responsible, "Uns. Univ.," p 111, "the potential E of a raised weight or bent spring." If the potential E. is in either one of these it cannot be in the other. We have the same in a single sentence in Thomson and Tait, p. 178 (two doctors, again, responsible); also in Tait's Glasgow lecture.

[1] The only exception that I remember to have seen is afforded, curiously enough, by Rankine himself, the inventor of E *in posse*. In *Phil. Mag.*, February, 1853, he says, "E. of gravitation;" and in "Encycl. Brit.," vol. xiv., "Mechanics," he speaks of the E. of an effort.

[2] "Nat. Phil," p. 79

I constructed a large battery and otherwise assisted Mr. Starr in his experiments on this light. The "wick," as Mr. Munro aptly calls it, was a stick of gas retort carbon, like that pictured (NATURE, p. 423), excepting that it was affixed to supports of porcelain in order to remedy the fracture which occurred to our first apparatus in which the carbon stick was rigidly held in metallic forceps. Thus the improvement of M. Kosloff was also anticipated.

The lamp-glass was a thick barometer tube about thirty-six inches long, with its upper end blown out to form a large bulb or expanded chamber. The carbon and its connections were mounted in this with a platinum wire passing through and sealed into the upper closed and expanded end of the tube.

The whole of the tube was then filled with mercury and inverted in a reservoir, and thus the carbon stick, &c., were left in a Torricellian vacuum. The current was passed by connecting the electrodes of the battery with the mercury (into which a wire from the lower end of the carbon dipped) and with the upper platinum wire respectively. A beautiful steady light was produced accompanied with a very curious result which at the time we could not explain, viz., a fall of the mercury to about half its barometrical height and the formation within the tube of an atmosphere containing carbonic acid.

I have now little doubt that this was due to the combustion of some of the carbon by means of the oxygen occluded within itself.

In pointing out this anticipation of M. Lodighin's invention I do not assume or suppose that any piracy has been perpetrated. It is one of those repetitions of the same idea which are of such common occurrence and which cost the re-inventor and his friends a vast amount of trouble and expense that might be saved if they knew what had been done before.

I may add that the result of our battery experiments was to convince Mr. Starr that a magneto-electric arrangement should be used as the source of power in electric illumination; and that he died suddenly in Birmingham in 1846, while constructing a magnetic battery with a new armature which, theoretically, appeared a great improvement on those used at that date. Of its practical merits I am unable to speak.

Twickenham, September 18 W. MATTIEU WILLIAMS

Serpula Parallela

Two or three years ago I read somewhere that *Serpula parallela* of M'Coy is probably a vitreous sponge. Can any of your readers give me a reference for this? I wish to give the authority for this happy suggestion to which Mr. Young and I referred last year. JOHN YOUNG

Glasgow University, September 19

HYDROGRAPHIC SURVEY OF THE BALTIC

WE learn from the Stockholm *Nya Dagligt Allehanda* that during the month of July last a hydrographical survey of the Baltic was carried out by two vessels belonging to the Swedish navy, which were placed for this purpose at the disposal of the Swedish Royal Academy of Sciences for a month. A grant of about 550*l.* is intended to cover the expenses of three such expeditions. The whole of the Baltic, from a line drawn from Arendal to Jutland to the head of the Gulf of Bothnia and from the Swedish coast on the one side to the Finnish, Russian, German, Danish, and Norwegian on the other, was examined for temperature and salinity along thirty-four lines, measuring together more than 23,000 English miles, and including 200 stations. At every such station the temperature and salinity of the sea water were ascertained at the surface and at several different depths down to the bottom, about 1,800 different determinations of temperature having been made and a corresponding number of samples of water obtained. The nature of the bottom has also been ascertained by instruments which brought up samples not only from the surface of the bottom, but also from a variable depth, occasionally several feet, under it. The plan of this survey, which is said to be the most complete that has yet been made for its special objects, the determination of the salinity and temperature, was drawn up and carried out by Prof. F. L. Ekman. New instruments for taking samples of sea-water at different depths were employed, and as the temperature of the water did not undergo any perceptible alteration during the time required for getting it to the surface, for every sample that was obtained, the temperature of the depth from which it was raised was ascertained simultaneously, without any great loss of time. The survey shows the Baltic and the Gulf of Bothnia to consist of three strata, differing greatly in temperature, and often very sharply defined, viz., an upper stratum, which is warmed during the summer by the heat of the sun to a pretty high temperature, a lower, in which the cold of winter still prevailed to a great extent, and under the latter still another of a somewhat higher temperature than the intermediate stratum, the third stratum being of great thickness where the depth was considerable. In the Gulf of Bothnia, as in Skagerack and Kattegat, on the other hand, the temperature diminished steadily in proportion to the depth, as is commonly the case in the ocean. The uppermost summer-warm stratum of water was found to be of variable thickness at different places in the Baltic; at some it was scarcely perceptible at the period of observation. This and other peculiarities will probably be explained in the course of the working out of the observations which is now proceeding.

OUR ASTRONOMICAL COLUMN

THE SATURNIAN SATELLITE HYPERION.—The following ephemeris of this satellite for the next period of absence of moonlight is founded upon the elements calculated by Prof. Asaph Hall, of Washington, from his measures in 1875. Though limited to dates when Saturn may be observed while the moon is absent, probably her presence, except when very near the planet, is less an impediment to viewing so faint an object than the unavoidable proximity of the planet itself.

At 10h. Greenwich M.T.

		Pos.	Dist.			Pos.	Dist.
		°	″			°	″
Sept.	30	261·8	47·8	Oct.	6	277·4	219·5
Oct.	1	270·6	118·6	,,	7	278·8	177·4
,,	2	273·0	176·6	,,	8	281·2	130·8
,,	3	274·3	215·8	,,	9	286·7	76·5
,,	4	275·3	234·1	,,	10	321·0	22·1
,,	5	276·3	232·3				

An ephemeris of the five inner satellites of Saturn, by Mr. Marth, appears in No. 2,154 of the *Astronomische Nachrichten*. It is elaborately compiled, but this the first portion, extending to September 20, only reached this country on the date of its expiration. It is to be regretted that a work of this interest involving so much care and trouble in its preparation, should not have been in the hands of astronomers earlier; it is not the first instance of unfortunate delay in the publication of communications of immediate utility in this periodical of late.

THE NEW COMET (1877, IV.).—A first approximation to the orbit of the faint comet discovered at Marseilles on the 14th inst. calculated by Mr. Hind upon M. Coggia's observation on that date, and observations at Leipsic by

Prof. Bruhns, and Strasburg by Prof. Winnecke on the 17th and 18th respectively, gives the following elements:—

Perihelion Passage, September 6·4104 Greenwich M.T.

	°	′	″	
Longitude of the Perihelion ...	111	47	45	True equinox,
,, Ascending Node ...	251	45	50	Sept. 16.
Inclination to Ecliptic	78	35	58	
Log. Perihelion Distance	0 198282			

Motion—retrograde.

The middle observation is represented with errors of + 7″ in longitude and − 2″ in latitude.

A few computed positions are subjoined, but they are to be regarded only as rough ones:—

At 12h. G.M.T.	R.A.	N.P.D.	Distance from the earth.
	h. m. s.	° ′	
Sept. 26	8 21 52	45 0·9	1·655
,, 28	8 19 34	45 40·6	1·622
,, 30	8 17 4	46 24·4	1·588
Oct. 2	8 14 21	47 3·2	1·554
,, 4	8 11 24	47 43·2	1·520
,, 6	8 8 13	48 24·8	1·486

According to the above orbit the comet will remain visible for many weeks, approaching the earth, though receding from the sun, as it descends to the node; the intensity of light, however, is not likely to much exceed twice its actual amount. The elements do not present a striking similarity to those of any comet previously computed.

It is the fourth comet newly discovered in the present year, the others having been found (1) by M. Borrelly, February 8; (2) by Prof. Winnecke, April 5; and (3) by Mr. Swift on April 11. D'Arrest's comet of short period, which has been observed on its fourth return, is to be added to these, and as this comet is still in a position to be observed with our larger instruments a few places are subjoined:—

At Paris Noon.

	R.A.	N.P.D.	Distance from the earth.
	h. m. s.	° ′	
Sept. 29	4 51 2	89 56·4	1·425
Oct. 3	4 51 12	90 32·6	1·416
,, 7	4 50 44	91 8·5	1·409
,, 11	4 49 41	91 43·5	1·403
,, 15	4 48 2	92 17·3	1·398

FAMINES AND SHIPWRECKS

THE following letter from Dr. Balfour Stewart appeared in the *Times* of Saturday last:—

I have read with much interest your various articles on Dr. Hunter's researches into the Madras rainfall and the possible connection of famines with sun-spots, and I perceive from the letter in your columns of yesterday by Mr. Henry Jeula, of Lloyd's, that he has found most shipwrecks during periods of maximum solar activity.

I consider it a fortunate thing for science that the physics of the sun are now judged of sufficient importance to occupy the attention of the leading journal, inasmuch as the duty of the man of science is rather quietly to continue investigating than to endeavour to force prominently before the public the results of his work.

It has been recognised now for several years that in this particular case of shipwreck as in others the study of solar physics must ultimately lead to results of national importance. In illustration of this I may quote from a lecture delivered by Mr. Lockyer in October, 1872 ("Solar Physics," p. 423):—

"Mr. Meldrum, a distinguished meteorologist, who lives, not in the temperate zones of the earth, where the meteorological conditions are irregular, but in the torrid zone, where regular meteorological phenomena, and among them cyclones, abound, tells us that it is no longer correct to merely associate cyclones with the tropics. He tells us that the whole question of cyclones is a question of solar activity, and that if we write down in one column the number of cyclones in any given year and in another column the number of sunspots in any given year, there will be a strict relation between them—many sun-spots, many hurricanes; few sun-spots, few hurricanes. Only this morning I have received a letter from Dr. Stewart, who tells me that Mr. Meldrum has since found that what is true of the storms which devastate the Indian Ocean is true of the storms which devastate the West Indies; and, on referring to the storms of the Indian Ocean, Mr. Meldrum points out that at those years where we have been quietly mapping the sun-spot maxima the harbours were filled with wrecks, vessels coming in disabled from every part of the great Indian Ocean. Now that surely is something worth considering, because, if we can manage to get at these things, to associate them in some way with solar activity, so that there can be no mistake about it, the power of prediction—that power which would be the most useful one in meteorology, if we could only get at it—would be within our grasp."

I will, with your permission, make a few remarks on the present position of the problem and on the scientific policy for the future which this position naturally suggests. In the first place, what are the facts? Without the sun, the atmosphere of our earth would be as dead and inactive as a cotton mill without fire in its boiler. As in the case of such a mill, the work done will depend upon the strength of the furnace fire, so that if the fire be variable the work will vary with it. As regards our earth, we know, to begin with, of two distinct periods of meteorological variation. The one of these is due to the change of apparent position of the sun in the heavens caused by the earth's rotation, and its length is one day. The other is due to a similar change caused by the earth's revolution, and its length is one year. If the sun were intrinsically constant we should not be justified in looking for any other variation (unless we attribute some influence to the moon); but if the sun be variable in its power we are led to look for a corresponding variation in terrestrial activity. Now we know that the sun is a variable factor. At certain periods his disc is absolutely free from spots, while at others it is studded with these curious objects. On these last occasions we have other lines of evidence, which lead us to believe in the intense activity of the sun, or, in the forcible language once used by the late Sir J. Herschel to myself, the solar pot seems then to be boiling very rapidly. If we are to reason by analogy at all, the terrestrial pot must follow the solar one, and occurrences denoting a deficiency of energy, such as periodical famines, depending on local failures of vegetable food, will be grouped round years of deficient solar activity, while other occurrences, depending on great energy, such as cyclones and shipwrecks, will rather follow the periods of maximum solar activity. I have taken two instances (famines and cyclones), but I might have taken others of a more recondite nature, such as the phenomena of terrestrial magnetism, concerning which I believe I am justified in stating that those who work at this branch of science are satisfied that a decided effect has been traced to the variability of the sun as a cause.

It is quite right that men of science who are pursuing other branches of inquiry, that statesmen who have to utilise the teachings of science for the benefit of nations, and, permit me to add, editors of powerful journals like your own, should wish that the proof might be of a more absolutely convincing kind than that which has satisfied the workers that they are on the right way. At present the problem has not been pursued on a sufficiently large scale or in a sufficient number of places. In fine, if the attack is to be continued, the skirmishers should give way to heavy guns, and these should be brought to bear without delay now that the point of attack is known.

There are, of course, two ways of treating the problem. The evidence may be pronounced insufficient and the workers stopped, when the research will gradually die out from want of support. On the other hand, if, as you, Sir, admit, we are on a track of a discovery which will in time enable us to foretell the cycle of droughts, public opinion should demand that the investigation be prosecuted with redoubled vigour and under better conditions.

My object in writing this letter is to express a hope that your powerful influence may not be wanting in furtherance of a branch of inquiry from which I, as an individual worker in it, am undoubtedly of opinion that the greatest national benefit must in time arise. If forewarned be forearmed, then such a research will ultimately conduce to the saving of life both at times of maximum and minimum sun-spot frequency.

Owens College, Manchester, September 20

PROF. ADAMS ON LEVERRIER'S PLANETARY THEORIES

THE following admirable statement concerning Leverrier's more recent work was made on the occasion of the presentation of the gold medal of the Astronomical Society, in February, last year, to him by Prof. J. C. Adams, the president. It will be read with a mournful interest at the present time :—

It is not many years since our medal was awarded to M. Leverrier for his theories and tables of the four planets nearest the sun, viz., Mercury, Venus, the Earth, and Mars. Long before this he had been occupied with the larger planets, but before proceeding further with their theories he found it necessary to establish on solid foundations the theory of the motion of the earth, on which all the rest depend, and this again naturally led him to investigate the theories of the three nearer planets which, with the earth, constitute the inferior portion of the planetary system.

By the comparison of these theories with observations, M. Leverrier was led to two interesting results. He found that in order to bring the theories of Mercury and Mars into accordance with observation, it was necessary and sufficient to increase the secular motion of the perihelion of Mercury, and also the secular motion of the perihelion of Mars.

Hence M. Leverrier inferred that there existed on the one hand, in the neighbourhood of Mercury, and on the other, in the neighbourhood of Mars, sensible quantities of matter the action of which had not been taken into account.

This conclusion has been verified with respect to Mars. The matter which had not been considered, turns out to belong to the earth itself, the mass of which had been taken too small, having been derived from too small a value of the solar parallax. A similar increase of the mass of the earth is indicated by the theory of Venus, and a corresponding increase of the solar parallax is likewise derived from the lunar equation in the motion of the sun.

With respect to Mercury, a similar verification has not yet taken place, but the theory of the planet has been established with so much care, and the transits of the planet across the sun furnish such accurate observations, as to leave no doubt of the reality of the phenomenon in question; and the only way of accounting for it appears to be to suppose, with M. Leverrier, the existence of several minute planets, or of a certain quantity of diffused matter circulating about the sun within the orbit of Mercury.

The results which M. Leverrier had thus obtained from his researches on the motions of the inferior planets added to the interest with which he now entered upon similar researches on the system of the four great planets which are the most distant from the sun. Such researches might furnish information respecting matter hitherto unknown existing in the neighbourhood of these planets. Possibly they might afford indications of the existence of a planet beyond Neptune, and at any rate they would provide materials which would facilitate future discoveries.

As I shall have occasion to explain later on, the theories of the mutual disturbances of the larger planets are far longer and more complicated than those of the smaller, so that all that M. Leverrier had yet done might be almost regarded as merely a prelude to what still remained to be done. Increased difficulties, however, far from deterring, seemed rather to stimulate him to greater exertions.

On May 20, 1872, M. Leverrier presented to [the Academy an elaborate memoir, containing the first part of his researches on the theories of the four superior planets, Jupiter, Saturn, Uranus, and Neptune. This memoir contains an investigation of the disturbances which each of these planets suffers from the action of the remaining three. Throughout this investigation the development of the disturbing function, as well as that of the inequalities of the elements is given in an algebraical form, in which everything which varies with the time is represented by a general symbol, so that the expressions obtained hold good for any time whatever. Thus the eccentricities and inclinations, the longitudes of the perihelion and of the nodes are all left in the condition of variables. The mean parts of the major axes, which suffer no secular variations, are alone treated as given numbers.

At the end of the *résumé* of the contents of this memoir, given in the *Comptes Rendus*, M. Leverrier lays down the following almost appalling programme of the work still remaining to be done.

It would be necessary, he says,—

1. To calculate the formulæ, and to reduce them into provisional tables.
2. To collect all the exact observations of the four planets, and to discuss them afresh, in order to refer their positions to one and the same system of co-ordinates.
3. By means of the provisional tables, to calculate the apparent positions of the planets for the epochs of the observations.
4. To compare the observed with the calculated positions, to deduce the corrections of the elliptic elements of the four planets, and to examine whether the agreement is then perfect.
5. In the contrary case, to find the causes of the discrepancy between theory and observation.

Extensive as is this programme, it has already been completely carried out as regards the planets Jupiter and Saturn, and partly so as regards Uranus and Neptune.

Having received from the Academy the most effectual encouragement to pursue his researches, M. Leverrier lost no time in bringing them gradually to completion, so that they might become available for practical use.

Accordingly, on August 26, 1872, he presented to the Academy a memoir containing a complete determination of the mutual disturbances of Jupiter and Saturn, and thus serving as a base for the theories of both these planets, which are closely connected with each other.

Again, on November 11, 1872, he presented his determination of the secular variations of the elements of the orbits of the four planets, Jupiter, Saturn, Uranus, and Neptune. These variations are mutually dependent on each other, and must be treated simultaneously. Their determination consequently involves the solution of sixteen differential equations, which are very complicated in form, and can only be integrated by repeated approximation.

This part of the work forms a necessary preliminary to the treatment of the theory of any one of these planets in particular.

On March 17, 1873, M. Leverrier presented to the Academy the complete theory of Jupiter; and on July 14 in the same year he followed it up by the complete theory of Saturn.

On January 12, 1874, he presented his tables of Jupiter, founded on the theory which has just been mentioned, as compared with observations made at Greenwich from 1750 to 1830, and from 1836 to 1869, and with observations made at Paris from 1837 to 1867.

Again, on November 9, 1874, he presented to the Academy a complete theory of Uranus. Already in 1846, in his researches which led to the discovery of Neptune, M. Leverrier had given a very full investigation of the perturbations of Uranus by the action of Jupiter and Saturn. In the memoir just mentioned he gives a fresh investigation, including a full treatment of the perturbations of Uranus by the action of Neptune.

On December, 14, 1874, he presented a new theory of the planet Neptune, thus completing the theoretical part of the immense labours which he had undertaken with respect to the planetary system.

Finally, on August 23, 1875, he presented to the Academy the comparison of the theory of Saturn with observations.

Such is a bare enumeration of the various labours for which our science is already indebted to our illustrious Associate.

That any one man should have had the power and perseverance required thus to traverse the entire solar system with a firm step, and to determine with the utmost accuracy the mutual disturbances of all the primary planets which appear to have any sensible influence on each other's motions, might well have appeared incredible if we had not seen it actually accomplished.

I will now proceed to give a brief outline of the investigations relating to the motions of the four larger planets, with which we are now more particularly concerned. The most important parts of these investigations are printed in full detail in the volumes of *Memoirs* which form part of the *Annals of the Observatory of Paris.*

As in his former researches, M. Leverrier here also exclusively employs the method of variation of elements, and the investigations are based on the development of the disturbing function given by him, in the first volume of the *Annals of the Paris Observatory*, with greater accuracy and to a far greater extent than had ever been done before.

The eighteenth chapter of M. Leverrier's researches, which forms nearly the whole of the tenth volume of the *Memoirs*, is devoted to the determination of the mutual action of Jupiter and Saturn, which forms the foundation of the theories of these two planets.

These theories are extremely complicated, and I shall endeavour briefly to point out, and to explain as far as I can without the introduction of algebraical symbols, the nature of the peculiar difficulties which M. Leverrier has had to encounter in their treatment, and which he has so successfully overcome. These difficulties either do not present themselves at all, or do so in a very minor degree in the theories of the smaller planets.

First, then, the masses of Jupiter and Saturn are far larger than those of the inferior planets, the mass of Jupiter being more than 300 times and that of Saturn being nearly 100 times greater than the mass of the earth. For this reason it is necessary to develop the infinite series in which the perturbations are expressed to a much greater extent when we are dealing with Jupiter and Saturn than when we are concerned with the mutual disturbances of the inferior planets. Also Jupiter and Saturn are so far removed from these latter planets that the disturbances which they produce in the motion of these planets are extremely small, in spite of the large masses of the disturbing bodies.

But the great magnitude of the disturbing masses is far from being the only reason why the theory of the mutual disturbances of Jupiter and Saturn is so complicated.

Another cause which aggravates the effect of the former is the near approach to commensurability in the mean motions.

Twice the mean motion of Jupiter differs very little from five times that of Saturn. In other words, five periods of Jupiter occupy nearly the same time as two of Saturn, so that if at a given time the planets were in conjunction at certain points in their orbits, then after three synodic periods they would be again in conjunction at points not far removed from their positions at starting. Hence, whatever uncompensated perturbations may have been produced in the motions of the two planets during these three synodic periods will be very nearly repeated in the next three synodic periods, and again in the next three, and so on.

Hence the disturbances will go on accumulating in the same direction during many revolutions of the two planets, and will become very important. The inequalities of long period thus arising will affect all the elements of the orbits of the two planets; but the most important are those which affect the mean longitudes of the bodies, since these are proportional to the square of the period of the inequalities, whereas the inequalities affecting the other elements are proportional to the period itself.

The principal terms of the inequalities of mean longitude are of the third order, if we consider the excentricities of the orbits and their mutual inclination to be small quantities of the first order.

Terms of the same period, however, and those far more numerous and more complicated in expression, occur among those of the fifth and of the seventh order of small quantities, and M. Leverrier has included these terms also in his approximations.

But the circumstance which contributes in the highest degree to cause the superior complexity of the theories of the larger planets is the necessity, in their case, of taking into account the terms which depend on the squares and higher powers of the disturbing forces.

I will endeavour to point out the nature of these terms and the manner in which they arise.

By the theory of the variation of elements we are able to express at any given time the rate of variation of any one of the elements in terms of the mean longitudes and the elements of the orbits of the disturbed and the several disturbing bodies. If this rate of variation were given in terms of the time and known quantities, we should at once find the value of the element for any given time by a simple integration. But this is not the case.

The method of variation of elements gives us not a solution, but merely a transformation of our original differential equations of motion. The rates of variation are given in terms of the unknown elements themselves; and in order to find the elements from the equations so formed, we must employ repeated approximations.

Let us consider this matter a little more particularly.

The terms which express the rate of variation of any element may be divided into two classes—

1. Those which involve the mean longitudes of one or both of the planets concerned, as well as the elements of their orbits.

2. Those which involve the elements only.

The first are called periodic terms, since they pass from positive to negative, and *vice versâ*, in periods comparable with those of the planets themselves. The second are called secular terms, and vary very slowly, since the elements on which they depend do so. Each of the terms in the expression of the rate of variation of any element will involve the mass of one of the disturbing bodies as a factor. Hence, if all these masses be very small, all the periodic inequalities of the elements will be likewise very small, and we shall obtain a value of the rate of variation which is very near the truth if we substitute for the complete value of any element its value when cleared of periodic inequalities. Then the periodic inequalities in the element under consideration may be found by direct integration, supposing the elements to be constant in the terms to be integrated, and the mean longitudes only to vary. Also the secular variation of the element considered, that is, the rate of variation of the element when cleared of periodic inequalities, will be given by the secular terms taken alone. If the disturbing masses, however, are not very small, this process is not sufficiently accurate, and the periodic inequalities thus found can only be regarded as a first approximation to the true values. In order to find more correct values, we must substitute for the elements in the second member of the equation their secular parts augmented by the approximate periodic inequalities before found.

Now, if in any periodic term we increase any element by a periodic inequality depending on a different argument, that is, involving different multiples of the mean longitudes, the result will evidently be to introduce new periodic terms which will involve the square of one of the masses or the product of two of them as a factor. Similarly, if in any periodic term any element be increased by a periodic inequality depending on the same argument, the result will also introduce new terms of the second order which do not involve the mean longitudes, and which therefore constitute new secular terms. These will be particularly important if the inequality in question be one of long period. Also in the secular terms the result of increasing any element by a periodic inequality will be to introduce a new periodic term depending on the same argument. Lastly, it should be remarked that in finding the periodic inequalities of any element by integration of the corresponding differential equation, we must take into account the secular variations of the elements which were neglected in the first approximation. The new terms thus introduced, like the others which we have just described, will evidently be of the second order with respect to the masses.

If the disturbing masses be large, as in the case of the mutual disturbances of Jupiter and Saturn, it may be necessary to proceed to a further approximation, and thus to obtain new terms, both periodic and secular, which involve the cubes and products of three dimensions of the masses. The number of combinations of terms which give rise to these terms of the second and third orders is practically unlimited, and the art of the calculator consists in selecting those combinations only which lead to sensible results. This is the chief cause of the great complexity of the theories of the larger planets, and more especially of those of Jupiter and Saturn.

M. Leverrier lays it down as the indispensable condition of all progress that we should be able to compare the whole of the observations of a planet with one and the same theory, however great may be the length of time over which the observations extend. In order to satisfy this condition, he develops the whole of his formulæ algebraically, leaving in a general symbolical form all the elements which vary with the time, such as the excentricities, the inclinations, and the longitudes of the perihelia and nodes. He treats in the same way the masses which are not yet sufficiently known.

All the work is given in full detail, and is divided as far as possible into parts independent of each other, so that any part may be readily verified. All the terms which are taken into account are clearly defined, so that if it should ever be necessary to carry on the approximations still further, it will be easy to do so without having to begin the investigation afresh. The whole work is presented with such clearness and method as to make it an admirable model for all similar researches.

After the development of the disturbing functions, and the formation of the differential equations on which the variations of the elements depend, the first step to be taken is to determine by integration of these equations the periodic inequalities of the elements of the orbits of Jupiter and Saturn which are of the first order with respect to the masses. As we have already said, the expressions of these periodic variations of the elements are given with such generality that, in order to obtain their numerical values at any epoch whatever, it is sufficient to substitute the secular values of the elements at that epoch. The calculation of the various terms under this general form is very laborious, and it requires great and sustained attention in order to avoid any error or omission of importance. On the other hand, by substituting from the beginning the numerical values of the elements at a given epoch, the calculation is rendered much shorter and admits much more readily of verification; but the result thus obtained only holds good for the given epoch, and is thus entirely wanting in generality.

In the determination of the long inequalities of Jupiter and Saturn, the approximation is carried to terms which are of the seventh degree with respect to the excentricities and the mutual inclination of the orbits. In the next place the terms of the first order in the secular variations of the elements of the orbits are determined. After this the periodic inequalities of the second order with respect to the masses are considered. These are determined in the same form as the terms of the first order, in order that their expressions may hold good for any epoch whatever. The formulæ relating to these terms are necessarily very complicated. The coefficient belonging to a given argument depends, in general, on a great number of terms which are classed methodically. Next are determined the terms of the second order in the secular variations of the elements of the orbits. Afterwards, M. Leverrier takes into account the influence of the secular inequalities on the values of the integrals on which the periodic inequalities depend. The last part of this chapter is devoted to the completion of the differential expressions of the secular inequalities by the determination of certain secular terms in the rates of variation of the excentricities and the longitudes of the perihelia, which are of the third and fourth orders with respect to the masses.

(*To be continued.*)

NOTES

We record with sincere regret the death of Prof. Alphonse Oppenheim, at Hastings, on the 17th inst.; he died by his own hand through grief at the death of his wife. Prof. Oppenheim is well known for his numerous researches in organic chemistry. Formerly one of the professors of chemistry at the University of Berlin, he only a few months ago, as we recorded at the time, had accepted the chair of chemistry at the University of Münster, in Westphalia. Prof. Oppenheim was a frequent contributor to this journal, and was much esteemed by a large circle of friends in England.

The death is announced, on the 17th inst., at the age of seventy-seven, of Mr. W. H. Fox Talbot, F.R.S., the inventor of the photographic process known as Talbotype, a name latterly merged in the general name photography. Mr. Talbot was a man of varied attainments and manifold work. He was educated at Harrow and Cambridge, where he distinguished himself as a Greek scholar. He took a delight in chemistry, and it was in 1833 that he seems to have conceived the idea of inventing some process by which the beautiful pictures exhibited in a camera lucida could be impressed and rendered permanent. He and Daguerre seem to have brought their several processes to a satisfactory result almost simultaneously, though Daguerre was the first to announce his process, in 1839. Mr. Talbot lost no time in communicating to the Royal Society the details of his own process, though it was not till 1840 that he made the discovery which "laid the foundation of the photographic art in its present form." In 1842 Mr. Talbot was presented with the gold medal of the Royal Society. He did not patent his discovery, but on account of its great value and many uses, freely gave the benefit of it to the public. In subsequent years Mr. Talbot published various modifications and applications of his process, but latterly he had turned his attention to quite a different field, publishing various works on antiquarian, classical, and linguistic subjects.

WE have received from our correspondent a full report of the meeting of the German Association at Munich, with important addresses by Prof. Haeckel, "On the Evolution Theory at the Present Day," and by Prof. Nägeli, on the "Limits of Natural Knowledge." Pressure on our space compels us to defer this report till next week.

PROF. J. E. HILGARD, assistant in charge of the United States Coast Survey, has been offered the directorship of the new International Bureau of Weights and Measures in Paris. Prof. Hilgard is one of the excursion party which includes Sir J. D. Hooker, Prof. Asa Gray, and Dr. Hayden.

THE second biennial meeting of the International Congress of Americanists for the discussion of all matters relating to American archæology, philology, ethnology, and pre-Columbian antiquities generally, was held at Luxemburg on September 10–13. There was a numerous attendance of delegates from all parts of the world. Many papers were contributed to the Congress. Dr. Leemans, Prof. Leon de Rosny, Abbé Pipart, and M. Madier de Montjan, read papers upon primitive American civilisation, and especially picture-writing and hieroglyphics. Several Americans sent communications relative to the mound-builders of the Mississippi valley and the Pueblo Indians of New Mexico; amongst these Messrs. Gillman, of Detroit, Michigan (so well known for his discoveries amongst the burial mounds there), Force, of Cincinnati, and Moody, of Illinois, may be specially mentioned. Dr. Rink contributed a valuable paper upon the primitive habitat of the Esquimaux, maintaining, in opposition to the usual belief, that they came from the interior of America. Messrs. Hyde Clarke and F. A. Allen, of London, contributed essays upon the wider aspect of the question, seeking to trace the civilisation of the primitive races of the New World to a fountain-head in Asia. A valuable paper from M. Lucien Adam furnished a detailed analysis of the grammar of sixteen Indian nations ranging from Lake Athabasca to the Llanos of Brazil. It was resolved by the Congress to memorialise the South American Governments to take steps to preserve authentic records of the language and customs of all small Indian tribes likely soon to become extinct. Throughout the Congress great interest was manifested by the inhabitants in the important subjects discussed, and the visitors were most hospitably entertained by the burgomaster and municipality at a final banquet on Thursday, the 13th, upon which occasion congratulatory telegrams were received from the King of the Netherlands and from Prince Henry, the Governor of the Grand Duchy. It has been decided to hold the third Congress in 1879, at Brussels, when it is hoped the attractiveness and convenience of the locality will induce a larger attendance. The proceedings will be published as soon as possible, and are expected to fill three volumes of 600 pages each.

THE value of the work accomplished by Mr. Stanley, who reports himself on August 10 from Emboma, near the mouth of the Congo, will be universally acknowledged; and there can be but one opinion as to the rank he will hold among geographical explorers. He has solved one of the few great geographical problems which remained for solution, and has performed a feat which baffled even Livingstone's patient genius. Both Livingstone and Cameron had to turn away from Nyangwe on the Lualaba, in Manyuema, foiled in their desire to descend the mysterious river; had Stanley been equally scrupulous no doubt he also would have had to submit to defeat. Determined, however, to trace the course of the river or meet with the fate of Park, he tells us that partly by marching along the banks, partly by sailing down the river, he traced the course of the Lualaba, changing its name "scores of times," almost direct north from Nyangwe to 2° N. lat., where it turns north-west, then west, then south-west, until as it approaches the Atlantic coast it becomes known as the Kwango or Zaire. Many cataracts had to be passed, and at one of the last of them the remaining one of the Pocock brothers was drowned. The breadth of the stream, Mr. Stanley states, varies from two to ten miles, and in some parts is choked with islands. If we consider Webb's Lualaba as the main stream, then its origin must be regarded as the Chambeze rising to the west of Lake Nyassa, and under many names flowing thence through Lake Bangweola, northwards through Lake Moero, Kowamba, and the reported Kamolondo by Nyangwe to at least 2° N. lat., and thence south-west to the Atlantic Ocean—a course with all its windings, not far short of 3,000 miles. Its basin will thus be included between 32° E. and the west coast of Africa, and 12° S. and 2° N. lat. Its affluents are many, some of them very large. There is the Western Lualaba with its many tributaries, probably the Casai, also with numerous affluents, and very possibly even the Ogovai may be an offshoot from the lower Congo. Between 26° and 17° E. the river has an uninterrupted course, descending thence by about thirty falls and rapids to the great river between the falls of Yellala and the Atlantic. Livingstone heard of a large lake with many islands many miles to the north of Nyangwe, and this may simply be one of the ten-mile wide stretches referred to by Mr. Stanley. Further details will be anxiously looked for, but with our present information we must regard the Congo as one of the largest and most important rivers on the globe. It seems clear that Livingstone was mistaken in connecting the Lualaba with the Nile system. The conduct of Mr. Stanley's expedition it is not our business to criticise; but it seems clear that unless we were prepared to wait for an indefinite period the solution of this important problem and the opening up of undiscovered Africa to commerce and science and civilisation, some pioneer must sooner or later have forced his way through the tribes along the route taken by Stanley. This addition to knowledge has been achieved with much suffering and loss of life, though it seems probable that the many "battles" reported to have been fought may turn out to have been exaggerated in their details. Mr. Stanley was to proceed from Emboma to Cabinda, and thence to St. Paul de Loanda, so that we may soon expect to be able to welcome him home.

IN speaking of the famine in Madras the *Times* Madras correspondent, under date August 29, writes as follows:—"I have not seen Mr. Pogson, the Government astronomer, very lately, but I am informed that he has indicated to the Government the probability of the coming north-east monsoon being a failure also, as the intensity of the solar heat continues unabated. If this be so it is quite impossible to say what the subsequent months will bring forth. The possibility of a great catastrophe such as the failure of seasonal rains at the end of two seasons of scarcity and famine is too horrible to contemplate; but it is in accordance with the history of former famines and the conclusions of scientific men, that rainy seasons in the tropics should be abnormal under the influence of the intense solar heat and the absence of 'spots' on the sun." It is a pity that positive statements like this should be published without reference to any data on which they are based. Had observations on the monsoons been carefully made, tabulated, and worked out for many years past, it would be possible to predict with something like certainty the character of the coming monsoon.

DR. MATTHEWS DUNCAN, of Edinburgh, is to succeed Dr. Greenhalgh at St. Bartholomew's.

DR. WILLIAM STIRLING has been appointed to the chair of physiology in the University of Aberdeen.

THE *American Journal of Pure and Applied Mathematics*, the New York *Nation* states, will appear quarterly, beginning with January, 1878. The form will be quarto, and 384 pages will constitute a volume. The associate-editor in charge is Dr. W. E. Story, Johns Hopkins University, Baltimore.

AT the Social Science Congress which has been meeting in Aberdeen during the past week, there were very few papers of strictly scientific interest. Among papers in the Educational Section was one by Prof. Bain on Competitive Examination for Public Appointments. In their choice of subjects the Civil Service Commissioners had, he remarked, been guided by the received branches of education in the college and schools, but after an inquiry into the essential nature of the subjects, he arrived at the conclusion that the sciences and not the languages were the proper subjects for competition. Other languages than our own were only of secondary utility. He expressed surprise at our intense conservatism in the matter of languages. There were according to him three great regions of study that should be fairly represented by every successful candidate—first, the sciences as a whole; secondly, English composition; and thirdly, institutions and history, with perhaps literature. These he would fix as a minimum. Sir Alexander Grant, principal of the Edinburgh University, read a paper on the Best Means of Securing a High Standard of Education. He considered a revision of the code, in order to remove the inequality in which classics and mathematics stood in relation to science in the "specific subjects," and a reconstruction of the normal school system to be necessary. Dr. Brown, of Haddington, read a paper in which he advocated the establishment of schools of forestry in Great Britain, in view of the fact that all candidates for admission to the department of the Indian Civil Service which had to deal with this matter, had to pass an examination which they at present could only qualify themselves for by going to France or Germany for the instruction. Something of this kind was being attempted in connection with the botanic gardens of the Edinburgh University, where ground had now been acquired for an arboretum.

SOME of our readers may like to know that, as might have been expected, the three rhinoceroses now exhibited in the Alexandra Park are specimens of the African Black Rhinoceros (*Rhinoceros bicornis*). This species is extremely uncommon in menageries, and we have heard of no other in this country except the fine adult male now living in the Zoological Society's Gardens in Regent's Park. The three specimens above referred to are all young, a pair being about eighteen months old, and the other a male not more than a year old. In the larger specimens the posterior horn is much smaller than that upon the nose, whilst in the young male its existence is only indicated by a slight rugosity. The late development of the posterior horn is of particular interest, as it shows that the growth of this dermal appendage is a secondary phenomenon, which makes it not surprising that there may be causes which result in it attaining a greater size than usual, as it does in the so-called distinct species, *R. keitloa*, in which the only characterising feature is its large posterior horn.

IT is perhaps a fortunate thing that our great politicians, like the Chancellor of the Exchequer and Mr. John Bright, are beginning to concern themselves in their public addresses with science as well as art. With reference to Mr. Bright's recent address, as the *Times* remarks, if his hearers complain that they have not been told much about either science or art, we can only say that we agree with them, and that we deplore our common loss. In the coming time it is to be hoped that public speakers, like Mr. Bright, will know better what science really is than they seem to do now.

IT is stated that the Italian Government has authorised two officers of the Royal Navy to take part in the Polar expedition which the Swedish Government is fitting out.

THE *Gaulois* states that M. Duruof, the balloonist, has been engaged by the Russian Government to organise an aëronautical service for the Danube army.

THE last field meeting of the Woolhope Naturalists' Field Club for the year will be held at Hereford, for a foray among the funguses, on Thursday, October 4. M. Maxime Cornu, of Paris, is expected to be present. An exhibition of funguses, apples, and pears will be held in the museum room at the Free Library. The fungus foray will be made on the Whitfield Lawns, by the kind permission of the Rev. Archer Clive. Carriages will leave the Free Library at 10 A.M., to return there by 3.30. A meeting of the members will be held on the return, in the Woolhope Room, for the election of officers for the ensuing year, and for the transaction of the ordinary business of the club. After dinner, or in the course of the evening, the following among other papers will be given:—A Report on the Progress of Mycology during the Year, by Dr. Bull; a Report on the Progress of "The Herefordshire Pomona," by the Rev. C. H. Bulmer; "On a Fossil Fungus (*Pythium*) with Zoospores *in situ*, belonging to the Palæozoic Epoch," by Worthington G. Smith, F.L.S.; and if time permit, a paper "On the Mosses of Herefordshire," by the Rev. Augustin Ley.

AT a meeting of the Linnean Society of New South Wales, on March 26, 1877, Mr. E. P. Ramsay read a "Note of a Species of Echidna (*Tachyglossus*) from Port Moresby, New Guinea," in which he described a fine and apparently full-grown male Echidna from that locality, applying to it the specific name *lawesi*, after its discoverer, Mr. Lawes, who had given the specimen to the Museum at Sydney. Mr. Ramsay's description has been published in the *Proceedings* of the above-named Society, and is accompanied by a plate representing the head and fore-part of the animal and one of the hind feet, of the natural size. Unfortunately no diagnosis is given whereby the differences between this New Guinean form and the two long-known species of Australia and Tasmania are made plain; but as that gentleman is doubtless familiar with both of them, we may take his word for it that *Tachyglossus lawesi* is a good and distinct species. Its distinctness from the other New Guinean form, *T. bruijni*, is manifest.

IT has been proposed by a correspondent of the *New York Tribune* to give the names of Romulus and Remus to the two satellites of Mars.

WE understand that the Council of the Working Men's College, Great Ormond Street, have arranged for the ensuing session a series of lectures in connection with the Science and Art Department upon Human Physiology. The lectures will be delivered on Friday evenings by Mr. Thomas Dunman, and will commence on October 5.

AT the meeting of the Birmingham Natural History Society on the 18th inst. Mr. W. R. Hughes, F.L.S., gave some account of the recent dredging excursion of the Society to Arran. He described how the idea of such an excursion took shape, and gave an interesting account of the numerous finds of the party, mainly in Lamlash Bay, where, of course, it was not to be expected that anything new was to be found. Still, many of the forms obtained were of great interest, and the members present gained much solid instruction by being able to examine specimens fresh from their native habitat. Other societies would do well to imitate this enterprising Birmingham association; indeed it might not be a bad idea for several societies to club together and carry out a similar excursion on a more extended scale. Dr. Marshall described the echinoderms, molluscs, annelids, and crustaceans taken.

PROF. PALMIERI has noted for the present year great anomalies of temperature. The degree of heat observed at the Vesuvius Observatory is unprecedented, having reached 34° C., and the mercury has fallen as low as − 7° C. This low temperature has never been reached once before, even in January and February, in the twenty-five years during which the observatory has been established.

THE Emperor of Brazil has formed a commission charged with the determination of geographical positions in the empire, and the first work of this commission is just published. It contains an account of the determination of the longitude and latitude of Barra de Pirahy. Geodesic operations are continued for localities situated on the prolongation of the Santos railway, and also on the parallel (10° in length) destined to join Rio to the great meridian of the empire, which will be measured by the commission.

Die Natur of September 17 contains an interesting collection of some of the myths and stories which constitute the folk-lore of the Australian aborigines.

IN the Anthropological Section of the Havre meeting of the French Association M. Gustave Lagneau exhibited an ethnographic map of France, on which he has attempted to indicate, in accordance with historical and ethnographical data, the division, juxtaposition, superposition and mixture of the various ethnical elements which have contributed to the formation of the present population of the country.

OF the many natural history societies in the United States but one, so far as is known, is composed almost entirely of Germans, the proceedings of which are published in the German language. This is the Naturhistorisches Verein, of Milwaukee, Wisconsin, of which the annual report for 1876–77 has just been published. This society is organised in five sections—zoology, botany, mineralogy, geology, and ethnology—holds regular meetings, and has quite a large active membership.

A GENERAL inventory has been taken by the French ministry of all the public libraries of France. More than 200 towns have been found to possess each a library numbering from 10,000 to 20,000 volumes.

A SWEDISH paper just received publishes an interesting article under the heading, "Why is the Climate of Europe growing Colder?" The article states that in the Bay of Komenok, near Koma, in Greenland, fossil and very characteristic remains of palm and other trees have been discovered lately, which tend to show that in these parts formerly a rich vegetation must have existed. But the ice period of geologists arrived, and, as a consequence of the decreasing temperature, this fine vegetation was covered with ice and snow. This sinking in the temperature, which moved in a southerly direction, as can be proved by geological data, *i.e.*, the discovery of fossil plants of certain species, seems to be going on in our days also. During the last few years the ice has increased far towards the south; thus between Greenland and the Arctic Sea colossal masses of ice have accumulated. On European coasts navigators now frequently find ice in latitudes where it never existed before during the summer months, and the cold reigning upon the Scandinavian peninsula this summer results from the masses of ice which are floating in the region where the Gulf Stream bends towards our coasts. This is a repetition of the observations made in the cold summer of 1865. The unaccustomed vicinity of these masses of ice has rendered the climate of Iceland so cold that corn no longer ripens there, and the Icelanders, in fear of a coming famine and icy climate, begin to found a new home in North America.

PROF. NORDENSKJÖLD's voyages seem to have been of service in opening up a sea-route to Siberia for commerce. A vessel belonging to M. Sidoroff, Capt. Schwanenberg, arrived at Vardö on September 16, after a passage of twenty-one days from the mouth of the Yenisei; and the steamer *Trazer*, belonging to M. Sibiriakoff, Capt. Dahlmann, which sailed from Bremen on July 28 for the mouth of the Yenisei, returned to Hammerfest on September 24.

A FIRE in Washington has destroyed the greater part of the Patent Office Museum, with thousands of patent models, many of great value.

WE notice among Messrs. Churchill's announcements for the forthcoming season: "A Handbook of Analysis of Water, Air, and Food, for the Medical Officer of Health," by Cornelius B. Fox, M.D., M.R.C.P., Medical Officer of Health for Central, East and South Essex; "Parasites: an Introduction to the Study of the Entozoa of Man and Animals, including some Account of the Ectozoa," by T. Spencer Cobbold, M.D., F.R.S., F.L.S., Professor of Helminthology in the Royal Veterinary College; and a "Student's Guide to the Anatomy of the Joints," by Henry Morris, M.A., M.B., F.R.C.S., Assistant-Surgeon to and Lecturer on Anatomy at the Middlesex Hospital.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseoviridis*) from West Africa, a Nisnas Monkey (*Cercopithecus pyrrhonotus*) from Nubia, presented by Mr. W. D. James; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. W. W. Stead; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. J. F. Greenwood; a Capybara (*Hydrochœrus capybara*) from South America, presented by Mr. W. Smith; a Peregrine Falcon (*Falco peregrinus*), European, an African Buzzard (*Buteo tachardus*) from Africa, presented by the Rev. W. Willimott; a West African Python (*Python sebæ*), a Royal Python (*Python regius*) from West Africa, presented by Mr. J. J. Kendall; a Goffin's Cockatoo (*Cacatua goffini*) from the Fiji Isles, an Ariel Toucan (*Ramphastos ariel*), a Maximilian's Aracari (*Pteroglossus wiedi*), two Blue-bearded Jays (*Cyanocorax cyanopogon*), two West Indian Rails (*Aramides cayennensis*) from South America, deposited; two Upland Geese (*Bernicla magellanica*) from the Straits of Magellan, three Andean Geese (*Bernicla melanoptera*), two Slaty Coots (*Fulica ardesiaca*) from Peru, purchased; an Axis Deer (*Cervus axis*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), born in the Gardens.

THE DIRECT PROCESS IN THE PRODUCTION OF IRON AND STEEL[1]

IN mixing comparatively rich iron ore in powder, with about twenty-five per cent. of its weight of pounded coal, and in exposing this mixture for some hours to the heat of a common stove or of a smith's fire, metallic iron is formed, which, on being heated to the welding point, on the same smith's hearth, may be forged into a horse-shoe of excellent quality. The admixture with the ore of some fluxing materials, such as lime or clay, will, in most cases, be of advantage to rid the iron of adherent slag.

The simplicity of this process is such that it naturally preceded the elaborate processes now in use for the production of iron and steel upon a gigantic scale, nor can it surprise us to find that attempts have been made from time to time down to the present day, to revert to the ancient and more simple method. It can be shown that iron produced by direct process is almost chemically pure, although the ores and reducing agent employed may have contained a considerable percentage of phosphorus, sulphur, and silicon, and that, if freed from its adherent slag, it furnishes a material superior in quality and commercial value to the ordinary iron of commerce.

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[1] Some Further Remarks regarding the Production of Iron and Steel by Direct Process. Paper read at the Newcastle Meeting of the Iron and Steel Institute, by C. William Siemens, D.C.L., F.R.S., President.

The practical objections to the direct process, as practised in former days, and as still used to a limited extent in the United States of America and in some European countries, are that—

1. Very rich ores only are applicable, of which about one-half is converted into iron, the remainder being lost in forming slag.
2. The fuel used is charcoal, of which between three and four tons are used in producing one ton of hammered blooms.
3. Expenditure of labour is great, being at the rate of thirty-three men, working twelve hours, in producing one ton of metal (see Percy). Iron produced by direct process in the Catelon forge is therefore expensive iron, and could not compete with iron produced by modern processes except for special purposes, such as furnishing melting material for the tool steel melter.

But, it may be asked, could the advantages of the direct process not be combined with those of modern appliances for the production of pure and intense heats, and for dealing with materials in large masses, without expenditure of manual labour, and cannot chemistry help us to larger yields and the faculty of using comparatively poor and impure ores?

A careful consideration of these questions led me to the conclusion, some years ago, that here was a promising field for the experimental metallurgist, and that I possessed some advantage over others in the use of the regenerative gas furnace as a means of producing the requisite quality of heat without the use of charcoal and blowing apparatus. I engaged, accordingly, upon a series of experimental researches at my sample steel works, at Birmingham, and, in 1873, I had the honour of submitting the first fruits of these inquiries to the Iron and Steel Institute, in a paper entitled "On the Manufacture of Iron and Steel by Direct Process." Encouraged by the results I had then obtained, I ventured with some others upon some larger applications, the principal one of which has been one at Towcester, in Northamptonshire.

Viewed by the light of present experience, it would have been wiser to have fixed upon another locality with fuel, skilled labour, and better ores within easy reach; but in extenuation of the error committed, it may be urged that the site was fixed by force of circumstances rather than by selection, the chief temptation being an ample supply of small Northamptonshire ore at a very low cost. It was, however, soon discovered that this ore, although capable of producing iron of good quality, was too poor and irregular in quality to yield commercial results unless it was mixed with an equal weight of rich ore, such as pottery mine, Spanish ore, or Rollscale, all of which, as well as the fuel, are expensive at Towcester, owing to high rates of carriage. It is in consequence of these untoward circumstances that the works at Towcester have not been completed by the addition of rolling mills, the intention being to transfer the special machinery ultimately to existing ironworks when the process has been sufficiently matured for that purpose.

The Towcester Works were visited, in the autumn of last year, by two eminent metallurgists, Professors von Tunner, of Leoban, and Akermann, of Sweden, who have published the results of their observations in separate reports [1] The results noted down by Mr. von Tunner are referred to by our past-president, Mr. I. Lowthian Bell in his paper on the "Separation of Carbon, &c.," which was read in March last, and will be discussed at the Newcastle meeting. The criticisms contained in these publications are conceived in the fairest possible spirit, and form indeed a most valuable record of the progress achieved up to that time, but they furnish me with an inducement to break silence sooner than I had intended, regarding the further progress which has been effected, and the conclusions I am disposed to draw from past experience regarding the direct process of the future.

The leading idea which guided me in these was to operate upon such mixtures of ores, fluxes, and reducing agents as would, under the influence of intense heat, resolve themselves forthwith into metallic iron and a fluid cinder, differing essentially from the methods pursued by Chenat, Guilt, Blair, and others, who prepare spongy metal in the first place by a slow process which is condensed into malleable iron or steel by after-processes, but assimilating to some extent to the process first proposed by Mr. Wm. Clay. In my paper of 1873 I described two modes of effecting my purpose, the one by means of a stationary, and the other by means of a rotative furnace chamber, the former being applicable chiefly where comparatively rich ores are available, and the latter for such poorer ores as occur near Towcester.

At the Towcester Works three rotative furnaces have been erected, two of them with working drums seven feet in diameter and nine feet in length, and the third of smaller dimensions. The gas flame both enters and passes away from the back end of the furnace, leaving the front end available for the furnace door, which is stationary. The ends of the furnace chamber are lined with Bauxite bricks, and the circumference with ferrous oxides, resulting from a mixture of furnace cinder enriched with roll scale or calcined blackband in lumps. About 30 cwt. of ore mixed with about 9 cwt. of small coal having been charged into the furnace, it is made to rotate slowly for about two and a half hours, by which time the reduction of the metal should be completed, and a fluxed slag be formed of the earthy constituents containing a considerable percentage of ferrous oxide. The slag having been tapped, the heat of the furnace and the speed of rotation are increased to facilitate the formation of balls, which are in due course taken and treated in the manner to be presently described.

These balls contain on an average seventy per cent. metallic iron and thirty per cent. of cinder, and upon careful analysis it is found that the particles of iron, if entirely separated from the slag, are pure metal, although the slag may contain as much as six per cent. and more of phosphoric acid, and from one to two per cent. of sulphur. In shingling those balls in the usual manner the bulk of the cinder is removed, but a sufficient residue remains to impart to the fracture a dark appearance without a sign of crystalline fracture. The metal shows in being worked what appears to be red shortness, but what should be termed slag shortness. In repiling and reheating this iron several times this defective appearance is gradually removed, and crystalline iron of great purity and toughness is produced, but a more ready mode of treatment was suggested by Mr. Samuel Lloyd, one of my co-directors in the Towcester Company, in reverting to the ancient refinery or charcoal hearth. The balls as they came from the rotator are placed under the shingling hammer and beaten out into flat cakes not exceeding an inch in thickness. These are cut by shears into pieces of suitable size and formed into blooms of about 2 cwt. each, which are consolidated under a shingling hammer and rolled into bars.

The bars have been sold in Staffordshire and Sheffield at prices varying from 7*l*. to 9*l*. per ton, being deemed equal to Swedish bar as regards toughness and purity.

It may therefore be asserted as a matter of fact that iron and steel of very high quality may be produced from ores not superior than Cleveland ores by direct process, but the question remains at what cost this conversion can be effected. The experimental works at Towcester are, unfortunately, not sufficiently complete to furnish more than the elements upon which the question of cost may be determined, the principal reasons being that the one reheating furnace and a 30 cwt. hammer at the works are not sufficient to deal with the iron produced by the three rotators, that the iron has to be finished at a rolling-mill elsewhere, and that transports weigh heavily upon the cost of production. The principal factor in the calculation of cost is unquestionably the rotator. [A table furnishes the working result of eighteen consecutive charges as taken from the charge-book.] The mixture of ore consisted for each charge of 12 cwt. of Towcester ore (containing about 38 per cent. metallic iron) mixed with 8 cwt. of calcined Great Fenton ore, 1 cwt. of tap cinder, 1 cwt. of limestone, and $6\frac{1}{2}$ cwt. of small coal. The time occupied for each charge was three hours fifty-seven minutes, or say four hours, and the yield of hammered blooms was on an average 6 cwt. 2 qrs. 13 lbs., whereas the metal contained in each charge amounted (by estimate) to 9 cwt., showing a loss of 25 per cwt. This loss is, however, partially recovered in using a portion of the cinder again in succeeding charges, but the proportion of cinder that may be used again with impunity depends upon the amount of impurities, namely, of phosphorus, sulphur, and alumina contained in the ore. The coal used in the producers amounted to two tons per ton of hammered blooms produced, and in pricing the materials used and labour engaged upon the work, the table—prepared by the manager at the works—gives 3*l*. 8*s*. as the cost per ton of hammered blooms. To this must be added for repairs and general expenses, and the cost of rolling the hammered blooms into bars, which in the case of Towcester practice are very heavy, but of which an experienced iron-master would form his own estimate. The cost of working the metal in the hollow fires is also not included, and this may be taken to add from 25*s*. to 30*s*. to the ton. The refined iron so produced will, therefore, cost from 5*l*. 5*s*. to 5*l*. 10*s*. per ton.

Other tables give the analysis of irons produced from various descriptions of ores, and Kirkaldy's tests of the mechanical properties of the iron; but it should be understood that these tests were taken with a view rather to test various modes of manufacture than to show high results. Only a small proportion of the samples had been subjected to the refinery process, and the variable percentage of phosphorus may be taken really as indicative of the extent to which the cinder had been removed from the metal.

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[1] Das Eisenhüttenwesen von L. Ritter von Tunner, Wien, 1870.

Another table gives the analysis of slags produced in the process. These are, no doubt, rich in iron, but it must be remembered that in the case of comparatively pure ore they can be used almost entirely in succeeding charges, and that in the case of ores containing much sulphur and phosphorus they are the recipients of those impurities—in the same way as the puddling cinder carries off the same impurities in the puddling furnace—and thus serve a useful end.

If rich ores, such as hematites, are available, it is more advantageous to use a stationary furnace and to modify the process as follows:—

A mixture of pulverulent ore mixed with a suitable proportion of fluxing materials and reducing agent is prepared, and from four to five tons of it is charged from a charging platform into the heated chamber to the depth of some twelve to fifteen inches. But before charging the mixture some coke dust or anthracite powder is spread over the bottom and sides of the chamber to protect the silica lining of the same. The heat of the furnace is thereupon raised to a full welding heat, care being taken that the flame is as little oxidising as possible. The result is a powerful superficial action upon the mixture or batch, causing simultaneous reduction of the ore and fusion of the earthy constituents. In the course of two hours a thick skin of malleable iron is formed all over the surface of the mixture, which, on being withdrawn by means of hooks, is consolidated and cleared of cinder under a hammer, and rolled out in the same heat into rough sheets or bars, to be cut up and finished in the refinery furnace or charcoal hearth. One skin being removed, the furnace is closed again, and in the course of an hour and a half another skin is formed, which, in its turn, is removed and shingled, and so on until, after three or four removals, the furnace charge is nearly exhausted. A fresh charge is then added, and the same operation continued. Once every twelve hours the furnace should, however, be cleared entirely, and the furnace lining be repaired all round.

The shingled metal so produced forms an excellent melting material for the open-hearth or Siemens-Martin process; but if ores both rich and free from sulphur and phosphorus are used, together with roll and hammer scale, which forms an admirable admixture, I simplify the process still further in causing the fusion to take place in the reducing furnace.

The furnace having been charged with say five tons of batch, the heat is allowed to ply on it for four or five hours, when about two tons of hematite pig iron are charged upon the surface by preference in a heated condition. The pig metal on melting constitutes a bath on the surface of the thick metallic skin previously formed, and gradually dissolves it on the surface while it is forming afresh below, and in the course of from three to four hours the whole of the materials charged are rendered fluid, consisting of a metallic bath with a small percentage of carbon, covered with a glassy slag containing about 15 per cent. only of metallic iron. The carbon of the bath is thereupon brought down to the desired point of only about 1 per cent. of carbon and spiegeleisen or ferro-manganese is added, and the metal tapped in the usual manner. By these means the direct process of making cast steel is carried to a further limit than I have been able to accomplish before, and no difficulty has presented itself in carrying it into effect. The steel so produced is equal in quality to that produced by the open hearth process as now practised. If light scrap, such as iron and steel turnings or sheerings, are available, these may be mixed with advantage with the batch to increase the yield of metal.

These are, in short, the more recent improvements in the direct process of producing iron and steel which I have been able to effect, and which I should have been glad to lay before the Iron and Steel Institute in a more complete form than I am able to do at the present time.

THE AMERICAN ASSOCIATION AT NASHVILLE

AS we have said already, while the Nashville Meeting of the American Association could not be called a brilliant one, most of the papers read were of substantial importance, and show that a large amount of valuable scientific work is being carried on in the United States. The number of visitors does not appear to have been up to the usual mark, mainly, we believe, on account of the great heat which prevailed at Nashville, but among those present were many of the most prominent men of science in America. The reception by the authorities of the State and city was all that could be desired, and the arrangements as to excursions, entertainments, and public lectures were in every way satisfactory.

The Western Union Telegraph Company, which has a Telegraphic Station in the building where the Association met, tendered the use of its wires free for all members so far as related to domestic affairs.

It is customary at the meetings of the American Association for each of the vice-presidents to give a public lecture; we give a long abstract of the lecture by Prof. O. C. Marsh, the importance of which cannot be overrated. We have already referred briefly to Prof. Pickering's paper on the Endowment of Research. The first obstacle encountered, he said, was the opinion widely maintained, even by scientific men, that the original research of a country was natural, and that it was useless to try to force it. We might as well say that music and art were natural growths. What should we have of ancient art were it not for the encouragement of many ancient rulers? In later days how would art and literature have thrived had it not been for the support of the public in purchasing books, &c. With the man of science it was different. There was generally little or no pecuniary reward for his success. The consequence was he was obliged to engage in some other occupation, generally teaching, which still allowed a little time for research. If these same men were allowed to devote their entire energies to investigation, and were aided by the necessary appliances, far more would be accomplished. The solution of the matter was organisation, the carrying out of a plan by which researches should be rendered as systematic as the process of mechanical arts. They had first the munificent bequest of one of the first presidents of the Association. The income of the Bache fund amounted to 2,000 dols. or 3,000 dols. Second was the Rumford fund, originally intended for giving medals in light and heat, but now largely applied to aiding investigation in these sciences. Besides these were many indirect aids. The paper then gave a plan of an institution for making researches: First, a president; second, a corps of investigators of acknowledged scientific ability; third, a large corps of assistants, whose duty it should be to carry out work laid out for them; fourth, workmen, such as mechanics. He then went on to describe a building such as would be as perfect as possible for the institution. It was useless to hope for architectural beauty, as the effect would be spoiled by attachments made to the exterior. No more common mistake was made than in wasting money which should be used for equipment. They had too many colleges with far too little endowment. Such an institution, added to a college, would prove of great advantage.

At a general evening meeting, Prof. Newcomb (president) spoke at some length, extemporaneously, on the two recent important discoveries made by American men of science, viz., the existence of oxygen in the sun, by Prof. Draper, and the satellites of Mars, by Prof. Hall. At the same evening meeting Prof. A. R. Grote, of Buffalo, read a sketch of a scheme for an international scientific service formed by the union of the various civilised governments and national scientific societies, for the carrying out of such scientific work as all the world is interested in. Under the auspices of such an association "all extra-limital, astronomical, geographical, and biological expeditions would be fitted out and directed to those places which would be most fruitful for the particular purpose."

Of the papers read in the various sections we are able, at present, to give little else but the titles. In Section A, which includes Mathematics, Astronomy, Physics, Chemistry, and Mineralogy, the following, among other papers were read:—*On a New Type of Steam Engine theoretically capable of utilising the full Mechanical Equivalent of Heat Energy, and on some points of Theory indicating its Practicability*, by Prof. R. H. Thurston; *Mechanics of the Flight of Birds*, by Mr. A. C. Campbell. An interesting paper in this section by Prof. Forshey, treated of *The Physics of the Gulf of Mexico and of its Principal Affluent the Mississippi*; the author brought together many important data concerning what he styled "the cis-Atlantic Mediterranean." Another paper in this section by Prof. Mendenhall, was *On Measurement of the Wave-length of the Blue Line of the Indium Spectrum.*

Among papers in Section B (Geology, Zoology, Botany, and Anthropology) we notice the following as likely to prove of importance:—*The Structure of Eruptive Mountains*, by Prof. Powell; *On Sex in Flowers*, by Mr. Thos. Meehan; *On the Original Connection of the Eastern and Western Coalfields of the Ohio Valley*, *On the Continuation of the Fields of the Alleghany Chain to the North of the Delaware River*, and *On the Geographical and Geological Distribution of the Genus Beatricea, and of certain other Fossil Corals in the Rocks of the Cincinnati Group*, all by Prof. Shaler; *On the Classification of the Extinct Fishes of the Lower Types*, and *On the Origin of Structural Variation*, by Prof. Cope; *Notes on the Geology of the Rocky Mountains*, by Prof. Sterry Hunt; *Some Popular Errors concerning the North American Indians*, by Capt. Powell. In a paper by the same author, *On Overplacement*, he asserted that the effects of glacial action had been greatly over-estimated in the western country, and that the "overplacement" in the Mississippi Valley was due rather to the erosion of the atmosphere, the rains of centuries, and the rivers. A curious paper in this section was by a lady, Mrs. H. K. Ingram, *On Atmospheric Concussion as a Means of Disinfection*, in which she confidently advanced the idea, based on the germ-theory of disease, that by means of concussion produced by gunpowder explosion or other effective method, cholera and other epidemic diseases could be effectually prevented or dissipated. In a paper by Lieut.-Col. Mallery, the author held that the Indians are not passing away; there are now in existence, he stated, 300,000 Indians, of whom 50,000 are Sioux. Instead of decreasing with advancing civilisation, they are steadily increasing, and Col. Mallery believes that the native population of America, north of Mexico, at the time of its discovery, has been widely over-estimated. Capt. Powell agreed with Col. Mallery, and stated his conviction that at the time of the discovery of America there were not more than 500,000 natives north of Mexico, while now in the States, Canada, and Alaska there are about 400,000. As president of the Sub-section of Anthropology, Prof. Daniel Wilson gave an interesting address on *Races in America*, presenting a *résumé* of the various theories that had been advanced with respect to American ethnology and the peopling of America, and giving some wise advice as to how future researches ought to be conducted. Another anthropological paper was on the *Origin of the Japanese*, by a native of Tokio, Shuje Isawa, in which the author came to the conclusion that the present Japanese are descended from Hindoo conquerors.

No paper of general importance seems to have been read in permanent Sub-section C (Chemistry), all of them, judging from the titles, being on points mainly of manufacturing interest.

It was decided that the next meeting should be held at St. Louis, and at the closing meeting an Education Committee was appointed with a view to the introduction of science into the schools of the country. Another committee was appointed to report annually on the relations of science to the industrial arts, and the following important resolution was passed in reference to the Signal Service Weather Reports:—

"Resolved, that this Association most respectfully asks the attention of Congress and the country to the great advances in the science of meteorology and in the art of weather prediction, which might be hoped for if the meteorological observations now taken by the Army Signal Office, under the direction of the Secretary of War, were made the subject of special research and discussion by scientific experts.

"Resolved, further, that a committee of five members or fellows be appointed by the President to represent this Association before Congress as petitioners for such permanent and liberal organisation of the meteorological service, that the valuable material collected by it may be utilised in the manner here suggested."

INTRODUCTION AND SUCCESSION OF VERTEBRATE LIFE IN AMERICA[1]

II.

The reptiles most characteristic of our American cretaceous strata are the *Mosasauria*, a group with very few representatives in other parts of the world. In our cretaceous seas they ruled supreme, as their numbers, size, and carnivorous habits enabled them to easily vanquish all rivals. Some were at least sixty feet in length, and the smallest ten or twelve. In the inland cretaceous sea, from which the Rocky Mountains were beginning to emerge, these ancient "sea serpents" abounded; and many were entombed in its muddy bottom. On one occasion, as I rode through a valley washed out of this old ocean bed, I saw no less than seven different skeletons of these monsters in sight at once. The mosasaurs were essentially swimming lizards, with four well-developed paddles, and they had little affinity with modern serpents, to which they have been compared.

The *Crocodilia* are abundant in rocks of cretaceous age in America, and two distinct types are represented. The tertiary marine beds of the Atlantic coast contain comparatively few crocodilian remains, and all are of modern types, the genus *Gavialis* having one eocene species, and the alligator being represented only in the latest deposits.

It is worthy of special mention in this connection that no true *Lacertilia*, or lizards, and no *Ophidia*, or serpents, have yet been detected in American cretaceous beds; although their remains, if present, would hardly have escaped observation in the regions explored. The former will doubtless be found, as several species occur in the mesozoic of Europe, and perhaps the latter, although the ophidians are apparently a more modern type. In the eocene lake-basins of Western America, remains of lizards are very numerous, and indicate species much larger than any existing to-day.

The first American serpents, so far as now known, appear in the eocene, which contains also the oldest European species.

The *Pterosauria*, or flying lizards, are among the most interesting reptiles of mesozoic time, and many of them left their remains in the soft sediments of our inland cretaceous sea. These were veritable dragons, having a spread of wings of from ten to twenty-five feet.

The strange reptiles known as *Dinosauria*, which, as we have seen, were numerous during the deposition of our triassic shales and sandstones, have not yet been found in American Jurassic, but were well represented here throughout the cretaceous, and at its close became extinct. These animals possess a peculiar interest to the anatomist, since, although reptilian in all their main characters, they show clear affinities with the birds, and have some features which may point to mammals. The cretaceous dinosaurs were all of large size, and most of them walked on the hind feet alone, like modern struthious birds. Near the base of our cretaceous formation in beds which I regard as the equivalent of the European Wealden, the most gigantic forms of this order yet discovered have recently been brought to light. One of these monsters (*Titanosaurus montanus*) from Colorado, is by far the largest land animal yet discovered, its dimensions being greater than was supposed possible in an animal that lived and moved upon the land. It was some fifty or sixty feet in length, and, when erect, at least thirty feet in height. It doubtless fed upon the foliage of the mountain forests, portions of which are preserved with its remains. With *Titanosaurus* the bones of smaller dinosaurs, one (*Nanosaurus*) not larger than a cat, as well as those of crocodiles and turtles, are not uncommon. The recent discovery of these interesting remains, many and various, in strata that had long been pronounced by professional explorers barren of vertebrate fossils, should teach caution to those who decline to accept the imperfection of our knowledge to-day as a fair plea for the supposed absence of intermediate forms.

In the marine cretaceous beds of the west only a single dinosaur (*Hadrosaurus agilis*) has been found, but in the higher fresh-water beds which mark the close of this formation their remains are numerous, and indicate several well-marked species, if not genera.

The first appearance of birds in America, according to our present knowledge, was during the cretaceous period, although many announcements have been made of their existence in preceding epochs. The evidence of their presence in the trias, based on footprints and other impressions is at present, as we have seen, without value, although we may confidently await their discovery there if not in older formations. *Archæopteryx*, from the European Jura, the oldest bird known, and now fortunately represented by more than a single specimen, clearly indicates a much higher antiquity for the class. The earliest American forms at present known are the *Odontornithes*, or birds with teeth, which have been exhumed within the last few years from the chalk of Kansas. The two genera *Hesperornis* and *Ichthyornis* are types of distinct orders, and differ from each other and from *Archæopteryx* much more than do any existing birds among themselves, thus showing that birds are now a closed type, and that the key to the history of the class must be sought for in the distant past.

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[1] Abstract of a lecture delivered at the Nashville meeting of the American Association, August 30, by Prof. O. C. Marsh Continued from p. 450.

In *Hesperornis* we have a large aquatic bird, nearly six feet in length, with a strange combination of characters. The jaws are provided with teeth, set in grooves; the wings were rudimentary, and useless, while the legs were very similar to those of modern diving birds. This last feature was merely an adaptation, as the more important characters are struthious, showing that *Hesperornis* was essentially a carnivorous swimming ostrich. *Ichthyornis*, a small flying bird, was stranger still, as the teeth were in sockets, and the vertebræ biconcave, as in fishes and a few reptiles. *Apatornis* and all other allied forms occur in the same beds, and probably all were provided with teeth. It is strange that the companions of these ancient toothed birds should have been pterodactyls without teeth. In the later cretaceous beds of the Atlantic coast various remains of aquatic birds have been found, but all are apparently distinct from those of the west.

During the tertiary period birds were numerous in this country, and all yet discovered appear to have belonged to modern types.

It is now generally admitted by biologists who have made a study of the vertebrates, that birds have come down to us through the dinosaurs, and the close affinity of the latter with recent struthious birds will hardly be questioned. The case amounts almost to a demonstration, if we compare, with dinosaurs, their contemporaries, the mesozoic birds. The classes of birds and reptiles, as now living, are separated by a gulf so profound, that a few years since it was cited by the opponents of evolution as the most important break in the animal series, and one which that doctrine could not bridge over. Since then, as Huxley has clearly shown, this gap has been virtually filled by the discovery of bird-like reptiles and reptilian birds. *Compsognathus* and *Archæopteryx* of the old world, and *Ichthyornis* and *Hesperornis* of the new, are the stepping-stones by which the evolutionist of to-day leads the doubting brother across the shallow remnant of the gulf once thought impassable.

It remains now to consider the highest group of the animal kingdom, the class *Mammalia*, which includes Man. Of the existence of this class before the trias we have no evidence, either in this country or in the old world, and it is a significant fact that at essentially the same horizon in each hemisphere, similar low forms of mammals make their appearance. Although only a few incomplete specimens have been discovered, they are characteristic and well preserved, and all are apparently marsupials, the lowest mammalian group which we know in this country, living or fossil. The American triassic mammals are known at present only from two small lower jaws, on which is based the genus *Dromotherium*, supposed to be related to the insect-eating *Myrmecobius*, now living in Australia.

Although the Jura of Europe has yielded other similar mammals, we have as yet none of this class from that formation; while, from rocks of cretaceous age, no mammals are known in any part of the world.

In the lowest tertiary beds of this country a rich mammalian fauna suddenly makes its appearance, and from that time through the age of mammals to the present, America has been constantly occupied by this type of life in the greatest diversity of form. Fortunately, a nearly continuous record of this life, as preserved, is now accessible to us, and ensures great additions to our knowledge of the genealogy of mammals, and perhaps the solution of more profound problems.

The boundary line between the cretaceous and tertiary in the region of the Rocky Mountains has been much in dispute during the last few years, mainly in consequence of the uncertain geological bearings of the fossil plants found near this horizon. The accompanying invertebrate fossils have thrown little light on the question, which is essentially whether the great lignite series of the West is uppermost cretaceous, or lowest eocene. The evidence of the numerous vertebrate remains is, in my judgment, decisive, and in favour of the former view.

This brings up an important point in palæontology, one to which my attention was drawn several years since, namely, the comparative value of different groups of fossils in marking geological time. In examining the subject with some care, I found that for this purpose plants, as their nature indicates, are most unsatisfactory witnesses; that invertebrate animals are much better; and that vertebrates afford the most reliable evidence of climatic and other geological changes. The sub-divisions of the latter group, moreover, and in fact all forms of animal life, are of value in this respect, mainly according to the perfection of their organisation, or zoological rank. Fishes, for example, are but slightly affected by changes that would destroy reptiles or birds, and the higher mammals succumb under influences that the lower forms pass through in safety. The more special applications of this general law, and its value in geology, will readily suggest themselves.

The evidence offered by fossil remains is, in the light of this law, conclusive, that the line, if line there be, separating our cretaceous from the tertiary, must at present be drawn where the dinosaurs and other mesozoic vertebrates disappear, and are replaced by the mammals, henceforth the dominant type.

It is frequently asserted, and very generally believed, that the large number of huge *Edentata* which lived in North America during the post-pliocene, were the results of an extensive migration from South America soon after the elevation of the Isthmus of Panama, near the close of the tertiary. No conclusive proof of such migration has been offered, and the evidence it seems to me, so far as we now have it, is directly opposed to this view. No undoubted tertiary edentates have yet been discovered in South America, while we have at least two species in our miocene, and during the deposition of our lower pliocene large individuals of this group were not uncommon as far north as the forty-third parallel of latitude, on both sides of the Rocky Mountains. In view of these facts and others which I shall lay before you, it seems more natural to conclude from our present knowledge that the migration which no doubt took place was from north to south. The edentates finding thus in South America a congenial home flourished greatly for a time, and, although the larger forms are now all extinct, diminutive representatives of the group still inhabit the same region.

The ungulates are the most abundant mammals in the tertiary, and the most important, since they include a great variety of types, some of which we can trace through their various changes down to the modified forms that represent them to-day. Of the various divisions in this comprehensive group, the perissodactyle, or odd-toed ungulates, are evidently the oldest, and throughout the eocene are the prevailing forms. Although all of the perissodactyles of the earlier tertiary are more or less generalised, they are still quite distinct from the artiodactyles, even at the base of the eocene. One family, however, the *Coryphodontidæ*, which is well represented at this horizon, both in America and Europe, although essentially *Perissodactyle*, possesses some characters which point to a primitive ungulate type from which the present orders have been evolved. Among these characters are the diminutive brain, which in size and form approaches that of the reptiles, and also the five-toed feet from which all the various forms of the mammalian foot have been derived. Of this family, only a single genus, *Coryphodon* (*Bathmodon*), is known, but there were several distinct species. They were the largest mammals of the lower eocene, some exceeding in size the existing tapirs.

In the middle eocene, west of the Rocky Mountains, a remarkable group of ungulates makes its appearance. These animals nearly equalled the elephant in size, but had shorter limbs. The skull was armed with two or three pairs of horn-cores, and with enormous canine tusks. The brain was proportionally smaller than in any other land mammal. The feet had five toes, and resembled in their general structure those of *Coryphodon*, thus indicating some affinity with that genus. These mammals resemble in some respects the perissodactyles, and in others the proboscidians, yet differ so widely from any known ungulates, recent or fossil, that they must be regarded as forming a distinct order, the *Dinocerata*.

Besides these peculiar mammals which are extinct, and mainly of interest to the biologist, there were others in the early tertiary which remind us of those at present living around us. When a student in Germany some twelve years ago, I heard a world-renowned professor of zoology gravely inform his pupils that the horse was a gift of the old world to the new, and was entirely unknown in America until introduced by the Spaniards. After the lecture I asked him whether no earlier remains of horses had been found on this continent, and was told in reply that the reports to that effect were too unsatisfactory to be presented as facts in science. This remark led me, on my return, to examine the subject myself, and I have since unearthed, with my own hands, not less than thirty distinct species of the horse tribe, in the tertiary deposits of the west alone; and it is now, I think, generally admitted that America is, after all, the true home of the horse.

I can offer you no better illustration than this of the advance vertebrate palæontology has made during the last decade, or of the important contributions to this progress which our Rocky Mountain region has supplied.

The oldest representative of the horse at present known is the diminutive *Eohippus* from the lower eocene. Several species have been found, all about the size of a fox. Like most of the early mammals, these ungulates had fourty-four teeth, the molars with short crowns, and quite distinct in form from the premolars. The ulna and the fibula were entire and distinct, and there were four well-developed toes and a rudiment of another on the fore-feet, and three toes behind. In the structure of the feet and in the teeth, the *Eohippus* indicates unmistakably that the direct ancestral line to the modern horse has already separated from the other perissodactyles. In the next higher division of the eocene another genus (*Orohippus*) makes its appearance, replacing *Eohippus*, and showing a greater, although still distant, resemblance to the equine type. The rudimentary first digit of the fore-foot has disappeared, and the last premolar has gone over to the molar series. *Orohippus* was but little larger than *Eohippus*, and in most other respects very similar. Several species have been found in the same horizon with *Dinoceras*, and others lived during the upper eocene with *Diplacodon*, but none later.

Near the base of the miocene, in the brontotherium beds, we find a third closely-allied genus, *Mesohippus*, which is about as large as a sheep, and one stage nearer the horse. There are only three toes and a rudimentary splint bone on the fore-feet, and three toes behind. Two of the premolar teeth are quite like the molars. The ulna is no longer distinct, or the fibula entire, and other characters show clearly that the transition is advancing. In the upper miocene *Mesohippus* is not found, but in its place a fourth form, *Miohippus*, continues the line. This genus is near the *Anchitherium* of Europe, but presents several important differences. The three toes in each foot are more nearly of a size, and a rudiment of the fifth metacarpal bone is retained. All the known species of this genus are larger than those of *Mesohippus*, and none pass above the miocene.

The genus, *Protohippus* of the lower pliocene, is yet more equine, and some of its species equalled the ass in size. There are still three toes on each foot, but only the middle one, corresponding to the single toe of the horse, comes to the ground. This genus resembles most nearly the *Hipparion* of Europe. In the pliocene we have the last stage of the series before reaching the horse, in the genus *Pliohippus*, which has lost the small hooflets, and in other respects is very equine. Only in the upper pliocene does the true *Equus* appear and complete the genealogy of the horse, which in the post-tertiary roamed over the whole of North and South America and soon after became extinct. This occurred long before the discovery of the Continent by Europeans, and no satisfactory reason for the extinction has yet been given. Besides the characters I have mentioned there are many others in the skeleton, skull, teeth, and brain of the forty or more intermediate species, which show that the transition from the eocene *Eohippus* to the modern *Equus* has taken place in the order indicated, and I believe the specimens now at New-Haven will demonstrate the fact to any anatomist. They certainly carried prompt conviction to the first of anatomists who was the honoured guest of the Association a year ago, whose genius had already indicated the later genealogy of the horse in Europe, and whose own researches so well qualified him to appreciate the evidence here laid before him. Did time permit I might give you at least a probable explanation of this marvellous change, but justice to the comrades of the horse in his long struggle for existence demands that some notice of their efforts should be placed on record.

(*To be continued.*)

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 17.—M. Peligot in the chair.—The president requested the meeting to designate one of their fellows to represent the Academy in the annual public meeting of the five academies, which will take place on October 25.—M. Tresca then, in the name of M. Leverrier, presented to the Academy vol. viii. of the "Atlas Météorologique de l'Observatoire de Paris pour l'an 1876."—A note by M. Faye, on the atlas of the superior movements of the atmosphere, by M. H. Hildebrandsson. The author bases his work on the observation of cirrhus clouds.—A note by M. G. de Saporta on the discovery of fossil plants in the tertiary strata in the vicinity of the North Pole.—On an erratic block of granite situated in the neighbourhood of Geneva, by M. de Marignac. It appears that the block in question is a mass of about 300 cubic metres of granite, and that the prefect of the Department, Haute Savoie, has given permission to a Railway Company to take possession of it and to cut it to pieces. M. de Marignac, who is the owner of the ground upon which it lies, now recommends the preservation of the block and offers it to the Academy together with the area it lies upon, under the sole condition that it shall be preserved. M. Dumas spoke in favour of M. de Marignac's proposition.—On the spontaneous disappearance of phylloxera, by H. Marès.—M. P. de Tchihatcheff then presented to the Academy his translation of M. Grisebach's work, "The Vegetation of the Globe," and made some remarks on the same.—M. Alluard read a memoir on a new condensation-hygrometer, invented by himself.—A letter from M. E. Stephan announcing the discovery of a new comet by M. Coggia was read. (Of this we gave the details in the Astronomical Column of our last number, p. 442.) The letter further contained details of an observation of one of the satellites of Mars, by M. Borrelly, made at Marseilles.—M. Leverrier transmitted to the Academy details of MM. Paul and Prosper Henry's observation of the same satellite, made with the equatorial in the garden of the Paris Observatory.—M. P. H. Boutigny pointed out that in a passage in his work, "Études sur les corps à l'état sphéroïdal," published some thirty years ago, he expressed his belief in the existence of satellites of Mars and pronounced the hope of their future discovery.—New researches on the ammoniacal fermentation of urine and spontaneous generation, by MM. P. Cazeneuve and Ch. Livon.—On the physiological action of salicylate of soda, by MM. Bochefontaine and Chabbert.—A note by M. V. Duram on a luminous meteor observed on September 11 at Boën (Loire), and on a shock of earthquake felt at the same place on September 12. The meteor was of unusual brilliancy; it appeared in the east of the sky at 7.45 P.M.; its elevation above the horizon was but small; it left a long curved trail, and its appearance was marked by a slight detonation; the direction of its path was from north to south. The shock of earthquake was felt at 6h. 52m. true time, and lasted several seconds.—M. Faye then drew the attention of the Academy to a memoir just published by M. P. de Saint Robert, on the spherical movement of the pendulum, with regard to the resistance of the air and the rotation of the earth.

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THURSDAY, OCTOBER 4, 1877

MICROSCOPICAL PETROGRAPHY

Microscopical Petrography. By Ferdinand Zirkel. Being Vol. VI. of the Report of the United States Geological Exploration of the Fortieth Parallel made under the direction of the Engineer Department by Clarence King, Geologist-in-charge. (Washington, 1876.)

TO the massive and important series of volumes in which the Report of the Exploration of the Fortieth Parallel has been published the Engineer Department of the United States has just added a sixth which, for general interest and usefulness beyond the area of the Survey, is equal if not superior to any that has preceded it. In the course of this protracted and laborious survey many rocks were encountered to which Mr. Clarence King and his coadjutors felt somewhat at a loss to apply the petrographical nomenclature of Europe. He accordingly sought help from the highest European authority on the subject, Prof. Zirkel, of Leipzig, whom he induced to undertake the task of examining the vast collection of rock-specimens which had gathered during so many years of field-work. Prof. Zirkel accordingly crossed the Atlantic, spent some time in New York with Mr. King and his staff in making a preliminary investigation of the collection, and in learning the geological position of the specimens and the geological structure of the wide region from which they had been obtained. Subsequently a large and typical series of rock-specimens was sent over to Leipzig to be submitted to careful microscopical investigation. No fewer than twenty-five hundred thin sections were prepared and examined under the microscope. The result of Prof. Zirkel's laborious task is now given to the world and most appropriately forms a separate volume of the Report on the Geology of the Fortieth Parallel. Mr. King may be congratulated upon the judgment he has shown in the allocation of his materials. He has enriched his official publications with the most important contribution yet made to the petrography of America.

Of the way in which Prof. Zirkel has acquitted himself of the task he undertook, it is hardly possible to speak too highly. With the characteristic method of his countrymen he marshals his facts in such orderly fashion that every observation has its appropriate and proper place where it may be expected and where, if sought for, it will be found. Familiar as he is with the minute texture and composition of most European rocks, it must have been a congenial, even though laborious work, to attack on such a scale those of another continent. He has evidently given himself heartily to the investigation, and has produced a work which more than sustains his well-earned reputation.

In an introductory chapter the author briefly sketches the leading types of microscopic structure which, largely as a result of his own previous labours, have been recognised among crystalline rocks. These may be reduced to three :—1. The purely crystalline, that is, rocks which display only crystals or crystalline particles so interwoven as to form a solid, compact mass. Granite may be taken as the type of this group. 2. The half-crystalline. Rocks of this group consist partly of crystals or crystalline particles, and partly of a non-crystalline amorphous substance or paste, which may be (*a*) a colourless but more usually yellow, brown, or grey glass ; (*b*) partly devitrified by the appearance of minute translucent but non-polarizable grains (globulites), or variously-shaped opaque needles or hairs (trichites) ; (*c*) still further devitrified by the increase of these grains and needles, so that little or no glass remains—a structure termed micro-crystallitic ; or (*d*) a peculiar amorphous substance neither showing the transparency of glass nor definite grains and needles (crystallites), but appearing to consist of indistinct grains or fibres, which seem to melt into each other. This is termed the microfelsitic. 3. The non-crystalline. Here the rocks consist sometimes merely of glass, as obsidian, sometimes of the amorphous microfelsitic substance, as in felsites. Dr. Zirkel admits, however, that even where these differences of minute structure are best shown they do not suffice as a basis for the systematic arrangement of rocks, which must rest on fundamental mineral constitution. The same mass of rock, indeed, may within a short space put on extraordinary diversities of minute structure.

A number of terms are introduced into the Report which, though most of them have for some time been in use in Germany, for the most part make their first appearance here in an English dress. "Ground mass" is employed to denote what seems to the naked eye to be the dense homogeneous matrix of a rock, wherein the usual scattered porphyritic crystals are held ; "base" is used as the designation of what is only seen under the microscope to be a non-crystallised or unindividualised paste, glassy, globulitic, micro-crystallitic or micro-felsitic, as the case may be, in which the crystals, whether microscopic or visible to the naked eye, are held. "Macroscopic" has obtained wide currency in German petrographical literature as a convenient designation for what can be seen without the use of lenses. "Microlites" are minute, thin, needle-shaped, usually cylindrical bodies, which occur both in the base and in separate crystals of rocks, and represent imperfect stages in the crystallisation of different minerals ; when colourless they are called "belonites," when black and opaque, "trichites."

As most rocks have undergone more or less internal alteration, many products of decomposition are met with under the microscope which cannot always be identified with definite mineral species. No one who has practically studied microscopic petrography can fail to have been often puzzled to name some of these products. They are in far too minute quantity and too intimately diffused through the substance of a rock to be capable of being collected for chemical analysis. They present no recognisable crystallographic form, and they show no distinctive reaction under the polariscope ; yet they have too often, with no expression of hesitation, been identified with known minerals, the identifications being at the best only guesses, and sometimes most improbable ones. It has lately been the practice at Leipzig to avoid attempting such identifications when the evidence is so slight, but to be content with the application of provisional names which may include many different compounds having at least some common characters, such as opacity or colour, and to wait until the progress of investigation allows more precise names to be affixed. Prof. Zirkel now introduces these provisional and useful terms to English readers. "Opacite" includes all the black opaque amorphous grains, scales, and streaks which have resulted from the decomposition of different minerals, and which, no doubt, vary widely in chemical constitution. They probably in most cases consist largely of metallic oxides. "Ferrite" embraces those yellowish, brownish, or reddish specks, grains, veinings, or pseudomorphous crystals which occur in so many rocks where oxides of iron have decomposed. "Viridite" is the term applied to greenish transparent or translucent scales, fibres, or veins, frequently seen where hornblende, augite, or olivine have been altered. They must vary much in composition, sometimes approaching chlorite, sometimes delessite or serpentine.

These scientific terms may be usefully transplanted into English text-books. The only one which, though the great need of such a word cannot be denied, seems open to considerable objection, is "macroscopic." It is too like "microscopic," whether as written, printed, or spoken. "Gymnoscopic" would be better. But there occur throughout the Report many nouns and adjectives which the reader will in vain look for in any dictionary, and the meaning of some of which he will not readily appreciate if he does not happen to be familiar with the German petrographical terms for which they are intended. Such are "fibration," "lamellation," "inclusion," "zonally," "lineated," "fluidal," "interwedged," and many more. Even ordinary words are used in a way which is apt to puzzle the uninitiated. For example, "some occurrences are poor in augite," "poorly-shaped crystals," "drop-like or crippled minerals." The English language is not quite so meagre as to be unable to furnish expression in already familiar words and phrases to the ideas sought to be conveyed by these novel and sometimes rather uncouth terms.

After a brief chapter devoted to the crystalline schists and their related rocks, the author proceeds to what are commonly known as the igneous rocks, beginning with granite and the early intrusive porphyries and felsites, passing thence through the diorite, diabase, gabbro, and other groups, into the wide series of tertiary volcanic products. V. Richthofen's name propylite is retained for the oldest eruptive rock of the tertiary series—a mixture of plagioclase felspar with hornblende, having most of the characters of the old diorites and dioritic porphyries. The petrographical differences between this rock and andesite are carefully summed up by Prof. Zirkel; but at the most they appear to be rather fine-drawn. He insists that rocks of different geological date can be distinguished petrographically, and that this may be done even among the different members of the tertiary series. Undoubtedly the most important chapter of the Report is that devoted to the trachytic and rhyolitic rocks. Among the trachytes some have been found containing augite instead of hornblende—a curious and novel fact which establishes an analogy between these tertiary masses and some old syenites of Tyrol and Norway, in which G. von Rath has lately shown that augite replaces hornblende. The author partly following von Richthofen divides the rhyolites into (1) Nevadite or granitic rhyolite; (2) Rhyolite proper, including the felsitic and porphyritic varieties, of which he has found among the rocks of the Fortieth Parallel no fewer than sixteen well-defined types; and (3) Hyaline rhyolite, including the glassy and half-glassy varieties, obsidian, pitchstone, pumice, &c. With the exception of some varieties in the eastern part of the region, all the basalts met with in the course of this survey prove to be felspar-basalts. Though repeating in Western America the familiar characters of the basalts of Western Europe they contain some varieties which merit a special subdivision. These are marked by (1) the invariable presence, though in small quantity, of sanidine, (2) the general absence of olivine, (3) the abundance of the glassy microlitic base, (4) the occasional presence of hornblende, (5) a high proportion of silica, (6) the dusty character of the included apatite. A petrographer who admits such wide departures from the normal type of a species must not be surprised at those who would further seek to unite some of his species which hardly differ from each other so much as these varieties of basalt do.

The Report is illustrated by twelve quarto coloured plates. For beauty of execution nothing has appeared like them since those of the lamented Vogelsang. They have been executed at Leipzig, under the author's own eye, and are evidently as faithful as they are vivid and artistic.

ARCHIBALD GEIKIE

OUR BOOK SHELF

Results of the Aralo-Caspian Expedition. Fascicule iv., 388 pp., with seven lithographed plates; and Fascicule v., sixty-eight pages. (St. Petersburg, 1877.) [Russian.]

THE fourth fascicule of this publication contains an important paper by the well-known Russian ichthyologist, Prof. Kessler, on "The Fishes of the Aralo-Caspian Pontic Ichthyological Region." After an introduction, in which the author briefly sketches the geography of the region, and makes a few objections to some statements of Mr. A. R. Wallace as to the geographical distribution of fishes, Prof. Kessler describes forty-three new species and varieties of fishes of the region, and twenty-four other species, the previous descriptions of which were incomplete. These descriptions, being the result of very elaborate researches, are based on extensive collections obtained by the members of the expedition, and by previous explorers. The new species are illustrated by seven plates. The second part of the work is a systematic catalogue of all fishes known to inhabit the region, with notes as to their geographical distribution.

The third part deals with the general conclusions arrived at by the author as to the geographical distribution of species, the relations of the Aralo-Caspian ichthyological fauna to the faunas of the neighbouring basins, the distribution of species in different waters of the region, the zoological characteristics of the fishes inhabiting it, and their genealogical relations, their mode of life, and some remarks on the geological history of the region. These conclusions (some of which have already been noticed in NATURE) will certainly be of great interest to the zoo-geographer, and their importance is much enhanced by the usual caution of M. Kessler's statements. The work is altogether an important acquisition to ichthyological literature in general, all the more that it deals with countries very imperfectly known until now.

The fifth fascicule of the work contains two papers by M. Alénitzin: "On the Sweet Water Springs on the Shores of Lake Aral," and a "Sketch of the History of the Islands of that Lake," the former containing some interesting information as to the distribution of water in sandy steppes.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Urticating Organs of Planarian Worms

THERE exist, as is well known to all comparative anatomists, in the skin of most planarian worms certain rod-like bodies (Stäbchenkörperchen of German authors) concerning the function and homologies of which there has been considerable speculation and difference of opinion. By some authors these bodies, which always at an early stage of their existence are contained in cells, "the rod-cells" have been compared to the thread-cells or nematocysts of cœlenterata, the rod-cells being considered homologous to or possibly homogenous with these cœlenterate nematocysts. In the July number of the *Quarterly Journal of Microscopical Science*, vol. lxvii., new series, 1877, I published a paper on the structure of several forms of land planarians obtained by me during the voyage of H.M.S. *Challenger*. In this paper is described and figured the structure of the rod-cells of several genera of land planarians as observed in the fresh and living condition. In an American form *Geoplana flava* and also in a *Geoplana* from New Zealand and a *Rhynchodemus* from the Cape of Good Hope rod-cells were observed in which the rods are much longer than the cells and are in their quiescent condition coiled spirally within the cells (*l.c.*, Pl. xx. Figs. 15, 20, 21, 22, 23), but which rods are shot out from the cells and protruded for a long distance beyond the surface of the epidermis when the animal is compressed or irritated. Such probably is the mature condition of the cells in question in all land planarians. Mecznikow has described a somewhat similar form of cell as existing in his *Geodesmus bilineatus*.

In some microscopic sections of land planarians hardened in alcohol, the shot-rods or threads may be seen in abundance when closely looked for, projecting from the edges of the section of the epidermis. The demonstration of the spiral coiling of the rods within the cells, and of their protrusion on irritation, would at first sight seem to ally these bodies more closely than ever with cœlenterate nematocysts, but there is this great difference between the two structures, that several rods are present in each cell in the planarians, and that the rods are solid and apparently free within the cell, and when protruded by the bursting of the cell are shot clear of it. In cœlenterates, as is familiarly known, the thread is continuous with the cell and hollow, and is everted in the act of protrusion.

In the summary of my paper above referred to (*l.c.*, p. 292) I suggested that it would be interesting to test the action of the rod bodies of land planarians by applying a living worm to the tongue and observing whether urtication was produced. I wrote at the time to my friend, Mr. Thwaites, F.R.S., curator of the Royal Botanic Gardens at Peradeniya, Ceylon, and asked him to make the experiment, which he did forthwith, and the result shows that planarians do undoubtedly produce urtication in much the same way as cœlenterates, and there can be no doubt that this function is performed by the rod-bodies, which are thus weapons of defence, and no doubt used also to secure prey.

Mr. Thwaites writes :—"I have lost no time before attending to your request touching the planarians. I applied the tip of my tongue to two of them brought fresh and lively to me, and quite sensible was I to a feeling of unpleasant tingling, and it was accompanied with slight swelling. The sensations very similar to what is experienced upon a slight scalding. The planarian itself evidently felt very uncomfortable, as it became dilated laterally to a considerable extent during contact with the tip of the tongue, though it soon recovered its normal condition."

H. N. MOSELEY

New University Club, St. James's Street, S.W.

The Satellites of Mars

IT seems worthy of notice that the prophetic genius of Homer has already not only identified but even given names to the two satellites of Mars. I allude, of course, to the passage in the fifteenth book of the Iliad, where Ares is preparing to descend to the earth (possibly this refers to an unusually near approach at opposition, as at the present time) :—

> *Ὣς φάτο· καί ῥ' ἵππους κέλετο Δεῖμόν τε Φόβον τε*
> *ζευγνύμεν. . .* *Il.*, xv. 119.

which Pope renders—

> "With that he gives command to Fear and Flight
> To join his rapid coursers for the fight.",

Deimus and Phobus are not, perhaps, very euphonious names; but astronomers will not lightly reject the authority of Homer.

Eton, September 29 H. G. MADAN

On the Coming Winter

HAVING recently computed the remaining observations of our earth-thermometers here, and prepared a new projection of all the observations from their beginning in 1837 to their calamitous close last year—results generally confirmatory of those arrived at in 1870 have been obtained, but with more pointed and immediate bearing on the weather now before us.

The chief features undoubtedly deducible for the past thirty-nine years, after eliminating the more seasonal effects of ordinary summer and winter, are :—

1. Between 1837 and 1876 three great heat-waves, from without, struck this part of the earth; viz., the first in 1846·5, the second in 1858·0, and the third in 1868·7. And unless some very complete alteration in the weather is to take place, the next such visitation may be looked for in 1879·5, within limits of half a year each way.

2. The next feature in magnitude and certainty is, that the periods of minimum temperature, or cold, are not either in, or anywhere near, the middle time between the crests of those three chronologically identified heat-waves, but are comparatively close up to them *on either side*, at a distance of about a year and a half, so that the next such cold wave is due at the end of the present year.

This is, perhaps, not an agreeable prospect, especially if political agitators are at this time moving amongst the colliers, striving to persuade them to decrease the out-put of coal at every pit's-mouth. Being, therefore, quite willing, for the general good, to suppose myself mistaken, I beg to send you a first impression of plate 17 of the forthcoming volume of observations of this Royal Observatory, and shall be very happy if you can bring out from the measures recorded there, any more comfortable view for the public at large.

PIAZZI SMYTH
Astronomer-Royal for Scotland

Royal Observatory, Edinburgh, September 27

The Australian Monotremes

I OBSERVE in NATURE (vol. xvi. p. 439) that a doubt arises respecting the *Echidna* or Australian porcupine (recently renamed *Tachyglossus*) and the *Ornithorhynchus* being found in Northern Australia. It does exist in Queensland, but how far *north* it is impossible to decide until we are better acquainted with that extensive territory. The fact of one having been found by Mr. Kennedy, as mentioned by Mr. Forbes at Plain Creek in lat. 21° S. is, as far as the published statement can be depended upon, correct, and was never considered by any Australian in Queensland as a matter of doubt, as they were well acquainted with the animal; but whether the *Tachyglossus* was the same or of a different species I do not consider has been sufficiently noticed; whether it was the *Tachyglossus hystrix*, or with sufficient distinctive characters, as has been recently found in that of New Guinea to make it a new species, is not known, as ordinary travellers are not able to distinguish those characteristic differences which would immediately strike the experienced naturalist. The species found in the vicinity of Darling Downs, &c., is evidently the *Tachyglossus hystrix*, and from a recent letter received from my son, Mr. G. F. Bennett, he finds no difficulty in procuring specimens of this species near Foowoomba by offering rewards for those procured at certain intervals of time, to enable him to carry out his investigations on the mode of generation of the *Monotremata*, and if possible to procure the impregnated uterus of that animal, as well as that of the *Ornithorhynchus*, as in both animals it no doubt will be identical. As far as regards the rudimental pouch in the *Echidna* it is only able to be found in that animal during the breeding season, and I could never detect it at any other time. It is mentioned by Prof. Owen in his memoir on the young of the *Echidna* (*Philosophical Transactions*, 1865, p. 678), and indeed it has been a well-known fact for some period of time, as some years ago I doubted the assertion and public attention was most particularly drawn to it, and the fact was ascertained beyond doubt even before the publication of Prof. Owen's paper.

The *Ornithorhynchus* being an aquatic animal does not possess a pouch at any time. With respect to the New Guinea species of *Echidna*, the question whether the *Tachyglossus lawesi* and *T. bruijnii* are distinct species can now be decided, as I observe that examples of both sexes of *T. bruijnii* have been obtained in the mountains on the north coast of New Guinea at an elevation of about 3,500 feet. That a new and somewhat analogous species of *Tachyglossus* may yet be discovered in Northern Australia I consider very probable. GEORGE BENNETT

September 29

P.S.—By letters from Sydney dated August 4 no intelligence has been received from Sig. D'Albertis since his departure for the Fly River in May last.

Are there no Boulders in Orkney and Shetland?

IN your impression of the 13th inst. (p. 418), there is an interesting letter from Mr. Laing, M.P., stating that there are no boulders in Orkney or Shetland. He says that having "an intimate personal acquaintance with these islands, which are my native county, and almost every yard of whose surface and shores I have explored with rod and gun," . . . "I can assert positively that I never saw (in them) a boulder or perched block, or the trace of any till, or boulder clay, kame, eskar, raised beach, or other form of glacial or marine action."

Mr. Laing's object in drawing the attention of geologists to these facts is, that "if true, they seem to afford a crucial test of the truth or falsehood of some of the most important theories of modern geology."

Mr. Laing observes that in Orkney there could be no boulders, &c., because "there were no glaciers, there being no great local mass of mountains to produce them."

As regards Shetland, Mr. Laing says he cannot speak with the same confidence. "Still, having travelled over a great part of the principal islands, I can assert that I have never seen in them either, any traces of glacial action."

Mr. Laing having invited information on this subject, Prof. Geikie has published an article in the same number of your paper (p. 414), controverting Mr. Laing's statements, and maintaining that the facts ascertained by him and his colleagues in the Scotch Geological Survey establish that these islands form "no exception to the general glaciated condition of Scotland."

In corroboration, so far, of the Professor's statement, that there are in Orkney and Shetland "many transported blocks of gneiss, schist, and other rocks foreign to the immediate locality" of the blocks, I need only refer to the following list of boulders reported to the Edinburgh Royal Society Boulder Committee.

IN ORKNEY.

Eday Island.—Conglomerate B. 12 × 7 × $1\frac{1}{2}$ feet, = about 8 tons weight. Situated near top of a hill 250 feet above sea. No conglomerate rock in Eday, but there is in Stronsay Island.

Frith and Stennis.—White pebbles of freestone on the hills. But there are no white freestone rocks in this island. It is all old red.

Sanday.—Gneiss B. 7 × 6 × $2\frac{1}{2}$ feet = about 14 tons. No gneiss rocks on this island. Nearest island with gneiss rocks is Stromness, 30 miles distant, and in Shetland, still more distant.

Walls.—Lydian stone B. 9 × 7 × 6 feet = about twenty-eight tons. Sandstone is the prevailing rock.

Stromness Island.—Two granite boulders lying on red sandstone rocks—distant, one a quarter of a mile, the other one mile from granite hills.

IN SHETLAND.

Bressay.—A number of boulders here, of a rock foreign to the island. One of them is 10 × 7 × 4 feet. Supposed to have been transported from Norway.

Housay.—On a cliff 200 feet above the sea, rounded blocks resting on knolls of polished rock.

Neay.—Large perched blocks, some many tons in weight.

Sumburgh Head.—Conglomerate B. lying on sandstone rock.

Where can it be supposed that these boulders come from?

Prof. Geikie thinks there were glaciers, at all events, in one of the islands, viz., *Hoy*, and even "separate glaciers" in all the valleys of that hill, whose top is only 1,550 feet above the sea. I feel great difficulty in subscribing to that opinion; I rather agree with Mr. Laing, that there could be no glaciers, for want of a sufficient "mass of mountain region to produce them." Even if in Hoy glaciers could have been formed on a hill the highest peak of which is only 1,550 feet above the adjoining sea, what is to be said of those boulders which are on islands where the hills do not exceed 500 or 200 feet, and in which there are no rocks of the same nature as the boulders?

Prof. Geikie refers to the rock striations in Orkney and Shetland (which Mr. Laing seems not to have discovered) as additional proofs of glacial action. If these striations had been caused by glaciers, the direction of the striæ would vary with the direction of the different valleys in which the glaciers moved. But this is found not to be the case. Prof. Geikie says that both in Orkney and in Shetland the movement of the ice has been on the "whole along a north-west and south-east line." He refers to reports by his colleagues, Mr. Peach and Mr. Horne, in corroboration of his statements.

In looking into Mr. Peach's report, I find that he specifies the striæ on the rocks of Shetland as running in *various* directions. In *Unst*, the most northern island, he says "the destroying force (the nature of which force, however, he avoids indicating) coming against a hill (called the '*Muckle Heog*,' 500 feet high) on its north-west flank, had been partially *turned* by the hill into a valley (which he names) and made to produce the well-known phenomenon of 'crag and tail'"—the crag or bared rock being on the north side of the hill.

Mr. Horne in his paper also describes the striæ in Shetland as running in *various different* directions. Some of the striæ on the rocks, and the boulders on the surface, indicated, as he thought, ice action from east to west. "In addition to these, however, *others* (he says) were found, which could not have been produced by ice-shedding in the ordinary way. These cross the island, *regardless of its physical features*, and are *often at right angles* to the newer set."

These facts, I venture to submit, may be explained by supposing that the Shetland and Orkney Islands, when still under the ocean, were subjected to the action of Arctic currents loaded with icebergs and shore ice. We know that in the Arctic regions now, fragments of rocks are by these means carried about in various directions, and dropped on the sea-bottom; whilst the rocks at the sea bottom are ground down, polished and striated by the hard stones and gravel pushed forward by icebergs. The existence of Arctic currents from north-western points has indeed been already well established by a study of the boulders and striated rocks found along the west coasts of Caithness, Ross, Argyll, and the islands of Lewis, Harris, and Uist.

The inference of Mr. Peach from what he saw near Lerwick was, that there "the *direction* the *drift* came from is evidently *northerly*." "The destroying force" to which Mr. Peach refers as having swept across the island of Unst baring a hill up to a height of 500 feet *on its north-west* flank, could have been no other than an Arctic current loaded with ice.

These facts establish points of the highest geological interest. They indicate a submersion of the northern parts of Europe under the ocean to the extent of many hundred feet, and the non-existence of any gulf-stream flowing through the North Atlantic. The Isthmus of Panama requires to be depressed only 350 feet, to allow that stream to flow into the Pacific.

The separate question of "*Raised beaches*" mooted by Mr. Laing and discussed by Prof. Geikie, I do not enter on. Both of these authorities agree that there are no raised beaches in Shetland and Orkney. It is indeed very curious that such should be the case, considering that they exist along the Caithness coast, and in every other part of the kingdom, including Ireland. I may, however, notice that Mr. Peach in his Report on Shetland speaks of a "raised beach" as having been seen by him there.

Milne Graden, Coldstream, N.B. DAVID MILNE HOME

Fertilisation of Flowers by Birds

AMONG the "Biological Notes" in NATURE, vol. xv. p. 416, there is one referring to the agency of birds in effecting the fertilisation of flowers. A few weeks before reading this note I was induced to suspect that many flowers might be dependent wholly or in part on the visits of small birds for their effectual fertilisation by observing that a very considerable number of birds shot at that time had the plumes of the forehead and the lores thickly dusted with pollen. This fact was noticed in several species of *Dicæinæ* and *Nectariniinæ*, in the *Loriculi*, and even in a glossy starling (*Calornis panayensis*), which latter is mainly a frugivorous bird. Both the sun-birds and flower-peckers are fruit-feeders to a certain extent; but they also prey on minute insects, in search of which (and possibly of the nectar sometimes) they diligently probe the corollas of numerous flowers, and on withdrawing their heads a portion of the pollen remains in many instances adhering to the plumage bordering the bill, and is carried away and introduced into the next blossom visited.

The immediate source of attraction possessed by the flower for its feathered visitants lies, I think, in the small insects which resort to it, and not, at any rate usually, in their furnishing any nectareous secretion which is palatable to the birds. For if the latter visited the blossoms for the sake of the nectar they would be perfectly acquainted by experience with its situation and make no delay in going straight to it, whereas the habit of the sun-birds and the flower-peckers also is rather to hover on rapidly-vibrating wings a few inches in front of the opening of a blossom, as if prying into its recesses in search of food, before thrusting their beaks into the corolla; and often after thus examining a flower they fly off to another without touching it at all, having apparently satisfied themselves that the first one contained no prey for them.
A. H. EVERETT

N. Mindanao, July 23

Heat Phenomena and Muscular Action

ON reading the article which appeared in NATURE, vol. xvi. p. 451, on the heat phenomena accompanying muscular action, it has occurred to me to send the following problem which is akin to the subject.

If a man does work (say lifts a weight), the principle of the conservation of energy teaches us that the potential energy—the work done—(weight lifted) is at the expense of the man as a magazine of force, in fact that "virtue has gone out of him." Now suppose a man lifts say a ton of bricks and deposits the bricks one by one on the top of a wall six feet high, we can exactly estimate the amount of work done, the energy rendered potential and external, and if we knew also the extra amount of heat radiated or otherwise carried off from his body—as most probably the work would raise his temperature—we could exactly measure the amount of energy the lifting of the brick cost him.

Now suppose another man were to lift the bricks from the top of the wall and deposit them gently—*i.e.*, without concussion—on the ground, it is evident that there is a certain amount of potential energy disappearing, in fact that there is work being absorbed by the man, of course appearing in some other form, but the question is how? This second man's work is of course in one sense work, but in the sense of producing external, potential, or kinetic energy, is not so, unless, perhaps, in heat.

Strangely enough it follows that lifting down the brick ought to make the man either radiate heat more, waste tissue less, digest food less, or in some other way account for the energy absorbed by him.

Generally I think the conversion of force by obstruction is not always so clearly traced as it might be; in friction it is clear, as also in the compression of elastic bodies, but in the instance above, as also in the throttling of steam, it is not so clear.
A. R. MOLISON

Does Sunshine Extinguish Fire?

I READ Mr. Tomlinson's paper (NATURE, vol. xvi. p. 361) near the time of its delivery, and was struck with the inconclusive character of his experiments. What he attempted to obtain was the condition of combustion in sunshine and combustion in darkness, *cæteris paribus*. But he left the *cæteris paribus* entirely out of the experiment, and actually used a dark cubbard (I believe this is good spelling etymologically and phonetically), into which there was no free influx of atmospheric air. Naturally his candles burnt with inferior combustion there. I have for years together burnt Newcastle coal, and no other; and for years together burnt South Staffordshire coal, and no other; and I say that sunshine puts out a sea-coal fire and not a S.S. fire. The reason of this is, I apprehend, not far to seek. In the Midlands it is the practice to keep a fire alive by a raker, or gathercoal. It would be quite useless to attempt to do this with a sea-coal fire, which goes out in a short time unless the cakes of coal be broken up; in a word, one has to watch a sea-coal fire; and it must be in every Londoner's experience, that such a fire is apt to elude one at the last faint gleam from over reckless poking. Now, if the sun is shining on the coal, that last faint gleam is invisible, and the fire goes out as a matter of course. Sunshine puts out a sea-coal fire by insidiously eclipsing the warning glimmer of its expiring embers. This, at least, is a *vera causa*. *A priori* I should say that combustion would be less rapid in air rarefied by sunlight than in air deprived of it; but I do not believe sunshine extinguishes a coal fire in any other way than that I have described.
C. M. INGLEBY

Folkestone

OUR ASTRONOMICAL COLUMN

THE APPROACHING OPPOSITION OF IRIS.—The opposition of this minor planet in the present autumn affords another favourable opportunity of determining the amount of solar parallax on the method already successfully applied by Prof. Galle, of Breslau, in the case of Flora. The *Berliner astronomisches Jahrbuch* for 1879 contains a rough ephemeris of Iris for every twentieth day of the year, but this being insufficient for the purpose in view, we subjoin places calculated from Prof. Brünnow's tables of the planet, on the approximate formulæ explained in his introduction; the error of the tables being very sensible at the present time, nothing would have been gained by calculating in the accurate form. For the sake of brevity the planet's positions are given for every fourth day only, but they will be readily interpolated for the intermediate dates.

IRIS.—*At Greenwich Midnight.*

		Right Ascension. h. m. s.	North Declination. ° ′	Distance from the Earth.	Distance from the Sun.
Oct.	8	3 56 7	27 5·6	1·0034	1·8350
,,	12	3 56 50	27 3·3	0·9769	
,,	16	3 56 55	26 57·8	0·9526	1·8345
,,	20	3 56 20	26 48·9	0·9306	
,,	24	3 55 8	26 36·5	0·9111	1·8354
,,	28	3 53 20	26 20·5	0·8944	
Nov.	1	3 50 58	26 0·9	0·8808	1·8376
,,	5	3 48 8	25 37·8	0·8704	
,,	9	3 44 57	25 11·6	0·8633	1·8411
,,	13	3 41 32	24 42·7	0·8597	
,,	17	3 38 1	24 11·5	0·8597	1·8459
,,	21	3 34 31	23 38·7	0·8634	
,,	25	3 31 9	23 5·0	0·8708	1·8520
,,	29	3 28 2	22 31·2	0·8819	
Dec.	3	3 25 17	21 58·1	0·8965	1·8593
,,	7	3 22 58	21 26·5	0·9146	
,,	11	3 21 11	20 56·7	0·9360	1·8677

Iris will be in perihelion October 14·7, G.M.T., and nearest to the earth on November 15, her distance at this time being 0·859 (the earth's mean distance from the sun being taken as unity). Her intensity of light may be expected to rather exceed that of a star of the seventh magnitude, 6·8m. according to the *Berliner Jahrbuch*.

THE OUTER SATELLITE OF MARS.—This object is still under observation at the Observatory of Paris. It was also measured again by Mr. Common, of Ealing, with his 18-inch silver-on-glass reflector on September 24, the angle calculated from the elements which have been given in this column differing from the observed angle −4°. An observation on September 13, by M. Borrelly at Marseilles, presumed to apply to the satellite, must refer to a faint star, the satellite at the time being in the opposite quadrant.

BINARY STARS.—Dr. Doberck, of Markree Observatory, continues his investigations on the orbits of the revolving double stars. In No. 2,156 of the *Astronomische Nachrichten* he has given provisional elements of Σ 1768 and Σ 3121, the latter of which appears to be an object of special interest from the shortness of the period of revolution, which hardly exceeds that of the well-known binary, ζ Herculis. Also elements of Σ 3062, a star which was the subject of a pretty complete calculation by Dr. Schur in 1867. The results of the two discussions are as follow:—

	Schur 1867	Doberck 1877
Passage of peri-astre	1835·196	1834·88
Node	32° 10′	38° 35′
Angle between the lines of nodes and apsides	97° 31′	92° 7′
Inclination	29° 58′	32° 11′
Eccentricity	0·5009	0·4612
Semi-axis major	1″·310	1″·270
Period of revolution	112·64 years	104·415 years

These orbits are in very satisfactory confirmation of each other.

May we hope that at no distant period Dr. Doberck may find he has sufficient material to induce him to investigate the elements of α Centauri; a fair approximation to the true orbit might be expected from his experienced hand.

PROF. ADAMS ON LEVERRIER'S PLANETARY THEORIES[1]

II.

THE nineteenth chapter of M. Leverrier's researches, which forms the first part of the eleventh volume of the *Annals of the Paris Observatory*, contains the determination of the secular variations of the elements of the orbits of the four planets, Jupiter, Saturn, Uranus, and Neptune.

In the first place are collected the differential formulæ which are established in the previous chapter, and which give the rates of secular change of the various elements at any epoch in terms of the elements themselves, which by the previous operations have been cleared of all periodic inequalities.

The terms of different orders which enter into these formulæ are carefully distinguished.

If we were to confine our attention to the terms of the first degree with respect to the excentricities and inclinations of the orbits, and of the first order with respect to the masses, the differential equations which determine the secular variations would become linear, and their general integrals might be found, so as to give the values of the several elements for an indefinite period.

In the present case, however, the terms of higher orders are far too important to be neglected, and when these are taken into account the equations become so complicated as to render it hopeless to attempt to determine their general integrals.

Fortunately, however, these are not needed for the actual requirements of astronomy, and for any definite period the simultaneous integrals may be determined with any degree of accuracy that may be desired by the method of quadratures.

In this way M. Leverrier has determined the values of the elements for a period of 2,000 years, starting from 1850, at successive intervals of 500 years. The first steps in this integration were attended with some difficulties, because the determination of the numerical values of the rates of change of the several elements at the various epochs depends on the elements themselves which are to be determined. Hence several approximations were necessary in order to obtain the requisite precision.

After this work of M. Leverrier, however, the extension of the investigation to other epochs, past or future, is no longer attended with the same difficulties. In fact, from his results we may at once find, by the method of differences, very approximate values of the elements at an epoch 500 years earlier or later than those which he has considered. His general formulæ will then give the rates of change of the several elements at the epoch in question, and having these we can determine by a direct calculation the small corrections which should be applied to the approximate values of the elements first found.

This process may evidently be repeated as often as we choose.

It is important to remark that in the formulæ which give the rates of change of each of the elements at the five principal epochs considered, as well as in those which give the total variations of the elements at the same epochs, the masses of the several planets appear in an indeterminate form, so that it may be at once seen what part of the variation of any element is due to the action of each of the planets, and what changes would be produced in the value of any element at any epoch by any changes in the assumed values of the masses.

Consequently, when the astronomer of the future, say of 2,000 years hence, has determined the values of the elements of the planetary orbits corresponding to that epoch, it will be easy for him, by comparing those values with the general expressions given by M. Leverrier, to determine with the greatest precision the actual values of the masses, provided that all the disturbing bodies are known; and should there be any unknown disturbing causes, their existence would be indicated by the inconsistency of the values of the masses which would be found from the different equations of condition.

By means of the work which has just been described, everything has been prepared which is required for the treatment of the theories of the several planets.

The remainder of the eleventh volume of the *Annals* is accordingly occupied by the complete theories of Jupiter and Saturn, the former theory being given in Chapter 20, and the latter in Chapter 21 of M. Leverrier's researches.

The coefficients of the periodic inequalities of the mean longitudes and of the elements of the orbits are not only exhibited in a general form, but are also calculated numerically for the five principal epochs considered in Chapter 19 of these researches, viz., for 1850, 2350, 2850, 3350, and 3850.

The long inequalities of the second order with respect to the masses, depending on twice the mean motion of Jupiter plus three times the mean motion of Uranus minus six times the mean motion of Saturn, are also determined in a similar form.

Chapter 22 of M. Leverrier's researches, forming the first part of the 12th volume of the "Annals," contains the comparison of the theory of Jupiter with the observations, the deduction of the definitive corrections of the elements therefrom, and finally the resulting tables of the motion of Jupiter. The observations employed are the Greenwich observations from 1750 to 1830 and from 1836 to 1869 together with the Paris observations from 1837 to 1867.

To the results given in the Astronomer-Royal's "Reduction of the Greenwich Observations of Planets from 1750 to 1830," M. Leverrier has applied the corrections which he has found to be required by his own reduction of Bradley's observations of stars and his redetermination of the Right Ascensions of the fundamental stars, published in the second volume of the "Annals" (Chapter 10).

The equations of condition in longitude, for finding the corrections of the elements and of the assumed mass of Saturn, are divided into two series corresponding to the observations made from 1750 to 1830, and into two other series corresponding to the observations made from 1836 to 1869. Moreover in each, of these series the equations are subdivided into eight groups, corresponding to the distances of the planet from its perihelion, 0° to 45°, 45° to 90°, and so on. From these are formed four final equations, the solution of which gives the corrections of the epoch, of the mean motion, of the excentricity, and of the longitude of the perihelion, in terms of the correction required by the mass of Saturn, which is left in an indeterminate form. The substitution of these expressions in the thirty-two normal equations corresponding to the several groups above-mentioned, gives the residual differences between theory and observation in terms of the correction of the mass of Saturn. No conclusion can be drawn from the ancient observations; but from the modern observations M. Leverrier finds that the mass of Saturn assumed—which is that of Bouvard—should be diminished by about its $\frac{1}{200}$th part. This correction is very small, but M. Leverrier regards it as well established.

On the other hand, Bessel's value of the mass of Saturn, founded on his observations of the Huyghenian satellite, exceeds Bouvard's by about its $\frac{1}{350}$th part.

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[1] Continued from p. 464.

The equations of condition in latitude are treated in a similar manner, being grouped according to the distances of the planet from its ascending node. From these equations the corrections of the inclination of the orbit and longitude of the node are found separately from the ancient and from the modern observations. The results differ very little, but the second solution is employed in the construction of the tables. After the application of these corrections to the elements, the agreement between theory and observation may be considered perfect; so that the action of the minor planets on Jupiter appears to be insensible, and there is no indication of any unknown disturbing causes.

There are some peculiarities in the mode of tabulating the perturbations caused by the action of Saturn. The perturbations of longitude and of radius vector are not, as usual, exhibited directly, but instead of them M. Leverrier gives the perturbations, both secular and periodic, of the mean longitude, of the longitude of the perihelion, of the excentricity, and of the semi-axis major of the orbit, and then from the elements corrected by these perturbations he derives the disturbed longitude and radius vector by the ordinary formulæ of elliptic motion.

Where the perturbations are large M. Leverrier considers this preferable to the ordinary method of proceeding. The perturbations of latitude being small, he applies to the inclination and longitude of the node their secular variations alone, and then determines directly the periodic inequalities of latitude.

All these perturbations, whether of the elements or of the latitude, are developed in a series of sines and cosines of multiples of the mean longitude of Saturn, including a constant term, the coefficients multiplying these several terms being functions of the mean elongation of Saturn from Jupiter, which for a given elongation are developed in powers of the time reckoned from the epoch 1850. These coefficients only are tabulated with the mean elongation as the argument, and the perturbations are thence calculated by means of the ordinary trigonometrical tables. The intervals of the argument are so small, that the requisite interpolations are very simple, and the coefficients which relate to the four elements, and depend on the same argument, are given at the same opening of the tables.

The tables have been calculated specially for the 500 years included between the years 1850 and 2350. Nevertheless they may be applied to epochs anterior to 1850, by simply changing the sign of the time reckoned from 1850. For one or two centuries before 1850 this extension will have all the rigour of modern observations, while for still earlier times the accuracy of the tables will greatly surpass that of the observations which we have to compare with them.

M. Leverrier's Tables of Jupiter are now employed in the computations of the *Nautical Almanac*, beginning with the year 1878.

The thirteenth volume of the *Annals* is devoted to the theories of Uranus and Neptune. These theories are not unattended with difficulties. In the first place, these planets are disturbed by the actions of the two great masses Jupiter and Saturn, interior to their orbits, and these actions are modified by the great inequalities of Jupiter and Saturn depending on five times the mean motion of Saturn minus twice the mean motion of Jupiter. In the next place twice the mean motion of Neptune differs very little from the mean motion of Uranus, and thus arise inequalities of long period in the elements of their orbits which are large enough to produce very sensible terms of the second order. Lastly, the mean elliptic elements of the two planets are not yet sufficiently well known.

In a preliminary chapter, the 24th, M. Leverrier investigates formulæ which are specially applicable to the case of a planet disturbed by another which is considerably nearer to the sun. In this case it is easily seen that, by the direct action of the disturbing planet on the sun, perturbations of large amount may be produced in the *elements* of the orbit of the disturbed planet, while the corresponding perturbations of the co-ordinates of the planet are comparatively small. Hence arises the advantage of considering this case apart.

We have seen how closely the theories of Jupiter and Saturn are related to each other. In a similar manner the theories of Uranus and Neptune are also closely related in consequence of the great perturbations introduced into the elements of their orbits by the near approach to commensurability in their mean motions. Hence, before entering upon the separate theories, M. Leverrier devotes Chapter 25 of his researches to the determination of the mutual actions of Uranus and Neptune, and this forms the base of the theories of both planets. The method employed is similar to that adopted in the case of Jupiter and Saturn, and the results are exhibited in the same general form.

It is important to remark that the elements of Uranus and Neptune as determined from observations severally differ from their mean elliptic values by the amount of their perturbations of long period corresponding to the mean epoch of the observations. The apparent elements of Uranus and Neptune for the epoch 1850 have been carefully determined by Prof. Newcomb in his excellent work on the theory of those planets which obtained the Society's medal in 1874. By the application of his own general formulæ, M. Leverrier deduces from these elements the values of the mean elliptic elements corresponding to the same epoch. It may be remarked that the mean elements thus determined will depend on the assumed masses of the two planets, and will therefore require small corrections when more accurate values of the masses have been obtained.

When the secular variations of Uranus and Neptune given in Chapter 19 were found, the elements were less accurately known, and M. Leverrier has therefore recalculated the values of the excentricities and longitudes of the perihelia of the two planets for the same five epochs as before, starting from the mean elliptic values of the elements above referred to.

Chapter 26 contains the completion of the theory of Uranus. The last chapter, which contains the completion of the theory of Neptune, is not yet printed.

The twenty-third chapter also, which contains the comparison of the theory of Saturn with observations, together with the tables of the planet, and which will form the latter part of the twelfth volume of the *Annals*, is not yet printed. The results of this comparison of the theory with observations have, however, been fully published in the *Comptes Rendus*, and I understand that the tables will be used for computing the place of Saturn in the forthcoming volume of the *Nautical Almanac*.

Although the comparison of the theory of Saturn with observations shows in general a satisfactory accordance, there occur some discrepancies in individual years which are larger than might be desired.

During the thirty-two years over which the modern observations extend, viz., from 1837 to 1869, the discrepancy between theory and observation, however, remains constantly less than 2″·5 of arc, excepting in two instances, viz., in the years 1839 and 1844, when the differences amount to 4″·5 of arc.

In the ancient observations only, made in the time of Maskelyne, rather larger differences occur, amounting in two instances to nearly 9″ of arc.

In order to test whether these discrepancies could be due to any imperfections in the theory, M. Leverrier has not shrunk from the immense labour of forming a second theory of the planet independent of the former, employing methods of interpolation instead of the analytical developments. I learn directly from M. Leverrier that this second investigation entirely confirms the accuracy of the first as regards the periodic inequalities, but that the secular variations of the excentricity and longitude of the perihelion are slightly changed. The effect of these changes is to bring the theory into very satisfactory accordance with the observations of Bradley, but the discrepancies above mentioned in the time of Maskelyne and in the modern observations still remain unaffected. The character of the discrepancies shown by the modern observations makes it very improbable that they can be due to any errors in the theory.

In fact, the error appears to change almost suddenly from a positive one of $4''\cdot4$ in 1839 to a negative one of $5''\cdot0$ in 1844, a variation of nearly $9''\cdot5$ in five years. Now no terms or group of terms due to the action of the planets could thus suddenly disturb the motion in five years, at a given epoch, and then leave the motion unaffected during the following twenty-five years. M. Leverrier is therefore inclined to think that the discrepancies arise from errors in the observations, notwithstanding that the Greenwich and Paris observations are mutually confirmatory of each other.

He suggests that it is possible that the varying aspects presented at different times by the ring may affect the accuracy of the observations of the planet, and may cause changes in the personal equations of the observers, which, from being rather large in the case of the ancient observations, have gone on diminishing as the system of observation has become more perfect.

One unlooked-for result follows from M. Leverrier's comparison of his theory of Saturn with the observations. Considering that the influence of Jupiter on the longitude of Saturn may amount to $3800''$, it might have been expected that from observations of the planet extending over 120 years the mass of Jupiter could have been determined with great precision. M. Leverrier has found, however, that this is not the case.

The equations of condition furnished by the comparison of the heliocentric longitudes of Saturn as deduced from theory and observation contain five unknown quantities, viz., the corrections of the assumed values of four elements and the correction of the assumed mass of Jupiter. On solving the equations with respect to the first four unknown quantities, the corrections to be applied to the elements are found to be greatly influenced by the intermediate correction of the mass of Jupiter, and after they have been substituted in the equations of condition, the coefficients of the correction of the mass of Jupiter in great part destroy each other, nowhere amounting in the resulting equations to one-tenth part of their values in the primitive equations. Hence these equations are insufficient to determine the mass of Jupiter with any precision. Consequently, in the formation of the Tables of Saturn, M. Leverrier has employed the value of the mass of Jupiter determined by the Astronomer-Royal from his observations of the 4th satellite.

The result which has just been noticed will appear to be less paradoxical if we consider that by far the larger part of the disturbances which Jupiter produces in the motion of Saturn is represented by the inequalities of long period which affect the mean longitude and the elements of the orbit. Now in the course of 120 years these inequalities have run through only a small part of their whole period, and therefore, during this interval, the greater part of their effects may be represented by applying changes to the several mean elements equal to the mean value of the corresponding long inequalities during the interval. It is only from the residual disturbances, which are comparatively small in amount, that any data can be obtained for the correction of the mass of Jupiter.

In the course of a few centuries, when these long inequalities, as well as the secular variations of the elements of Saturn, shall have had time to develop themselves, it will be possible to determine the mass of Jupiter from them with all desirable precision.

THE GLANDULAR ORIGIN OF CONTAGIOUS DISEASES[1]

TEN years ago, on the occasion of a Congress held in this town to discuss the question of the disposal of sewage, I had the honour, at the request of the committee of management, to deliver a lecture on the subject of the poisons of the spreading or communicable diseases. An abstract of the lecture was afterwards printed by the Congress, and for a time it gained a wide circulation.

The lecture of which I speak was based on a series of experimental researches which for some years previously I had been carrying out on the question of the mode of production and communication of those diseases which were anciently called plagues or pestilences, but which are now called communicable or spreading diseases.

I do not think that at a health congress like the present I can do better than recall attention to this same subject. The suppression of plagues is one of the grandest and supremest efforts of the sanitary reformer. The suppression can never be accomplished until all educated people understand the advances of modern science as to the cause and mode of origin and mode of propagation of these diseases. Whatever, therefore, tends to strike out light of knowledge on these subjects tends to elucidate, and though the spark lighted may go out again it may help to show the way.

I shall in this present effort first go back to the point where I stood when here ten years ago. I shall then briefly survey the course of thought that has sprung up between that time and the present. Next I shall state the position of my own views as influenced by the work of the past ten years. Lastly, I shall touch for a moment or two on the practical applications of theory to the development of practice.

Outline of the Glandular Theory.

From my researches previous to the year 1867, and which formed the subject-matter of my previous lecture here, I had discovered that the fluids secreted during various stages of disease in some forms of communicable disease could be made to propagate disease. A portion of secreted fluid taken from a patient of Mr. Spencer Wells, a patient who was suffering from surgical fever following upon the operation of ovariotomy, had been made to produce a definite form of fever in an inferior animal by being simply brought into contact with the peritoneal surface of the animal. The secretion from the peritoneum of the affected animal was shown by further experiments to have the power of inducing the same order of phenomena of disease in other similar animals, and through four generations of animals the phenomena were repeated. These were the first experiments in which this class of phenomena of disease by direct propagation and repropagation were produced synthetically. They have since been performed and modified in many ways, and the origination of them has been assigned to different experimentalists, but I am entitled to say they were the first of the kind; they were carried out in the years 1864-65, and they were communicated to the Association of Medical Officers of Health in the year 1865.

During the same course of research I made an attempt to separate the poisonous matter from the poisonous secretion, and in one attempt of the sort I believed myself to have been successful. Certainly I separated a substance which was exceedingly poisonous in its action, and which, after the manner of an alkaloid, combined with acids producing salts which were not only themselves poisonous but which reproduced poisonous secretion. In the lecture delivered in this place in 1867 specimens of these salts and of the substance from which they were derived were placed on the table.

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[1] A Theory as to the Natural or Glandular Origin of the Contagious Diseases. Address by the president, Benjamin W. Richardson, M.D., F.R.S., at the Sanitary Congress, Leamington, October 3.

To the poisonous substance, that is to say, to the base of the poisonous matter of the communicable diseases, I gave originally the name of *septine*, and I classified all diseases that are induced by such substance, *septinous diseases*. Before this period the diseases had been named *zymotic*, under the idea that they were connected with a process of zymosis or fermentation. They are still commonly known by that name. Since the name I suggested was given to them, they have been called septic diseases, and the term septicæmia has been brought into use in relation to them. I am of opinion, with all respect, that the word septine, as applied to the basic poisonous matter, and the term septinous, as applied to the phenomena, are the two simplest and best terms we can employ.

As the inquiries which led up to the experiments with septine progressed, I was led to form a view as to the nature of the poisonous base and as to its mode of origin. As I have already said it seemed to me to be an alkaloid, or chemical substance resembling in physical properties morphine, strychnine, and other bodies of that class, but derived, not like them from vegetable, but from animal organic matter. The difficulty in proving this lay in finding a reason for the various effects of the septinous material. If it were a common base like that which I suspected I had found, why should it not always produce the same form of septinous disease? Why should it, on the other hand, produce, as we know it does, many kinds of disease, each having a certain general likeness to the others, but each at the same time different in many important details, as different, for example, as small-pox is from scarlet fever, or measles from hydrophobia?

The difficulty in this way suggested led me to reflect on the connection which might exist between the bases of the different secretions of the animal body and the matter of septine. Each secretion yields some organic product; the gastric secretion pepsine, the salivary secretion ptyaline, and so on, and each secretion plays a different part in function although the organic bases of them all may present a general similitude of construction.

Thereupon I was led to the conception that the secretions of the animal body are in fact the sources of the septinous diseases, and that the various septinous diseases are, in fact, all of glandular origin; that in every case of disease the poison producing it is nothing more and nothing less than a modified form of one or other secretion.

In the lecture of 1867 delivered here a sketch was supplied of the number of diseases which affect the human family. They were stated to be about two hundred and fourteen in number, that is to say, when we classify the symptoms together so as to make them into great groups to which we can give specific names, we may reckon up two hundred and fourteen such groups of diseases. Amongst these groups I described one group as depending for its cause on the action of organic poisons.

The diseases produced by the organic poisons were classed as follows:—

Small Pox.
Measles.
Scarlet Fever.
Diphtheria.
Typhus Fever.
Typhoid Fever.
Erysipelas.
Hospital Fever.
Puerperal Fever (or the fever which occurs to women in child-bed).
Cholera.
Yellow Fever.
Ague.
Glanders.
Boil and Carbuncle.
Infectious Ophthalmia.

On the nature of the organic poisons which produce the diseases I urged the following points:—

(*a*) That in every case the poisons are in themselves specific. Each poison has a specific property, always bringing out the same disease through countless ages. From the time when man was first attacked by them, on to the present time, I have no doubt that each of the communicable diseases has been developed from, and has depended upon one specific poison.

(*b*) That the organic poisons are inodorous, have no smell whatever, and that no communicable disease ever depends upon the mere gases of decomposition of organic matter.

(*c*) That as regards the organic poisons themselves and their physical properties the great type of them all is represented by the poison of any venomous snake. If we puncture the poison bag, there exudes from it a fluid substance that contains the poison. If we gently dry that down, it becomes a darkish, somewhat powdery, half-glistening mass. It is the type of all the poisons which produce disease.

(*d*) That the special poisons are separable, and that I had separated one of them, namely, the poison of hospital fever. This is a secretion formed in the wound of a person suffering from surgical injury, and as it could be obtained in large quantities, it had been specially selected for the purpose of experiment. The poison, when obtained in large quantities, could be evaporated to the form of an extract or syrup, and produced, when dried, a substance resembling closely the snake poison. It admitted of being pulverised, and when introduced into the wound of a healthy animal, it produced symptoms similar to those of the patient from which it was taken.

A specimen of the poison of hospital fever, so prepared was shown. It was extracted from the fluid of the peritoneal cavity of a lady who had been operated upon for ovarian disease by Mr. Spencer Wells.

(*e*) That the poison thus obtained may be introduced into the body in various ways; that communicated to an animal, it will give to the body of that animal the same poisonous property as was possessed by the poisonous substance first introduced; the poison, that is to say, could be passed on, and made to affect another animal, and so through a series of subjects.

(*f*) That in the course of some diseases, these poisons are separated by nature in an almost pure state. This is singularly the case with regard to the poison of small-pox. The poison of small-pox may escape from the surface of the body, in an early stage, as a very fine vapour, and in that way communicate disease. It may be communicated in a fluid form, as we know when we use it by inoculation. In a dry state, as in the scale of a small-pox patient, it is innocuous till it comes into contact with the water or with the fluids of the body; then it becomes poisonous.

(*g*) That the poisons will probably dry solid. In the solid state they are inert, but they are capable of reabsorbing water apparently after any lapse of time, and of regaining their activity.

(*h*) That they admit of being charged with water almost to any degree; but that as we progress in charging them with water, and diluting them with water, they entirely lose their active power. This accounts for a fact which was observed by the famous Dr. Fordyce in the last century. At that time inoculation for small-pox was the rule; and Dr. Fordyce thought, "if he diluted the poison he would produce a milder form of disease." In fact, he was aiming in this way to produce what Jenner afterwards did produce by vaccination, namely, a modified small-pox. He took the poison of small-pox, mixed it with water, and refined it to a considerable extent, and he inoculated patients with the diluted solution. He then found out the fact—that, up to a given point, dilution made no difference, the poison always producing the disease; but beyond that certain point of dilution there was no disease at all produced by the solution—not even a milder disease. This was in accordance with my experiments, from which I found that the organic poisons retain their activity up to a given point of solution, and beyond that the water renders them inert. Through their extreme capacity for becoming watery, they lose their activity altogether.

(*i*) That the poisons are transferable also by the vapour of water, and in this way may escape from the living body. So long as a person is affected with these poisons, and is giving off vapour at a certain temperature, he is poisonous. The poison is distributed by the vapour, and the vapour is diffused in what I might almost call invisible spray. The poisons are mechanically carried with the vapour, and the vapour from the affected person may be absorbed by the healthy person. But as soon as the body is dead, the vaporisation having ceased, or a reverse process having been set up,—that is to say, there being a condensation of vapour as there sometimes is on the dead body,—the poisons are no longer infectious in the ordinary sense of the word.

(*j*) That the poisons are harmless in their dry state, but commence to resume their activity in water. In order to ensure their continuous action, they need certain temperatures—certain degrees of heat; that in this respect one poison often differs materially from another; and that this marks out on the surface of the earth a specific range for some poisons. For instance, the poison of typhus fever is probably volatile, and condenses with difficulty, with the result that it only lives at a given low temperature, and that at a certain degree north of the equatorial line, the disease ceases. There are other poisons which require a greater degree of heat for their distribution, of which the poison of yellow fever is an example. If yellow fever be brought from a hot country to one of our northern ports, it will not live. It may linger for a few days, but as a rule, it will not extend.

(*k*) That the poisons are all capable of being destroyed by various means. They are all destroyed, as already said, by extreme dilution. They are all destroyed also by heat and by oxidising agents. The mere exposure of them to moist oxygen destroys them rapidly. The exposure of them to ozonised, or electric oxygen, destroys them even more rapidly than ordinary moist oxygen. Exposure of them to chlorine is instantaneous destruction to them. Exposure to iodine is nearly as effective, and if the iodine can be diffused equally, it is as destructive as chlorine. Exposure to bromine leads to the same result. Exposure to nitrous acid has the same kind of effect, but not in so marked a degree. Exposure to sulphurous acid likewise produces destruction.

(*l*) That snake poison is destroyed by sunlight, and that the destruction does not depend upon the temperature. That bright sunlight is probably one of the means for destroying the organic poisons.

(*m*) That almost all the organic poisons are preservable by cold, and that, in fact, there is no limit to the preservation of them by extreme cold. The poisons are preserved also by many antiseptics. Sulphur, creasote, and arsenic, hold these organic poisons in preservation, so that they preserve their active properties.

(*n*) That some of the poisons are only poisonous during certain stages of their decomposition; with regard to the disease called hospital-fever, there is perhaps only one certain stage when the secretions really contain the poison. There is a certain given stage in the process of the manufacture of the poisons when the secretions change, and at that point the poisonous matter becomes innocuous.

(*o*) That in considering the development of these poisons it is a common error to suppose that they multiply from a germ as offspring multiply from parents, but that what occurs is this:—Each particle of any one of these poisons brought into contact either with the blood of the living animal or with certain secretions of the living animal, possesses the property of turning the albuminous part of that same blood or that same secretion into substance like itself. The process of change is catalytic. It is a change by which a body is transformed by the presence of some other body which does not itself undergo change. The multiplication of the poison thus takes place through the force of secretion of the person affected, not through the propagation of germ from germ. For instance if poison producing contagious ophthalmia be passed from the eye of one person into the eye of another, presently there is a free secretion. The secretion soon is profuse and is affected by catalysis from the poison. If the inoculation has been deep the whole animal will be affected; if it has not been deep only the eye will at first be affected. It is not that the particle of poison has propagated a new particle, but it is that the natural secretion of the eyeball has come in contact with a speck of poisonous matter, and immediately at that point where the speck of poison was there is a change in the secretion. This process widens the circle, more secretion pours out and more poison is produced, and the increase goes on until in the end the whole body of the animal may become affected by absorption into the body from the injured surface of poisonous matter: ultimately, *i.e.*, the poison is absorbed into the blood.

(*p*) That as a general rule the human body furnishes all the poisons that the human body suffers from, that is to say, there is a progression of poison from one body to another, and that ordinary secretions may change and become poisonous without previous infection. This, I showed, had been remarkably brought out in the case of puerperal poison, where a secretion from the hand of the accoucheur had produced puerperal fever. In the case of peritonitis, or inflammation of the peritoneum, there is a secretion which may be carried on the hand of a healthy person and reproduce the disease. Typhus may be produced by the overcrowding of persons in a room through the vaporisation of organic matter at a low temperature. Thus we may have springing up *de novo* an organic poison which afterwards, on being introduced into one particular body, becomes increased by the secretions of that body.

(*q*) That as regards the mode in which the organic poisons may be transmitted, they may travel in each of three ways. They may travel by means of sewage as dry solid matter; and all the poisons do this constantly. They may be wafted in the air, or carried by means of linen saturated with the secretions of patients and dried. Again, they may travel in water or in water suspended in the form of vapour.

(*r*) That the mode of the entrance of organic poison into the body, when they enter by contact, varies with the different poisons, the character of the poison changing the mode of its introduction. The poisons of measles, scarlet fever, and typhus are inhaled. The poisons of small-pox, diphtheria, glanders, erysipelas, hospital-fever, and ophthalmia, require direct contact. The poisons of cholera, yellow fever, and typhoid fever seem always to be swallowed poisons, and may be called, specifically, the poisons of sewage, and therefore mostly travel in a fluid form. They may, nevertheless, travel for short distances as fine dust, and they may travel in water in the form of vapour.

The thought that the poisons of the various spreading diseases are poisonous secretions, and nothing more, came naturally out of my researches. I realised, as it seemed to me, that all these spreading and communicable diseases spring out of the living animal body. That they are as distinctly the offspring of living animals as real progeny are, and that to look to outside sources for them, to look to vegetative growth for them, for example, or seedling, is merely to ignore the basic facts which lie obviously before us for lesson and learning. As well suppose that procreation of animals is due to an external vegetable product or other product dissevered altogether in its origin from the animal as that the poison which creates disease of a communicable kind is in such manner dissevered as to its origin.

Another thought which occurred to me in the course of my labours, and which I expressed in those earliest records of them, has relation to the force by which the poisons of the various diseases are developed and thrown off. It is well known that the production of the poisons in a living body infected by one of them is limited in respect to duration of time of production even when the body lives and recovers. This fact seemed to me to prove to demonstration that the poison itself is produced by the affected body, and is determined in its production by some natural function of the body or of some part of it. On the basis of my theory, that the poison in every case is a modified secretion, this view of the force of production of the secretion is easily accepted as in accordance with natural law. The force of production is the force of secretion, and so long as the secretion continues changed in character, so long it is thrown off as a poisonous secretion. But so soon as the modification of secretion which rendered it poisonous is stopped, so soon the secretion, flowing onward as before, is rendered innocuous, that is to say, no longer poisonous. If this were not the case, there is no reason, as far as I could see, why, in every instance of infection the infected person should not die. Endow the poison itself with independent forces of life and of reproduction, give to it a distinct reproductive life of its own; then why should it ever cease to reproduce? Why should it not in every case continue to increase within the infected body indefinitely until it kills the body, and why should any one ever recover? But consider the poison as a part only of the animal body itself, a substance to be eliminated from the body by natural methods and then the process of removal of the poisonous condition comes into the natural course of events, and recovery is a natural process, unless some unusual conditions occur to interrupt the natural course.

We see in a common nasal catarrh the outline of this scheme. There is first a dryness of the secreting surface, with reflex nervous irritation and much nervous depression and disturbance thereupon in the circulation of the blood. After a time there is a copious secretion from the nostrils, which continues until the disturbed nervous balance is brought back to steady natural action. At that time the excess of secretion is checked, and nothing more is left than the local effects of hardened secretion or scale due to the desquamation caused by the excessive previous action. In outline this is really the natural course of every epidemic disease, with the exception that the secretion of a catarrh is not definitely proved to be a contagious secretion. I believe it may be so, and sometimes is so; but I need not press the point. The illustration is adduced merely to show that the course of the disease is from within outwards, and that it is checked in its course by restoration of internal natural function. If catarrh were produced by some external vital agency, reproductive in character, lighting upon the nasal tract; if it were due to the colonisation of the nasal tract by an army of foreign invaders which settled there and began and continued to replenish and multiply, when would the catarrh cease? It would, as far as I can see, continue, until by destruction of parts and continued abstraction of secretion and extension of mischief over a wider tract of surface, it killed inevitably.

A catarrh, according to my view, as it was originally expressed, is typical of all the diseases which run a given course, and are called spreading diseases. It springs up constantly from external atmospheric variations; it runs a given course; it subsides. It is an epidemic, and it would be a true contagious epidemic if the matter secreted from the nasal cavity and the conjunctiva were not so innocuous. As I have hinted already, I believe it may be contagious. I am quite sure that many times in my life I have taken catarrh by coming near to a person who was affected by it, but whether this contagion is sympathetic or toxic, I am not able to define. On these intimate relations I shall have more to say on a future page.

The Germ Theory.

In the ten years that have passed since the time named, another hypothesis in reference to the spreading diseases and in relation to their origin from particular poisons has been brought prominently forward. Owing chiefly to the simple name which has been given to this hypothesis, and the commonness of the analogies on which it is based, it has gained much popular favour—I need hardly say that I refer to the so-called germ theory of disease.

This hypothesis has been most prominent for eight or ten years, but it is really a very old speculation indeed, perhaps one of the oldest in medicine. It has its root in the fancy of the analogy that as seed cast on the ground yields, or may yield, a certain harvest after its kind, as a field or garden plot may become fertilised by vegetable seeds or germs which may come to it borne by the atmosphere or by other modes of conveyance, so the body may be infected with germs of disease, which germs, being received in the body as a field for their reception, may increase and multiply in the body, and by their presence excite the phenomena which particularise all the special diseases of a communicable kind.

In modern times Dr. Grove, late of Wandsworth, was the first to advocate this hypothesis, and I need not tell a learned assembly like the present that it has been most energetically advocated more recently by many of the ablest foreign and English men of science. In the course of the discussions and of the researches which have been conducted on this subject much knowledge has, I am sure, been gained in the domain of natural history, and much interesting discussion for history has been written on the origin of some forms of life. But I protest that the attempt to connect this knowledge with the phenomena of the various communicable diseases, so as to suggest, or, as some do, to assert, that the diseases in question arise from germs, and that the person affected with a contagious disease has been fertilised like a piece of ploughed land or virgin soil by a crop of germs, and that in turn he is the soil in which another crop is being produced by the independent increasing and multiplying of the germs in him, I protest, I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive. What most astounds me is that men who are conversant with the practice of physic, who are treating diseases of a communicable kind every day, should for a moment connect such a hypothesis with the phenomena they have under their observation. Does any one of them believe that hydrophobia is from a germ, that syphilis is from a germ, or other diseases I need not specify?

It is suggested by some advocates of the germ speculation that the cause of the communicable diseases is after the manner of the putrefaction of dead organic matter. Does any physician who thinks as he observes, see anything like a general rule of putrefactive change in the contagious diseases? He may of course see local decompositions of secretions and of blood itself in the course of any of the diseases, but these he knows are all secondary results, while he may see and constantly does see all the diseases running their course without any sign whatever of the kind. Nay, in regard to one disease—cholera—he may, as I have done, see it run its fatal course and leave the dead bodies as loth to decompose as if they had been embalmed. Again, does any physician, who thinks as he observes, fail to see that the first symptoms of every one of the contagious diseases are purely nervous symptoms, that they indicate nervous irritation, and that the particular local injuries which occur are not primary at all, but are dependent on special nervous change modifying nutrition at the part. Between a boil or abscess and a pustule of small-pox what is the difference except in degree of purulent matter formed at one point of formation of matter?

There is nothing whatever in fact in the clinical history of plagues that connects them with the hypothesis of an origin from germs produced without the body and entering it to fertilise it and create a decomposition. When I say there is nothing, I mean there is nothing except the analogy of which I have spoken above, and even that breaks down, for the analogy of the fertilisation of a field by seed means always a definite process of fructification and of results from it; whereas in the history of epidemic plagues there is no such definition.

The germ hypothesis fails, however, on other grounds than the clinical. If it were true that living germs possessing an independent growth and vitality enter the animal body, that every disease of a communicable kind is due to its own external living germ, and that the germs continue to multiply and increase by an independent action of their own; if this be indeed true, why do the germs after a certain time cease to multiply and allow the sick person to recover? Why do they not go on multiplying until the person is infested in every part and fatally stricken? Who would get well from a disease due to living self-propagating contagions? Again, who, if the hypothesis were true, would escape fertilisation? A general fertilising diffusion of self-propagating matter in minute invisible form entering the body as the air may enter could hardly be expected to select a small minority of a population, and if it did so at the first, why should it do so when it had seized upon many centres in which it could increase. But the history of all the communicable diseases shows that each epidemic affects individuals individually at different periods in the course of the epidemic according, as a rule, to exposure to the infected, and that the period of the disease is limited by a development and a course rendered in certain periods of time.

I need hardly add in objecting to this germ hypothesis, because the fact is admitted, that not only has no one ever seen a germ of disease, but that no one has ever traced any order of germination in relation to any of the communicable diseases. When a really living self-propagating thing goes through its phases of life and action, like, for example, the yeast growth, we can trace it through its course of action on organic substances, and can study its effects, the changes it produces and the products of such changes. In the epidemic diseases we have no such guidance, no trace of it. Their phenomena, indeed, are opposed to the idea of the self-action of a foreign vital material.

Later Observations on the Glandular Theory.

I turn again to a brief review of the glandular theory of the origin of the contagious diseases, and of the advances I have made in support of that theory during the period of the past ten years.

In that time I have seen no reason to change my views on the subject of the glandular origin of the communicable diseases. On the contrary, every new observation has tended to confirm it and to make as I think the demonstration of its truth the more definite.

In continuance of observation I have noted that the number of the distinctly communicable diseases is closely related with the number of secretions. The poison of hydrophobia is from the salivary secretion; of diphtheria from the mucous glands of the throat; of scarlet fever I believe from the lymphatic glandular secretion; of glanders from the mucous secretion of the nasal surface; of typhoid from the mucous glands of the intestinal surface; and so on. In some instances the blood itself is infected, and the corpuscular matter becomes the seat of the catalytic change.

A second point which has occurred to me is that the matter or particle which sets up the poisonous action, instead of being living matter, is matter actually dead, and that its effect for evil depends, in fact, upon its being dead. I mean that the dead particles of organic matter in contact with living are the cause of the physical change which transforms the new particles of secretion into poisonous particles as they are brought up to the infected surface to be influenced by the infection.

On the ground that the poisons are always of glandular origin I have been led to the conclusion that under certain influences affecting glandular action the poisons may be made to originate directly through nervous impression without the necessary intervention of an infecting particle. In many epidemics it is common to see a number of examples of the prevailing disease the origin of which is traceable only to fear or anxiety. We call these nervous cases, and we try to define them as such and as distinct from cases due to contagion of a direct kind. But the symptoms are the same as those which follow actual contagion, and in epidemics of cholera they take even a fatal character. My theory explains fully the reason of this. It indicates that an extreme nervous impression acts on the glandular nervous supply, paralyses the glandular function, and thereupon produces the same phenomena as is produced in other instances by the action of a specific poison.

The theory in this manner accounts for the origin of an epidemic disease from an impression made on the nervous system without the direct contact of poisonous matter, as well as for the after-propagation of the disease by distribution of poisonous particles when that is communicated from an infected to a healthy person. It accounts equally well for the production of disease and of a poisonous glandular product under conditions of starvation and cold, by which the nervous tension is reduced. Again, it accounts for the production of disease and of a poisonous glandular secretion under special atmospherical conditions in which the activity of the atmospheric oxygen is reduced in sustaining power.

It has occurred to me further, as a result of the study of the action of the poisonous particles, that when they are brought into contact with the secreting surface, their action towards the body at large is, in the first instance, directly on the nervous fibre. The poisons act in the first instance as irritants on the peripheral nervous surface, and their effect may, I believe, extend particle by particle, as by diffusion, through the whole length of the nervous cord to the nervous centre. I have no doubt this is what slowly takes place in hydrophobia. I believe this is what takes place in diphtheria when paralysis is the sequel of the acute symptoms of the malady. I believe the same mode of progression of the poisonous influence is what happens after inoculation with matter of small-pox, that the severe nervous symptoms which mark the onset of that disease are due to the extensive injury inflicted on the nervous organisation, and that the diffusion of the eruption over so wide a surface of the skin and mucous membrane is the reflex on the peripheral nervous surface from the nervous centres.

It is worthy of special notice in connection with this part of my subject, that in the communicable diseases attended with an eruption on the skin or nervous surface, the eruption, as a rule, takes a circular form. If it be a point of vascular blush, a patechial spot, it is a rounded spot; if it be a pustule it is rounded; if it be a more diffused rash it commences in centres which are rounded points. This appearance is an indication of nervous injury. The rounded surface is the radius of injury done to the nervous supply of that part. It is a paralysis of the centre of nervous distribution over the affected part. My researches on the influence of extreme cold on nervous unction is strikingly illustrative of this fact. They suggest that the nervous impressions sent from the centres to the peripheral surface when they reach the peripheral surface, as on the surface of the skin, spread out like circling waves, just, in fact, as water spreads out in circles on a pool when a stone is made to impinge on the surface.

Some other recent investigations on the mode of action of the poisons of the communicable diseases has led me to suspect the source of the symptom which is so common to most of them, and which is known as the attendant fever. The fever is of three kinds: primary, reactive, and remittent. The primary fever is that which precedes and attends the eruption of an eruptive disease. The reactive fever is that which succeeds the extreme collapse of an acutely-exhaustive disease like cholera, or an exposure to extreme cold. The remittent fever is that which succeeds upon an acute form of disease, and indicates either that there has been secondary absorption of matter from an abraded surface in contact with poisonous substance, or that some fibrinous or pustular matter has formed within the body, as it were, and become a new and permanent centre of infection.

The first of these forms of fever is, I believe, due to the impression on the nervous centres by the poison in the manner I have described above.

The second, the reactive fever, is, I believe, due to the same action as that which locally may be induced by extreme cold, viz., by an influx of blood into vessels that have been paralysed, and by a rapid radiation of heat from extensive surface of blood.

The third form of fever, the remittent, has an origin, I believe, specifically its own. I have found that pustular matter and all secretions containing fibrinous or cellular structure have the property, by their presence, of liberating oxygen from solution. This extends, as I have found, to blood charged with oxygen, and I am led to the inference that when there is an absorption of such matter into the circulation it causes an undue liberation of oxygen with a quicker combustion, a fever which lasts until the exciting matter is itself destroyed and eliminated, and which does not recur until there is re-absorption of more of the exciting agent. In this physiological mode I should explain all the phenomena of the remittent attack; the cold stage incident to the absorption of the exciting matter; the hot stage incident to the period when, by its presence, the exciting matter is setting free excess of oxygen; the sweating stage when, by rapid elimination through the sweat glands, the equilibrium of temperature is restored.

The study of the glandular theory of the communicable diseases has suggested to me another thought which observation of the diseases fully confirms, viz., that these diseases, like all which have their root in nervous derangement, present a distinct heredity. The impression of disease made on a nervous centre is transmitted. There can be no doubt as to transmission of tendency to particular communicable diseases. Any physician in full practice can find any amount of evidence on the fact by simple natural inquiry. Typhoid fever is clearly a disease possessing hereditary transmissible quality. Diphtheria is the same. Scarlet fever is the same, and small-pox I should suspect was once almost universally so characterised. These facts alone, one of them alone, is sufficient to stamp the origin of the communicable diseases as from the animal body itself. It is certainly one of the best of proofs of the truth of the theory of the glandular origin of the poisons. It will be seen at once by those who look with sufficient patience, that the mode of connection of the diseases in hereditary line is the same as that which connects hereditary qualities of every kind, physical type, mental type, all else that binds many individualities into one family.

Lastly, the study of the glandular theory of the communicable diseases enables me to offer the the most rational explanation of the phenomenon of non-recurrence of the diseases after they have once attacked a person susceptible to them. It is well understood that, as a rule, a person who has been affected by a communicable disease is not affected a second time. To this rule there are many exceptions, but on the whole it holds good. On my theory the reason of the phenomenon is simple enough. They who are susceptible are born with a nervous impression tending to the production of a glandular secretion easily changed into poisonous secretion under the direct action of contact with poisonous matter, or even under the influence of a central nervous depression, whereby the glandular function is deranged. But when such a person has passed through the ordeal the tendency, for a time at least, disappears owing to the complete modification of glandular function that has been induced, to the free elimination that has been established, and probably to the change in the nervous matter itself that has resulted from organic modification. Hence the organism becomes susceptible for a time, and if the tendency be not intense that time may mean the whole of the life. Indeed as life advances and nervous susceptibilities derived directly from ancestry lapse into individual self-sustained susceptibilities, these tendencies to disease subside as a general fact, and lose their activity if not their existence.

I turn, in conclusion, to consider for a moment from the view of the glandular theory of the communicable diseases, the practice that is suggested for the suppression of the plagues of mankind.

It will be seen at once that on this point nothing can be wider than the distance between the idea of contagium as a living self-productive thing, reproductive and independent, and the theory of the production of the contagium in each affected person by the force of production of his own secretion. The latest and one of the ablest advocates of contagium vivum, Dr. W. Roberts, says, respecting contagium:—"We know of nothing that exhibits the phenomena of growth and multiplication except a thing possessed of life."

I admit that readily, but my argument is that the process of secretion is a process of life, and that this living process, perverted as I have described, is amply sufficient to account for the production of all the poisons that exist to cause the communicable diseases, that it accounts for the number of these diseases, and more, that it accounts for the limitation of the number.

The Glandular Theory in its Application to Practical Sanitation.

The practical usefulness of the theory, however, consists in its direct application. If the contagium vivum view be true, if the air around us is charged with invisible germs which come from whence we know not, which have unlimited power to fertilise, which need never cease to fertilise and multiply, what hope is there for the skill of man to overcome these hidden foes? Why on some occasion may not a plague spread over the whole world, and destroy its life universally?

My theory presents an altogether different aspect. I say to living men and women, it is you who are the producers of the communicable diseases, or if it be not you yourselves it is one of your lower earthmates in creation, some domestic animal that shares with you the power of producing a poisonous secretion and of giving a hereditary stamp of production to such poisonous product. I look on the man or animal affected with a contagious disease as one precisely, for the time, in the position of the cobra or other animal that naturally secretes a poison, and recognising this fact I see at once that the danger is all but limited to the person affected.

Isolate that person from the rest of mankind, take care that his secretions, volatile, fluid, or solid, do not come into contact with the secretions of susceptible healthy persons, and the danger is over. With the recovery of that person, that is to say, with restoration in him of a natural secretive process, the poison is destroyed; or should he unfortunately die, then with the death of his power to produce further secretion the danger is over, unless from his dead body some of the poison formed before the death be actually carried away to infect. In a word, if my theory be true, we sanitarians have complete mastery over the diffusion of the poisons of all the communicable diseases. We have but to keep steadily in view that the producing and reproducing power is in the affected body, and we can, even with our present knowledge, all but completely limit the action to the propagating power of that body—its power, I mean, of secretion and diffusion of secretion.

Beyond this, if the theory be true, we must expect, as we reduce the communicable diseases of one generation to reduce the tendency to them in the next generation, so that in time the heredity to particular spreading disease shall be thoroughly wiped out.

The theory suggests a profitable line of research on the subject of the production and reproduction of some of the poisons by the inferior animals and their transmission in that course to man. It brings all the inferior animals, in respect to their health and comfort, under our especial human care, not only for their sakes, but for our own self-preservation.[1]

Finally, the theory suggests to those who are engaged in treating diseases of a communicable kind the best means of arresting the progress of a communicable disease even when the phenomena of it have been developed in an individual. It leads us physicians to take a precise view, in each such case, of the nervous and glandular processes that are out of the natural order of work; it suggests to us to seek for remedies amongst chemical agents which affect special secretions; and it shows us how to place the sick under such conditions that the secondary absorption of their own poisonous secretions,—that deep absorption which, according to my experience, is the actual cause of death in the great majority of cases of contagious disease,—may be avoided.

In every direction, in fine, in prevention and in cure, the glandular theory of the origin of the communicable diseases opens practical work and hopeful work.

I have for some time past sought for a favourable opportunity of once more putting forward this theory of the natural origin and cause of the communicable diseases of men and animals. The present is opportune to the fullest degree, and therefore I have seized on it. I am too earnest after search of truth for its own sake, too certain that in science everything false must fall, and everything true must remain, to feel any sense of anxiety as to the fate of my simple theory, by the side of the doctrine of a living contagium. If my doctrine be as true as I believe it to be, it will live, whatever force be arrayed against it. If it be not true, I would be of the first to welcome its end, and to hail the ascendency of what is absolutely provable and certain on the momentous questions that have occupied our attention.

Meantime, I know I could not do a better thing for my own views than submit them once more to the public eye through the audience which has now so attentively listened to the argument.

NOTES

THERE has been a great deal of talk during the last few days, by prominent public men, on the advantages of some equivalent for university education for all the people, an education, too, in which science would be allotted a just place. Last week a Nottingham, the Earl of Carnarvon and Mr. Gladstone said much that was at least true on the advantages of an institution such as that newly founded at Nottingham, and each from his own standpoint lauded the advantages of wide culture for all classes.

Both Mr. Gladstone and Mr. Forster on Tuesday at Bradford seemed distinctly to approve of the movement for creating Owens College a University, and the only difficulty now seems to be the question of power to grant degrees. But surely those who are so eager on the latter point forget to distinguish between the shadow and the substance; the question of degrees will no doubt settle itself after the University has been established. Still we hardly sympathise with the trade-mark view of degrees propounded by Dr. Appleton in the *Times*. Bass's or Allsopp's label is imitated because their ales have a high and no doubt well-deserved reputation. But there were good ales before the names of either of the Burton brewers were heard of; there is the fine old Oxford ale, for instance, which, to judge by the public taste, has been improved upon by its new Burton rivals. Mr. Forster, however, we must say, seemed to think Oxford deserving of a word of praise for its present activity. Mr. Forster's address at Bradford was no mere essay on the beauty of culture, but the weighty utterance of a "practical" man who is forced to confess that he daily feels the immense disadvantage of having had no early training in science. He produced himself, in fact, as a practical comment on Sir John Lubbock's previous advocacy of the introduction of science into elementary schools. "His ignorance of science," he said, "his want of having been taught elementary laws of science when a boy, he felt every hour of his life, and it was too late now to learn. Science, if learnt at all, must be learnt in boyhood, and it was really disgraceful that in this civilised country, in this intellectual age, any one should be brought up in ignorance of the laws of nature, upon the breaking or keeping of which depended our happiness, our lives, and almost everything that relates to us. What a loss of pleasure, and what a different world the outside world of nature would be to him, if he could look around and understand the meaning of the various forces which were at work; and there was no doubt that a boy, even at an elementary school, if he learnt the elements there and went on afterwards, would get that kind of knowledge of the laws of science that it would become easy to him. There was a great talk about the dead languages. He was not going to say anything against them. Latin was almost a necessity to a man of culture, and Greek was of use; but why should nature, which spoke to us in so many ways, be a dead language to us? And therefore, if it came to this question—Whether we were to have classes on special subjects in elementary schools, classes for grammar, predicates, and a great many long words which he hoped nobody would examine him in, or for science—he certainly should go in favour of science." These are weighty words coming from a man of Mr. Forster's experience and "common sense," and indeed make us hope that things are progressing, and that we shall not now have long to wait before science is introduced not only into colleges, but into schools of all grades. Mr. Forster concluded by admitting that the German workers were superior to ours in the fact that they added to practical training scientific knowledge, and that he saw no reason why in secondary and even university education voluntary efforts should not be seconded by State aid.

M. YVON VILLARCEAU has been appointed "Administrateur Provisoire" of the Paris Observatory by an order of the Minister for Public Instruction, dated Saturday last. M. Villarceau held a similar office after the death of Delaunay, before the reappointment of Leverrier. Nothing has been said yet as to the appointment of a successor.

AT the Guy's Hospital *conversazione*, on Monday evening, a new government filter, invented by Major Crease, was shown, which reduced strong tea and infusions of logwood to clear tasteless water. The nature of the filtering material is not made known.

THE white whale, which was brought from America and placed in a tank (50 feet by 25) of fresh water in Westminster

Aquarium last Wednesday week, unfortunately died on Saturday morning. In the course of the first few hours after being put in the water the whole of the skin, piece by piece, peeled off, and after this the whale appeared to be more comfortable, fed well, and adopted a less restless style of swim. The change in its condition in two days was remarkable. In consequence of its journey it had been for twelve days without food, and it was on arrival so thin that the spinous processes formed a ridge two or three inches high along the back. In two days, however, feeding only on eels, it had regained its normal appearance, but, as we say, expired on Saturday morning. A post mortem examination was made by Prof. Flower and Prof. Garrod, assisted by Dr. Bond, of Westminster Hospital and Mr. Henry Lee. Everything was in a healthy condition except the lungs, which had quite lost elasticity, and in which inflammation had evidently been set up some time. Plastic pneumonia was the cause of death. The stomach, notwithstanding the twelve days' fast, had been working naturally, and some partly-digested eels were found. There was abundance of healthy-looking flesh in all parts of the body, which was not expected. The skeleton is to be exhibited at the Aquarium, and the viscera and brain have been presented to the College of Surgeons. The specimen was a partly-grown female Beluga or White Whale, nine feet six inches long. Prof. Wyman, of Harvard, published a description of one he dissected in the *Boston Journal of Natural History*, vol. vii., giving it Lesson's name, *Beluga borealis*. In Europe it is generally called *Delphinapterus leucas* (Pallas), the generic name being that given by Lacépède in 1804. Some of the daily papers have unfairly commented on the whale being put in fresh water. If the writers had taken the trouble to turn to "White Whale" in Bell, they would have found this statement: "It is abundant in Hudson's Bay, Davis Straits, and the Arctic Ocean generally. . . . It seems partial to large rivers; in America it ascends the River St. Lawrence as far as Quebec, and in Asia Schrenk and Nordmann state that it goes far up the River Amur." It is stated that one was kept in fresh water in New York for three years, fed on eels. The weight of the brain of this Westminster specimen has been ascertained by Prof. Flower to be 63 oz., an unusually high amount in proportion to the size of the animal. One peculiarity of this whale is that all the cervical vertebræ are separate. Several details of practical importance with reference to the carriage of large cetaceans have been learnt from this experiment, among the most important of which is the fact that unless the water-tank, in which it seems most reasonable to suppose that they would best travel, is sufficiently large to allow of the tail being brought well into action the creature is certain to be drowned from inability to reach the surface that it may breathe. Considering the difficulties attending the enterprise it is surprising how the animal arrived in this country without a trace of injury; and that inflammation of the lungs should have been the cause of death in an aquatic species was equally little to have been expected.

Prof. Quincke, the successor of Kirchhoff at Heidelberg, is now in this country inspecting the various laboratories and collections of apparatus.

The long-talked of Conference of Librarians commenced its meetings on Tuesday at the London Institution under the presidency of Mr. Winter Jones, librarian of the British Museum. Several papers were read on Tuesday and yesterday bearing on the organisation and utility of libraries, and we trust that the multitude who have for one purpose or another to deal with books and libraries will reap much benefit and decrease of worry by this congress of library officials. We would strongly commend to the attention of the members of the congress the letter published last week, from Prof. Mallet, of Virginia, advocating the organisation of a staff of searchers in connection with all our great libraries. Even the most devoted laboratory-worker must sometimes consult books, and it is desirable that this may be done with a minimum of waste of energy and time.

The Birmingham and Midland Institute was opened for this session, on Monday, by Prof. Tyndall, who gave an interesting address which we are glad to see is printed at full length in the *Times*.

Advices have been received from the Howgate Arctic schooner *Florence*, dated Cape Breton, August 8, up to which time the vessel had had a very comfortable and satisfactory passage. The arrangements for the accommodation of the naturalist and meteorologist prove to be quite ample and satisfactory, and already collections of natural history of some interest have been made.

Our Paris correspondent writes that important news has been received from M. de Brazza, the leader of the expedition to the Ogové, West Africa. Brazza writes from Doumé, a village beside one of the numerous cataracts of the river, in 0° 16′ S., and 13° 20′ E. The river is stated to flow from the south for a considerable distance, when it turns southward at or across the equator. The natives inform Brazza that the Ogové stretches a long way eastwards, and it is thought possible that it may come from some interior lake. Brazza seems to think that the Libumba, an affluent of the right bank of the Congo, may be also connected with the Ogové. As we hinted last week, it seems probable, since Stanley's discovery, that the Congo and Ogové are connected in some way.

The sea-coast branch of the United States Fish Commission has been at work for some time. The steam tug *Speedwell*, a powerful vessel of 300 tons, commenced operations at Salem, Massachusetts, about August 1. Unexpectedly rich results were obtained in that vicinity, embracing not only many rare forms of animal life, but much of practical importance to the fisheries. Several places were found abounding in fish previously unknown to the fishermen of Gloucester and Marblehead. Flounders of marketable size in immense numbers were taken of a species (*Glyptocephalus cynoglossus*) previously entirely unknown on the American coast. Leaving Salem on August 19, it arrived at Halifax on Wednesday the 22nd, trawling and dredging the greater part of the way. In the course of this journey many new animals were collected of much interest to naturalists, among them several species of Greenland fish hitherto never detected south of that country.

In Guido Cora's *Cosmos*, vol. iv. No. vi., we have an original chart of the Bay of Assab, accompanied by an elaborate description of the bay, the islands, and the adjacent continent, together with sailing directions. It appears to be somewhat better than the Red Sea Chart issued by the English Admiralty, but probably a drawing on a larger scale by Moresby or others is lying at the Hydrographical Office. The bay is on the African Coast, and is about forty miles from Perim Island, at the mouth of the Red Sea, and the same distance from Mocha. The most interesting point of this chart is that an area of some four miles by one mile and a-half is claimed for Italy.

M. Herpin, an old professor of mathematics and cosmography, is about to publish, through Baudry of Paris, an astronomical dictionary, quite a novelty in French scientific literature, since the astronomical part of the great Encyclopædia was published at the end of the last century.

An International Congress of Botany and Horticulture will be held at Paris during the International Exhibition next year. The Congress will open on August 16, 1878, and will last a week.

The Cunard steamer *Abyssinia*, which arrived at Queenstown on Sunday, experienced fearful weather from the 22nd to 27th ult.—gales from west, north-west, to north. On the 25th, lat. 45·38 N., long. 41·56 W., she met a cyclone from north, and was hove to for twenty-seven hours. This is believed to be a cyclone which recently started from the American coast and which thus vanished in the ocean.

THE geological survey of Brazil, which has been in progress for several years under the direction of Prof. C. F. Hartt, formerly of Vassar and Cornell Universities, United States, was lately for a short time threatened with suspension, but the proposal was countermanded and increased strength given to the commission after an investigation of all the circumstances. The temporary stoppage of operations was used advantageously by Prof. Hartt in placing the collections made by him in good order, and his parties have again entered the field in prosecution of their objects. Among the more important results so far accomplished by the survey has been the discovery of the existence in Brazil of the silurian, Devonian, carboniferous, triassic, Jurassic, cretaceous, and post-tertiary formations, all of them furnishing well-characterised fossils in great variety, and of which large numbers have been collected by the commission for its investigation, and for purposes of distribution in Brazil and of exchange with foreign establishments. So far no well-defined tertiary has been found to exist in Brazil. The survey has also been very successful in its ethnological researches, especially among the kitchen-middens of Santa Catharina, Paraná, Sao Paulo, Bahia, and the Amazonas, the results of which have been announced in part, although much of interest yet remains to be published. The researches in the coral reefs have been made the occasion of securing numbers of marine animals, all of which add to the resources of the survey. In connection with other operations, numerous photographs of scenery, of geological structure, and of the native races, have been taken.

THE death of Dr. Abraham Sager, an eminent anatomist and physiologist of the United States, took place on the 6th of August last. Dr. Sager, in 1837, was placed in charge of the botanical and zoological departments of the Michigan Geological Survey, and embraced this and subsequent opportunities to make large collections, which are now the property of the Michigan University. His investigations into the embryology and development of the tailed batrachians have added much to our knowledge of those forms.

THE bust of Sir Thomas Stamford Raffles, F.R.S., first president of the Zoological Society of London, has been placed in the new lion house of the Society's Gardens.

THE following foreign works have been sent us by Messrs. Williams and Norgate:—"Die kinetische Theorie der Gase," by Dr. Oskar Emil Meyer (Breslau); "Christian Gottfried Ehrenberg," by Johannes Hanstein (Bonn); "Phenomènes physiques de la Phonation," by J. Gadarret (Paris); "Ergebnisse physikalischer Forschung," by Dr. C. Bohn (Leipzig); "Physiologische Methodik," by Dr. Richard Gschiedlen (Braunschweig); "Synopsis Rubarum Germaniæ," by Dr. W. O. Focke (Bremen); "Lehrbuch der Analysis," by Rudolph Lipschitz: vol. I. (Bonn).

DR. F. A. FOREL, of Geneva, an energetic advocate of the doctrine of evolution, in an article published in the August number of the *Archives des Sciences physiques et naturelles*, proposes the application of natural selection for successfully healing certain diseases of silkworms, and also for rendering the European species of vines proof against the attacks of phylloxera. In the first matter experiments have already been made to a certain extent, and have been crowned with perfect success; in the case of vines the experiments are still to be made. The September number of the same journal, which is unusually bulky, is entirely devoted to a detailed biography of Auguste de la Rive, who died on November 23, 1873, at the age of seventy-two years.

WE have received a letter from Dr. Emil Bessels with reference to the *Polaris* observations (NATURE, vol. xvi. p. 358), and have much satisfaction in learning that it is proposed to revise the averages of the barometrical and thermometrical observations of the *Polaris* Arctic Expedition, these having been somewhat hastily prepared and published from a desire to have the report out before the expedition to the North Polar regions sailed from England.

A TELEGRAM from New York, September 30, states that the American Consul at St. John's (Newfoundland) has purchased from a seaman who was wrecked in Hudson's Bay, two spoons supposed to be relics of the Franklin Expedition, one of them being marked "J. G. F." It is said that Esquimaux living in the neighbourhood of Repulse Bay got them from a native chief, at whose camp the original owner, a white man, had died of scurvy. This statement does not seem quite consistent with the known facts as to the fate of the Franklin Expedition; moreover, we are not aware that "J. G. F." are Franklin's initials.

THE Museum of the Royal College of Surgeons of England has received as a present from the Hon. Charles P. F. Berkeley, the skeleton of a crocodile 15 feet 9 inches in length, which was shot by that gentleman last winter near Hagar Silsilis, in Egypt.

THE Swiss *Bundesrath* announces that the construction of the St. Gothard Tunnel is proceeding with increasing rapidity, and will probably be completed within three years.

THE seventh number (1877) of the *Bulletin* of the Belgian Academy of Sciences, contains a valuable paper, by M. C. Lagrange, "On the Influence of the Form of Bodies on their Attraction." This question, very incidentally treated by Brück, is thoroughly discussed by M. Lagrange, who arrives at some important conclusions. Discussing the attraction exercised by a body of irregular forms on a point situated at different distances from the centre of inertia of the body, and in different positions relatively to its axis of maximum and minimum inertia, the author proves that the attraction is directed to the centre of inertia only when the point is situated on one of the principal axes of inertia of the body; and that, at equal distances the attraction reaches its maximum when the point is on the axis of minimum inertia, and inversely, this maximum exceeding, and the minimum being less than, the attraction which would have been exercised were the whole mass of the body concentrated in its centre. Further, the author discusses the attraction exercised on a moving point, and arrives at the conclusion that the point, while attracted to the centre, will also receive an angular motion around the latter. Finally, he discusses the reciprocal attraction of two free bodies of irregular form, and, after having shown when the attraction will reach a maximum and a minimum, he proves also that the attraction will communicate to both bodies a rotatory motion, tending to bring into coincidence their axes of minimum inertia. In the two last paragraphs of his paper, M. Lagrange briefly notices the applications the principles he establishes may have in explaining the rotatory motion of the sun, as well as in accounting for crystallisation, further researches, not yet published, having enabled the author to account for the formation of different crystalline systems in a way which makes the whole question a problem of rational mechanics. The memoir is spoken of in very high terms by MM. Van der Mensbrugghe, Catalan, and De Tilly, who analysed it by order of the Academy.

NO. VIII. of the *Bulletin* of the United States National Museum consists of an "Index to the names which have been applied to the subdivisions of the class Brachiopoda," by Mr. W. H. Dall.

THE author of the work in the "Naturkräfte" series, on "Die Insecten," noticed in NATURE for September 13 (p. 418) is not Dr. Georg Mayr, but Prof. Vitus Graber, of Czernowitz University, the well-known author of numerous interesting monographs on insect anatomy and histology.

THE additions to the Zoological Society's Gardens during the past week include a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk; a Chimpanzee (*Troglodytes niger*) from West Africa, a Leonine Monkey (*Macacus leoninus*) from Arracan, a White-fronted Capuchin (*Cebus albifrons*), a Laughing Gull (*Larus atricilla*) from South America, deposited; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*), from Australia, presented by Mr. G. S. S. Williams; two Red-backed Shrikes (*Lanius collurio*), European, presented by Capt. F. H. Sahrn; two Spotted Turtle Doves (*Turtur suratensis*), bred in the Gardens.

INTRODUCTION AND SUCCESSION OF VERTEBRATE LIFE IN AMERICA[1]

III.

THE artiodactyles, or even-toed ungulates, are the most abundant of the larger mammals now living, and the group dates back at least to the lowest eocene. In every vigorous primitive type which was destined to survive many geological changes, there seems to have been a tendency to throw off lateral branches which became highly specialised and soon died out because they are unable to adapt themselves to new conditions. The narrow path of the persistent suilline type throughout the whole tertiary is strewn with the remains of such ambitious offshoots, while the typical pig, with an obstinacy never lost, has held on in spite of catastrophes and evolution, and still lives in America to-day. The genus *Platygonus* is represented by several species, one of which was very abundant in the post-tertiary of North America, and is apparently the last example of a side branch, before the American suillines culminate in existing peccaries. The feet in this species are more specialised than in the living forms, and approach some of the peculiar features of the ruminants; as, for example, a strong tendency to coalesce in the metapodial bones. The genus *Platygonus* became extinct in the post-tertiary, and the later and existing species are all true peccaries. No authenticated remains of the genera *Sus*, *Porcus*, *Phacochœrus*, or the allied *Hippotamus*, the Old World suillines, have been found in America, although several announcements to that effect have been made.

In the series of generic forms between the lower eocene *Eohyus* and the existing *Dicotyles*, which I have very briefly discussed, we have apparently the ancestral line ending in the typical American suillines. Although the demonstration is not yet as complete as in the lineage of the horse, this is not owing to want of material, but rather to the fact that the actual changes which transformed the early tertiary pig into the modern peccary were comparatively slight, so far as they are indicated in the skeletons preserved, while the lateral branches were so numerous as to confuse the line. It is clear, however, that from the close of the cretaceous to the post-tertiary the bunodont artiodactyles were especially abundant on this Continent, and only recently have approached extinction.

The selenodont division of the artiodactyles is a more interesting group, and so far as we now know, makes its first appearance in the upper eocene of the west, although forms apparently transitional between it and the bunodonts occur in the dinoceras beds, or middle eocene. The most pronounced selenodont in the upper eocene is the *Oromeryx*, which genus appears to be allied to the existing deer family, or *Cervidæ*, and if so is the oldest known representative of the group. These facts are important, as it has been supposed, until very recently, that our eocene contained no even-hoofed mammals.

A most interesting line, that leading to the camels and llamas, separates from the primitive selenodont branch in the eocene, probably through the genus *Parameryx*. In the miocene, we find in *Pæbotherium* and some nearly allied forms unmistakable indications that the cameloid type of ruminant had already become partially specialised, although there is a complete series of incisor teeth, and the metapodial bones are distinct. In the pliocene the camel tribe was, next to the horses, the most abundant of the larger mammals. The line is continued through the genus *Procamelus*, and perhaps others, and in this formation the incisors first begin to diminish, and the metapodials to unite. In the post-tertiary we have a true *Auchenia*, represented by several species, and others in South America, where the alpacas and llamas still survive. From the eocene almost to the present time North America has been the home of vast numbers of the *Camelidæ*, and there can be little doubt that they originated here and migrated to the Old World.

The deer family has representatives in the upper miocene of Europe, which contains fossils strongly resembling the fauna of our lower pliocene, a fact always to be borne in mind in comparing the horizon of any group in the two continents. Several species of *Cervidæ*, belonging to the genus *Cosoryx*, are known from the lower pliocene of the west, and all have very small antlers, divided into a single pair of tynes.

The proboscidians, which are now separated from the typical ungulates as a distinct order, make their first appearance in North America in the lower pliocene, where several species of mastodon have been found. This genus occurs also in the upper pliocene and in the post-tertiary, although some of the remains attributed to the latter are undoubtedly older. The pliocene species all have a band of enamel on the tusks, and some other peculiarities observed in the oldest mastodons of Europe, which are from essentially the same horizon. Two species of this genus have been found in South America, in connection with the remains of extinct llamas and horses. The genus *Elephas* is a later form, and has not yet been identified in this country below the upper pliocene, where one gigantic species was abundant. In the post-pliocene remains of this genus are numerous. The hairy mammoth of the Old World (*Elephas primigenius*) was once abundant in Alaska, and great numbers of its bones are now preserved in the frozen cliffs of that region. This species does not appear to have extended east of the Rocky Mountains, or South of Columbia River, but was replaced there by the American elephant, which preferred a milder climate. Remains of the latter have been met with in Canada, throughout the United States, and in Mexico. The last of the American mastodons and elephants became extinct in the post-tertiary.

Perhaps the most remarkable mammals yet found in America are the *Tillodontia*, which are comparatively abundant in the lower and middle eocene. These animals seem to combine the characters of several different groups, viz., the carnivores, ungulates, and rodents. In the genus *Tillotherium*, the type of the order, and of the family *Tillotheridæ*, the skull resembles that of the bears; the molar teeth are of the ungulate type, while the large incisors are very similar to those of rodents. The skeleton resembles that of the carnivores.

We now come to the highest group of mammals, the primates, which includes the lemurs, the apes, and man. This order has a great antiquity, and even at the base of the eocene we find it represented by several genera belonging to the lower forms of the group. In considering these interesting fossils it is important to have in mind that the lemurs, which are usually regarded as primates, although at the bottom of the scale, are only found at the present day in Madagascar and the adjacent regions of the globe. All the American monkeys, moreover, belong to one group, much above the lemurs, while the Old World apes are higher still, and most nearly approach man.

In the lower eocene of New Mexico we find a few representatives of the earliest known primates, and among them are the genera *Lemuravus* and *Limnotherium*, each the type of a distinct family. These genera became very abundant in the middle eocene of the West, and with them are found many others, all, however, included in the two families *Lemuravidæ* and *Limnotheridæ*.

In the miocene lake basins of the West only a single species of the *Primates* has been identified with certainty. This was found in the oredoon beds of Nebraska and belongs to the genus *Laopithecus*, apparently related both to *Limnotheridæ* and to some existing South American monkeys. In the pliocene and post-pliocene of North America no remains of primates have yet been found.

In the post-pliocene deposits of the Brazilian caves remains of monkeys are numerous, and mainly belong to extinct species of *Callithrix*, *Cebus*, and *Jacchus*, all living South American genera. Only one extinct genus, *Protopithecus*, which embraced animals of large size, has been found in this particular fauna.

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[1] Abstract of a lecture delivered at the Nashville meeting of the American Association, August 30, by Prof. O. C. Marsh. Continued from p. 472

It is a noteworthy fact that no traces of any anthropoid apes, or indeed of any Old World monkeys, have yet been detected in America. Man, however, the highest of the primates, has left his bones and his works from the Arctic Circle to Patagonia. Most of these specimens are clearly post-tertiary, although there is considerable evidence pointing to the existence of man in our pliocene. All the remains yet discovered belong to the well-marked genus *Homo*, and apparently to a single species, at present represented by the American Indian.

In this rapid review of mammalian life in America, from its first known appearance in the trias down to the present time, I have endeavoured to state briefly the introduction and succession of the principal forms in each natural group. If time permitted, I might attempt the more difficult task of trying to indicate what relations these various groups may possibly bear to each other; what connection the ancient mammals of this continent have with the corresponding forms of the Old World; and, most important of all, what real progress mammalian life has here made since the beginning of the eocene. As it is, I can only say in summing up, that the marsupials are clearly the remnants of a very ancient fauna, which occupied this continent millions of years ago, and from which the other mammals were doubtless all derived, although the direct evidence of the transformation is wanting.

The relations of the American primates, extinct and recent, to those of the other hemisphere, offer an inviting topic, but it is not in my present province to discuss them in their most suggestive phases. As we have here the oldest and most generalised members of the group, so far as now known, we may justly claim America for the birth-place of the order. That the development did not continue here until it culminated in man, was due to causes which at present we can only surmise, although the genealogy of other surviving groups gives some data towards a solution. Why the Old World apes, when differentiated, did not come to the land of their earlier ancestry, is readily explained by the then intervening oceans, which likewise were a barrier to the return of the horse and rhinoceros.

Man, however, came; doubtless first across Behring's Straits; and at his advent became part of our fauna, as a mammal and primate. In these relations alone it is my purpose here to treat him. The evidence, as it stands to-day, although not conclusive, seems to place the first appearance of man in this country in the pliocene, and the best proof of this has been found on the Pacific coast. During several visits to that region many facts were brought to my knowledge which render this more than probable. Man at this time was a savage, and was doubtless forced by the great volcanic outbreaks to continue his migration. This was at first to the south, since mountain chains were barriers on the east. As the native horses of America were now all extinct, and as the early man did not bring the Old World animal with him, his migrations were slow. I believe, moreover, that his slow progress towards civilisation was in no small degree due to this same cause, the absence of the horse.

It is far from my intention to add to the many theories extant in regard to the early civilisations in this country, and their connections with the primitive inhabitants or the later Indians, but two or three facts have lately come to my knowledge which I think worth mentioning in this connection. On the Columbia River, I have found evidence of the former existence of inhabitants much superior to the Indians at present there, and of which no tradition remains. Among many stone carvings which I saw there were a number of heads which so strongly resemble those of apes that the likeness at once suggests itself. Whence came these sculptures, and by whom were they made? Another fact that has interested me very much is the strong resemblance between the skulls of the typical mound-builders of the Mississippi Valley and those of the Pueblo Indians. I had long been familiar with the former, and when I recently saw the latter it required the positive assurance of a friend who had himself collected them in New Mexico to convince me that they were not from the mounds. A third fact, and I leave man to the archæologists, on whose province I am even now trenching. In a large collection of mound-builders' pottery, over a thousand specimens, which I have recently examined with some care, I found many pieces of elaborate workmanship so nearly like the ancient water-jars from Peru that no one could fairly doubt that some intercourse had taken place between the widely-separated people that made them.

The oldest known remains of man on this continent differ in no important characters from the bones of the typical Indian, although in some minor details they indicate a much more primitive race. These early remains, some of which are true fossils, resemble much more closely the corresponding parts of the highest Old World apes, than do the latter our tertiary primates, or even the recent American monkeys. Various living and fossil forms of Old World primates fill up essentially the latter gap. The lesser gap between the primitive man of America and the anthropoid apes is partially closed by still lower forms of men, and doubtless also by higher apes, now extinct. Analogy, and many facts as well, indicate that this gap was smaller in the past. It certainly is becoming wider now with every generation, for the lowest races of men will soon become extinct, like the Tasmanians, and the highest apes cannot long survive. Hence the intermediate forms of the past, if any there were, become of still greater importance. For such missing links, we must look to the caves and later tertiary of Africa, which I regard as now the most promising field for exploration in the Old World. America, even in the tropics, can promise no such inducements to ambitious explorers. We have, however, an equally important field, if less attractive, in the cretaceous mammals, which must have left their remains somewhere on this continent. In these two directions, as I believe, lie the most important future discoveries in palæontology.

As a cause for many changes of structure in mammals during the tertiary and post-tertiary, I regard as the most potent, *natural selection*, in the broad sense in which that term is now used by American evolutionists. Under this head, I include not merely a Malthusian struggle for life among the animals themselves, but the equally important contest with the elements, and all surrounding nature. By changes in the environment, migrations are enforced, slowly in some cases, rapidly in others, and with change of locality must come adaptation to new conditions, or extinction. The life history of tertiary mammals illustrates this principle at every stage, and no other explanation meets the facts.

The real progress of mammalian life in America, from the beginning of the tertiary to the present, is well illustrated by the brain-growth, in which we have the key to many other changes. The earliest known tertiary mammals all had very small brains, and in some forms this organ was proportionally less than in certain reptiles. There was a gradual increase in the size of the brain during this period, and it is interesting to find that this growth was mainly confined to the cerebral hemispheres, or higher portion of the brain. In most groups of mammals the brain has gradually become more convoluted, and thus increased in quality as well as quantity. In some, also, the cerebellum and olfactory lobes, the lower parts of the brain, have even diminished in size. In the long struggle for existence during tertiary time the big brains won, then as now; and the increasing power thus gained rendered useless many structures inherited from primitive ancestors, but no longer adapted to new conditions.

Another of the interesting changes in mammals during tertiary time was in the teeth, which were gradually modified with other parts of the structure. The primitive form of tooth was clearly a cone, and all others are derived from this. All classes of vertebrates below mammals, namely, fishes, amphibians, reptiles, and birds, have conical teeth, if any, or some simple modification of this form. The edentates and cetaceans with teeth retain this type, except the zeuglodonts, which approach the dentition of aquatic carnivores. In the higher mammals the incisors and canines retain the conical shape, and the premolars have only in part been transformed. The latter gradually change to the more complicated molar pattern, and hence are not reduced molars, but transition forms from the cone to more complex types. Most of the early tertiary mammals had forty-four teeth, and in the oldest forms the premolars were all unlike the molars, while the crowns were short, covered with enamel, and without cement. Each stage of progress in the differentiation of the animal was, as a rule, marked by a change in the teeth, one of the most common being the transfer, in form at least, of a premolar to the molar series, and a gradual lengthening of the crown. Hence it is often easy to decide from a fragment of a jaw, to what horizon of the tertiary it belongs. The fossil horses of this period, for example, gained a grinding tooth for each toe they lost, one in each epoch. In the single-toed existing horses all the premolars are like the molars, and the process is at an end. Other dental transformations are of equal interest, but this illustration must suffice.

The changes in the limbs and feet of mammals during the same period were quite as marked. The foot of the primitive mammal was doubtless plantigrade, and certainly five-toed. Many of the early tertiary forms show this feature, which is still seen in some existing forms. This generalised foot became modified by a gradual loss of the outer toes and increase in size of the central ones, the reduction proceeding according to systematic methods, differing in each group. Corresponding changes took place in the limb bones. One result was a great increase in speed, as the power was applied so as to act only in the plane of motion. The best effect of this specialisation is seen to-day in the horse and antelope, each representing a distinct group of ungulates with five-toed ancestors.

If the history of American mammals, as I have briefly sketched it, seems as a whole incomplete and unsatisfactory, we must remember that the genealogical tree of this class has its trunk and larger limbs concealed beneath the *débris* of mesozoic time, while its roots doubtless strike so deeply into the palæozoic that for the present they are lost. A decade or two hence we shall probably know something of the mammalian fauna of the cretaceous, and the earlier lineage of our existing mammals can then be traced with more certainty.

The results I have presented to you are mainly derived from personal observation, and since a large part of the higher vertebrate remains found in this country have passed through my hands, I am willing to assume full responsibility for my presentation of the subject.

For our present knowledge of the extinct mammals, birds, and reptiles of North America, science is especially indebted to Leidy, whose careful, conscientious work has laid a secure foundation for our vertebrate palæontology. The energy of Cope has brought to notice many strange forms, and greatly enlarged our literature. Agassiz, Owen, Wyman, Baird, Hitchcock, Deane, Emmons, Lea, Allen, Gibbes, Jefferson, DeKay, and Harlan deserve honourable mention in the history of this branch of science. The South American extinct vertebrates have been described by Lund, Owen, Burmeister, Gervais, Huxley, Flower, Desmarest, Aymard, Pictet, and Nodot Darwin and Wallace have likewise contributed valuable information on this subject, as they have on nearly all forms of life.

In this long history of ancient life I have said nothing of what life itself really is. And for the best of reasons, because I know nothing. Here at present our ignorance is dense, and yet we need not despair. Light, heat, electricity and magnetism, chemical affinity, and motion are now considered different forms of the same force; and the opinion is rapidly gaining ground that life, or vital force, is only another phase of the same power. Possibly the great mystery of life may thus be solved, but whether it be or not, a true faith in science admits no limit to its search for truth.

THE GERMAN ASSOCIATION AT MUNICH

THE fiftieth meeting of the German Association of Naturalists and Physicians began on September 17 by a large assembly of visitors in the old Town Hall at Munich. The meeting this year assumed quite a national character. Although in the programme its scientific character was principally considered, and pleasure trips, banquets, &c., had been reduced to the most modest proportions in comparison with former years, yet the aspect of the city of Munich, and of all the edifices that were in any way connected with the meeting, was a festive one. Some 2,000 visitors had arrived, and the Town Hall on the night of the 17th was crowded to suffocation. The authorities of the city gave a grand Keller-Fest in honour of the visitors on the 20th, which was attended by over 5,000 guests.

The first general meeting was opened by Prof. von Pettenkofer on the morning of the 18th inst. In a short address the professor announced that His Majesty the King had intended to send his royal greeting to the assembled men of science through H.R.H. Duke Carl Theodor, of Bavaria (brother to the Empress of Austria), but that the duke had suddenly been called to Dresden through the death of the dowager Queen of Saxony. In his absence His Majesty had intrusted the secretaries with this honourable message. After other congratulations Dr. von Pettenkofer delivered his inaugural address. He reminded the assembly that the present was a jubilee meeting, and then gave a retrospect of the growth of the Association since its foundation. The first meeting took place at Leipzig on September 18, 1822, when, following the invitation of Prof. Oken, twenty scientific men assembled and founded the Society. A paragraph of the statutes prescribed that the meetings should always begin on September 18, and should last several days. Under the political circumstances of that time and with the means of conveyance then existing the modest number of twenty members was considered a fair beginning. The next meeting occurred at Halle with thirty-four members, the third at Würzburg with thirty-six, the fourth at Frankfort-on-Maine with 110, the fifth at Dresden with 116, the sixth at Munich with 156, and the seventh at Berlin in 1828, when 464 members were present. The Association steadily increased and the meetings were held annually unless prevented by war or epidemics. The last meeting at Hamburg numbered over 2,000 members. Little by little a division of labour took place, and out of the seven original sections twenty-five have now resulted.

After speaking of the progress made by man as compared with the lower animals, Prof. Pettenkofer said—If knowledge is power, and nobody will doubt this, then amongst sciences natural science is certainly destined to play a great part, perhaps the greatest, in the history and culture of mankind. . . . Natural science has but to look for facts and truths, and need never busy itself about the immediate practical application of what has been found, because for them alone it deserves the sympathy of the entire civilised world, and the means necessary for its culture and development. No investment of capital bears higher interest. Finally, the speaker recalled the memory of Prof. Ludwig Lorenz Oken, the founder of the association and the author of the statutes which, with but a single and trifling exception are still in force to-day. He praised the patriotism of Oken, and regretted that he died before the reestablishment of the United German Empire.

At the end of the address, the assembly, at the request of Dr. von Pettenkofer, rose from their seats in honour of the memory of Oken.

Then followed the first scientific lecture, which was delivered by Prof. Waldeyer (Strassburg). He spoke on *Karl Ernst von Baer and his Influence on Natural Science*, giving an elaborate memoir of the late great naturalist, to whom we owe many of the bases of the present theory of evolution. Prof. Dr. Haeckel then delivered his address *On the Evolution Theory at the Present Time*, which we give elsewhere.

At the second general meeting, on the 20th, the choice of a place of meeting was made for next year, Cassel being selected, with Doctors Stilling and Gernau as secretaries. Duke Carl Theodor of Bavaria, himself an able ophthalmologist, took the chair in lieu of Dr. Pettenkofer, and again welcomed the assembly, in the name of the kingdom of Bavaria, in an interesting speech. Then followed the address of the eminent botanist, Prof. Dr. Nägeli, of Munich, "On the Limits of Natural Knowledge." He pointed out that many naturalists, when asked about the limits of natural knowledge, and thinking a solution by principles insufficient, simply reply that faith begins where knowledge ends. Humanity faces the whole of nature, masters new domains constantly by dint of meditation; the empire of knowledge thus always increases in extent, and that of faith decreases as constantly. But this solution does not satisfy our interest. We would wish to know particularly whether the limits of natural knowledge can be determined at all, and how far we can penetrate into nature. The solution of this question is determined by three conditions:—(1) By the condition and capacity of the investigating Self; (2) by the condition and accessibility of nature; and (3) by the demands which we make from knowledge. With regard to the first point, the undoubted fact is decisive that our power of thinking, in whatever condition it may be, can but give us nature as we perceive her with our five senses, and even this again not in her full extent and completeness, but only as far as we perceive her in the present. We see and hear only what is in the present; now as the organs of our senses are more or less sensitive for the one or the other perception, Darwin's ingenious idea that in nature only so many phenomena attained full development as were useful to the individual bearer, is fully justified; on the other hand it is very probable that many molecular forces exist of which we have no idea, simply because we cannot perceive them with our senses. The limited capacity of the I allows us only an extremely fragmentary knowledge of the universe. With reference to the second point, the condition and accessibility of nature, we can easily perceive the limit; for man it rests in space and time. The infinity of space and time, and its consequences, are insuperable for us, and nature is everywhere uninvestigable where she becomes eternal or infinite, and therefore she can never be *entirely* investigated. The naturalist must therefore always bear in mind that all his investigations are restricted to natural limits, otherwise he will lose himself in groundless fancies, and will arrive at absurd conclusions. The speaker then turned to the conceptions of the universe. The world which is known to us changes; if we follow this in the past and future we find, from a physical point of view, a state which approaches perfect rest more and more, without reaching it altogether. But if we suppose that in space worlds arise from worlds without end and perish again, then the successive states, according to the materialistic conception, are of the same value, while according to the philosophical conception they change their relative value by becoming more perfect. The one conception lets the world awaken from dead repose and return to it, the other condemns it to eternal repose. With regard to the extension of the universe in space, the thought that all material space must again and again have limits, leads us to the mathematical conclusion that our earth, just as it is now, reoccurs in infinite numbers in the universe. The speaker then passed to the third point, viz., the demands we make of knowledge. Our knowledge does not go further than to compare observed phenomena and judge of them with regard to others; we understand a phenomenon, understand its value with regard to other phenomena, if either we measure, count, or weigh it, or if we create it ourselves. It is in this latter manner that mathematical science is the product of our mind. The understanding of nature therefore rests in the recognition of the mathematical method in natural phenomena. As by the help of mathematics we understand only relative or quantitative differences, but not qualitative ones, because these cannot be compared, it follows that with regard to the latter scientific understanding is only possible separately within each single individual. Then Prof. Nägeli spoke against the opinion of those who divide nature into a material and a spiritual one, because no naturalist can avoid the conception of a causal connection of mind and body. The finite human mind is a double one; on the one hand it invents and puts the muscles into motion, on the other it contemplates, feels pleasure and pain, hate and love, and remembers. Even without this latter property, therefore, without consciousness, the world would have become world, man would have lived and taught, spoken and made music, but everything only mechanically—man would have been an automaton. Prof. Nägeli then passed from the domain of the mind to that of sensation, explaining that doubtless there was sensation in *all* molecular forces, the *same* sensations in the highest as well as in the lowest stages of organs, in the former only so much more vivid and refined than in the latter. If we understand spiritual life to be the mediator of cause and effect, then we find it everywhere. Du Bois-Reymond, who treated the same subject at Leipzig in 1872, finished his address with the words "Ignoramus et ignorabimus," but Prof. Nägeli ended *his* speech with the proud words—"We know, and we shall know if we are satisfied with human insight."

An address by Prof. Dr. Klebs, of Prague, followed "On the Changes in Medical Views during the Last Decades."

At the final meeting, on the 22nd instant, Prof. Rudolf Virchow gave an address "On the Liberty of Science in Modern State-life," which was received with loud acclamations of approval. After contrasting former with present times, Prof. Virchow said that the last few days had proved that now science enjoys full liberty. We must retain this possession, and must take care not to go too far. Moderation, the resignation of personal predilections, will be necessary to retain the present favourable conditions. The sum total of that which we may designate as true and real science, in the strictest sense of the word, and for which alone we may demand full scientific liberty, is a far more modest one than the domain of speculative expansion of problems and of presentiment. The speaker then in the most detailed and interesting manner drew the limit between speculative investigation on the one side and that which we have recognised as facts on the other. Prof. Virchow is ready to ask that everything which may be considered as a perfectly secured scientific truth, shall be admitted to the scientific treasure of the nation. If now we stand everywhere before reforms in education, and if for natural science a far-reaching consideration is claimed, it must first of all be perfectly clear to us *what* is to be comprised in this science and *what not*, and it cannot be left for the pedagogues to decide, as Prof Haeckel says it ought to be, whether the doctrine of evolution is to be comprised in the programme of education or not. If this doctrine is a scientific truth, and proved beyond doubt, then its admission to this educational programme *must* be demanded, unless we wish to make hypocrites of our teachers. But if it is completely proved it ought to be explained to every child in the schools, not only to the scientific man. The speaker then criticised somewhat severely Prof. Haeckel's theory of the plastidule soul and of the animated cell. As long as the undeniable proofs were wanting, he maintained, we ought, on the contrary, to ask our teachers *not* to teach the evolution doctrine. In the domain of the doctrine of evolution wise moderation is more necessary than anywhere else. For many years Harvey's maxim, "Omne vivum ex ovo" remained undenied; to-day we know for certain that the "omne" is incorrect. In the same way the "generatio æquivoca" may be true or not it certainly is not undeniably proved. In natural science belief and knowledge, *i.e.*, subjective and objective knowledge are united. The domain of dogmatic belief is lessened year after year in favour of objective knowledge which is based upon facts. But apart from the latter, subjective knowledge makes itself very prominent sometimes, and hallucinations and fancies are now and then hid beneath its cover. Anthropological investigations contradict directly the doctrine of evolution. The skulls found in the tombs of the oldest times show a far more human and a far less apish type than do a great many living heads, and we cannot suppose that only the highest-developed skulls of those periods have escaped destruction. Therefore, precaution, moderation, no overrating of our scientific power, for Bacon's "scientia est potentia" is only meant for true objective knowledge.]

Many papers of great scientific value were read in the various sections, and we hope to be able to refer to these in a future number.

THE PRESENT POSITION OF THE EVOLUTION THEORY[1]

On this festive day which unites us here for the opening of the fiftieth meeting of the Association of German Naturalists, universal science may justly point out its relation to the domains of our special investigations. On such a day the educated of all circles, who follow with vivid interest the astonishing progress of the investigation of nature are specially to ask what general results have been obtained for the entire domain of human education. If, therefore, to-day I comply with the honourable request addressed to me, and ask for your kind attention for a short time, I do not think that I can choose a more fitting subject for our common consideration than the relation of science as a whole to that branch of investigation which lies nearest to me, viz., the doctrine of evolution.

No other doctrine has so vividly claimed general attention for the last decade, no other affects our most important convictions so deeply, than the newly-risen doctrine of evolution and the monistic philosophy united with it. Because wholly and solely by this doctrine the "question of all questions" can be solved, the fundamental "question of the position of man in nature." As man himself is the measure of all things, thus naturally the last fundamental questions and the highest principles of all science must depend on the position which our advanced understanding of nature assigns in nature to man himself.

As you know, it is principally to Charles Darwin that the evolution theory of the present day owes this commanding position. Because it was he who, eighteen years ago, first broke through the rigid ice-cover of reigning prejudices, inspired by the same fundamental thought of a monistic development of the world, which a century ago moved our greatest thinkers and poets, Immanuel Kant and Wolfgang Goethe at their head. By the conception of his theory of selection—the doctrine of natural selection in the struggle for existence—Darwin could in particular give a firm foundation to the most important biological part of that doctrine, which had already appeared in the beginning of our century, viz., the theory of descent. In vain the older natural philosophy had then begun the fight for this theory; neither Lamarck and Geoffroy St. Hilaire in France, nor Oken and Schelling in Germany could obtain a victory for it. Just fifty years have now passed since Lorenz Oken began his academical lectures on the theory of evolution here at Munich, and it therefore becomes us here to-day to place a laurel wreath upon the tomb of this deep-sighted zoologist and inspired philosopher. It was Oken also who, in his enthusiasm for scientific unity, called together the first meeting of German naturalists at Jena in 1822, and to whom, for that reason alone, the thanks of this fiftieth assembly are due.

But the natural philosophy of that time could only draw up the general plan of construction and the first outline of the colossal edifice of the monistic theory of evolution; only the zealous and ant-like diligence of half the following century collected the building material for its execution. An immense literature and an admirable perfection of the methods of investigation now give the most brilliant proof of the astonishing progress of the empirical science of nature during that period. But of course the immeasurable widening of the field of empirical observation, and the special division of labour caused by this, often led to an unfortunate dispersion of powers; the higher object of the recognition of general laws was often entirely forgotten in the nearer interest in the observation of details.

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[1] "On the Evolution Theory of the Present Day in its Relation to Science in general." Address by Prof. Haeckel at the Munich Meeeting of the German Association.

Thus it could happen that while this strictly empirical investigation of nature was flourishing at its highest in the years 1830 to 1859, during thirty years, the two principal branches of real natural *history* started from totally different bases. In the history of the development of the earth the conviction gained ground more and more since 1830, the year of the publication of "Lyell's Principles of Geology," that our planet had neither been formed by a supernatural act of creation, nor had passed through a series of total revolutions of mystical origin, but that, on the contrary, a gradual and uninterrupted development had caused its natural formation step by step. On the other hand, in the history of development of the living inhabitants of the earth the old irrational myth remained in full force, according to which every single species of animals and plants, like man himself, had been created independently of one another, and that a series of such creations had followed each other without any genetic connection. The glaring contradiction of the two doctrines, of the natural development-theory of geologists, and of the supernatural creation myth of biologists, was only decided in favour of the former by Darwin in 1859. Since then we recognise clearly that the formation and change of forms of the living inhabitants of our globe follow the same great eternal laws of mechanical development as the earth itself and the whole world-system.

We need not to-day, as we were obliged to do fourteen years ago at the meeting of naturalists at Stettin, cite the reasons and proofs for Darwin's new theory of development. The recognition of its truth has since made its way in the most satisfactory manner. In that domain of natural investigation to which my own labours belong, in the wide field of the science of organic forms or *morphology*, it is already recognised everywhere as the most important basis. Comparative anatomy and the history of germs, systematic zoology and botany cannot to-day do without the theory of descent. Because only by its light the mysterious relations of the numberless organic forms amongst each other can be really explained, *i.e.*, reduced to mechanical causes. Their similarity results as the natural consequence of *inheritance* from common parental forms, their variation as the necessary effect of *adaptation* to different conditions of life. Only by the theory of descent can the facts of palæontology, of chorology, and of œcology, be explained in a way as simple as it is natural; only by this theory we understand the existence of the remarkable rudimentary organs, of the eyes which do not see, the wings which do not fly, the muscles which do not move—nothing but useless parts of the body, which refute in the most emphatic manner the old-fashioned *teleology;* because they prove in the clearest manner that the utility in the structure of organic forms is neither general nor perfect; that it is not the result of a plan of creation worked with an object in view, but necessarily caused by the accidental coincidence of mechanical causes.

Who, in the face of these overwhelming facts, still asks to-day for proofs of the theory of descent, proves by that only his own want of knowledge or reason. But it is utterly wrong to demand exact or indeed experimental proofs. This demand, which is so often heard, results from the widely-spread error that all natural science must be exact; all the other sciences are often confronted with this, under the name of "spiritual or pure sciences" (*Geisteswissenschaften*). Now in truth, only the smaller part of natural science is exact, viz., that part which can be proved *mathematically;* astronomy before all others, and higher mechanics in general; after these the greatest part of what remains of physics and chemistry, also a good part of physiology, but only a very small part of morphology. In this latter biological domain the phenomena are far too complicated and variable to allow of our applying the mathematical method at all. If indeed the demand for a foundation, which shall be as exact as possible, and mathematical if possible, stands good in principle for *all* sciences, it is yet quite impossible to carry this through in by far the greater part of the biological disciplines. Here, on the contrary, the historical and historico-philosophical method takes the place of the exact, mathematical, and physical one.

This applies to morphology before all others, because the scientific understanding of organic forms we obtain solely through the *history of their development.* The great progress of our time in this domain consists in our conceiving the meaning and object of the history of development in an infinitely wider sense than has been done before Darwin. Up to his time it meant only the history of the formation of the organic individual form, which to-day we call *history of the germ*, or *ontogeny.*

If the botanist followed the formation of the plant from the seed, the zoologist that of the animal from the ovum, they considered their morphological task accomplished by the perfect observation of the history of these germs. The greatest men in the domain of the history of evolution, Wolff, Baer, Remack, Schleiden, and the whole school of embryologists formed by them, understood by it, until a short time ago, the individual ontogeny exclusively. It is quite different to-day, when the mysteries of the wonderful history of germs confront us no longer as unintelligible riddles, but have clearly revealed their deep significance; because according to the laws of inheritance, the changes of form which the germ passes through in the shortest time, under our eyes, are a compressed and abbreviated repetition of the corresponding changes of form, which the ancestors of the organism in question have passed through in the course of many millions of years. If to-day we place a hen's egg into the breeding machine, and if twenty-one days later we see a little chicken creep from it, we no longer remain in mute astonishment at the wonderful changes which lead from the simple cell in the egg to the two-leaved gastrula, from this to the worm-shaped and skull-less germ and thence to further germ-forms, which on the whole show the organisation of a fish, an amphibian, a reptile, and only lastly that of a bird. On the contrary, we draw conclusions from this regarding the corresponding series of forms of the ancestors, which have led from the unicellular amœba to the parental form of the gastræa, and further on through the classes of worms, acephala, fishes, amphibia, reptiles, down to birds. The series of germ-forms of the chicken thus gives us a sketch of the series of its real ancestors.

Our biogenetic fundamental law gives the immediate causal connection which thus exists between the ontogeny of any organic individual form and the history of the forms of its ancestors in the following short phrase:—*The history of the germ is an extract from the history of its ancestors*, occasioned by the laws of inheritance. This *palingenetic* extract appears essentially disturbed only in case, through adaptation to the conditions of embryonal life, *cenogenetic* changes have taken place.

This phylogenetic interpretation of the ontogenetic phenomena is, up to the present, the only explanation of the latter. But it receives the most important confirmation and supplementation from the results of comparative anatomy and palæontology. It is of course impossible to prove this by an exact method or indeed an experiment, because all these biological disciplines are, according to the nature of the matter, *historical* and philosophical natural sciences. Their common object is the investigation of historical events, which happened in the course of many millions of years, long before the appearance of the human race on the surface of our youthful planet. The immediate and mathematically exact conception of these events is therefore altogether beyond the reach of possibility.

Only by the critical consideration of the *historical archives*, by a speculation which is just as circumspect as it is daring, an approximate understanding here becomes indirectly possible. Phylogeny uses these historical archives in the same manner and according to the same method as other historical disciplines do. Just as the historian, by the help of chronicles, biographies, and letters draws up a detailed representation of an event long past; as the archæologist by the study of inscriptions, pieces of sculpture, utensils, obtains the knowledge of the state of civilisation of a race long extinct; as the linguist by comparative investigation of all related living languages and their older written documents proves their development and origin from a common ancestral language; just in the same manner the naturalist of to-day, by the critical use of the phylogenetic archives, of comparative anatomy, ontogeny, and palæontology, arrives at an approximate understanding of the events which, in the course of unmeasured periods, have caused the change of forms in the organic life upon our globe.

The history of the parental forms of organisms, or *phylogeny*, can therefore be proved by an exact method or by experiment just as little as this is the case with her older and more favoured sister *geology*. But the high scientific value of the latter is nevertheless now generally acknowledged. Only the ignorant to-day smile incredulously at the explanation that the colossal mountain chains of the Alps, the snow-covered summits of which we see glistening in the far distance, are nothing else but the hardened deposits of the sea. The structure of these stratified mountains and the nature of the fossils they inclose do not admit of another explanation; and yet it cannot be proved in an exact way. In the same manner all geologists now unanimously suppose a certain systematic succession of the mountain strata, corresponding to their different ages; and yet this system of strata is nowhere perfectly present upon the earth. But our phylogenetic hypotheses may claim the same value as is given to these generally recognised geological hypotheses. The only difference is that the enormous structure of hypotheses in geology is far more perfect, simple, and easier to understand, than that of youthful phylogeny.

Thus these *historical sciences of nature*, geology and phylogeny, now form the uniting bond between the exact natural sciences on the one hand, and the historical sciences of the intellect, or pure sciences, on the other. The whole of biology, in particular systematic zoology and botany, are thus raised to the rank of a true natural *history*, an honourable title, which these sciences have borne long ago, but which they only now merit truly. If indeed to-day in many quarters, even in official ones, they are designated as "descriptive natural sciences," and opposed to the "explanatory" ones, this only shows what a false idea had hitherto been entertained of their true object. Since the "natural system" of organisms has been recognised as their *ancestral pedigree*, the living phylogeny of classes and species takes the place of dead descriptive systematics.

However highly we may estimate this enormous progress of morphology, yet it would not suffice by itself to explain the extraordinary effect of the evolution doctrine of to-day upon science in general. This, as you know, rests upon a single special deduction drawn from the theory of descent, upon its application to *man*. The very old question of the origin of our own race is by this theory solved for the first time in a natural scientific sense. If the theory of evolution is true at all, if there exists a natural phylogeny at all, then man also, the crown of creation, has resulted from the form *vertebrata*, from the class *mammalia*, from the sub-class *placentalia*, from the order *apes*. If Linnæus, in 1735, in his system of nature, already united man with apes and bats in the order of primates, if all following zoologists could not move him out of the class of mammalia, then this unanimously recognised systematic position can, phylogenetically, only be interpreted as descent from that class of animals.

All attempts to shake this most important deduction from the evolution doctrine are futile; it is vain to try to keep a particular exceptional position for man, by constructing for him a special line of ancestors, separated from those of the vertebrata. The phylogenetic archives of comparative anatomy, ontogeny, and palæontology, speak too distinctly in favour of an identical and uniform (*einheitlich*) descent of all vertebrata from a single common ancestral form, to permit of our having any doubts on this subject now. Not a single investigator and comparer of languages thinks it possible that languages as widely different as the German, Russian, Latin, Greek, and Indian languages have developed from different original languages. On the contrary, all linguists, by critical comparison of the structure and the development of these different languages, arrive unanimously at the conviction that they all have emanated from a single Aryan or Indo-Germanic mother language. Just in the same way all morphologists arrive at the firm conviction that all vertebrata, from the *amphioxus* upwards to man himself, all fishes, amphibia, reptiles, birds, and mammals descend originally from a single vertebrate ancestor; for we cannot imagine that all the different and highly-complicated conditions of life, which, through a long series of processes or stages of development, led to the typical formation of a vertebrate, have accidentally happened together more than once in the course of the earth's history.

For our consideration to-day only the general conception of the vertebrate-origin of man is of importance, we will not occupy our time with the single ancestral stages of our pedigree. I would only in passing point out that at least the principal stages of the same are now considered as firmly established, thanks to the excellent labours of our most illustrious morphologists, Gegenbaur and Huxley before all others. Of course it is still often supposed that thus, even to-day, only the origin of the human body is explained, but not that of our spiritual activity. In the face of this important objection we must remember, before all else, the physiological fact, that our intellectual life is inseparably united with the organisation of our central nervous system. The latter, however, is composed exactly like that of all higher vertebrata, and originates in exactly the same way. Also, according to Huxley's investigations, the differences between the structure of the brain of man and that of the higher apes are far less important than the corresponding differences between the higher and lower apes. Now as the function or work of each organ cannot be imagined without the organ itself, and as the function is everywhere developed along with the organ, we are forced to suppose that our psychical activity has developed slowly and gradually in connection with the phylogenetic development of our brain.

For the rest this highly significant "soul question" appears to us in quite a different light to-day from what it did twenty, yes, even ten, years ago. Whatever we may imagine to be the nature of the connection of soul and body, of mind and matter, so much results with perfect clearness from the evolution doctrine of to-day that at least all organic matter—if indeed not all matter—is, in a certain sense, animated. First of all, we have been taught by advanced microscopical investigation, that the anatomical elementary parts of organisms, the *cells*, universally possess individual animated life (*allgemein ein individuelles Seelenleben besitzen*). Since Schleiden founded, forty years ago at Jena, the highly-significant cell theory for the vegetable kingdom, and Schwann soon afterwards applied the same to the animal world, we universally ascribe to these microscopical life-beings an individual and independent life; they are the true "individuals of the first order," the "elementary organisms" of Brücke. The grand and highly fertile application which Virchow, in his "Cellular Pathology," made of the cell theory with regard to the entire domain of theoretical medicine, is indeed based upon his considering the cells no longer as the dead passive building stones of the organism, but as the living, active state citizens of the same.

This conception is finally confirmed by the study of infusoria, amœbæ, and other unicellular organisms, because here we find with the single cells, living in isolation, the same manifestation of soul-life, sensation, and conception, volition and motion, as with the higher animals, composed of many cells! Both in the case of these latter social cells, as well as in that of the former hermit-cells, the soul-life of the cell is tied to one and the same most important cell substance—*protoplasm*. We even see in the monera and other most simple organisms that single detached pieces of protoplasm possess motion and sensation, just like the whole cell. Accordingly, we must suppose that the cell-soul, the foundation of empirical psychology, is a compound itself, namely, the total result of the psychic activities of the protoplasm-molecules, which we shortly call plastidule. The plastidule-soul would therefore be the last factor of organic soul-life.

But has the evolution doctrine of the present day thus exhausted its psychological analysis? Not at all! On the contrary, we are taught by modern organic chemistry that the peculiar physical and chemical properties of an element, of *carbon*, in its complicated combination with other elements, cause the peculiar physiological properties of organic compounds, and before all others, of protoplasm. The monera, consisting exclusively of protoplasm, here form the bridge over the deep chasm between organic and anorganic nature. They show us how the simplest and oldest organisms must have originally sprung from anorganic carbon compounds. If therefore in spontaneous generation a certain number of carbon atoms unite with a number of atoms of hydrogen, oxygen, nitrogen, and sulphur to form the unity of a plastidule (or molecule of protoplasm), we must regard the plastidule-soul, *i.e.*, the total sum of its life-activities, as the necessary product of the forces of these united atoms. The sum of the central atomic forces we may call atom-soul in a consequentially monistic sense. By accidental meeting and varied combination of the constant and unchangeable atom-souls the diverse and highly variable plastidule-souls originate, the molecular factors of organic life.

Arrived at this most extreme psychological consequence of our monistic doctrine of evolution, we meet with those old conceptions of the animation of all matter, which already in the philosophy of Democritus, Spinoza, Bruno, Leibnitz, and Schopenhauer have found varied expression; because all soul-life can finally be reduced to the two elementary functions of *sensation* and *motion*, to their reciprocal action in reflex motion. The simple sensation of inclination and disinclination (*Lust und Unlust*), the simple form of motion, *attraction* and *repulsion*, these are the true elements out of which all soul-activity is built in infinitely varied and complicated combination. "The hating and loving of atoms," attraction and repulsion of molecules, motion and sensation of cells, and of the organisms composed of cells, the formation of thought, and the consciousness of man, these are only different stages of the universal psychological process of evolution.

The unity in the conception of the universe (or "monism") to which the new doctrine of evolution thus leads us, annuls the opposition which hitherto existed between the different dualistic world systems. It avoids the one-sidedness of materialism as well as that of spiritualism, it unites the practical idealism with the theoretical realism, it combines natural science with mental science (*Geisteswissenschaft*) to form an all-comprising uniform general or total science.

As thus we recognise the evolution doctrine of to-day to be a uniform and uniting cement of the most heterogeneous sciences, it gains the highest significance not only for the pure and theoretical but also for the practical and applied sciences. Neither practical medicine as an applied natural science, nor practical politics, jurisprudence and theology, in as far as they are parts of applied philosophy will in future be able to escape its influence. On the contrary we are convinced that it will prove, on all these domains, to be the most important lever of progressive knowledge as well as of ennobled civilisation in general. Now as the most important point of attack of the latter is the education of the young, the evolution doctrine will have to claim its just influence in the school as the most important means of education; here it will not be only tolerated, but it will become a ruling and guiding element.

If, finally, we are allowed to indicate, in a few words, at least the most important points of this relation, we may first of all lay stress upon the high significance of the genetic method in itself. Teachers as well as those they teach will contemplate each subject of their studies with infinitely greater interest and understanding, if, before all else, they ask themselves, "How did this originate? How did it develop itself?" Because in this question as to development the question as to the causes of facts is comprised; but after all it is always the recognition of the effecting causes, not the mere knowledge of facts, which satisfies the constant want of causalities of our mind. The recognition of common simple causes for the most varying and complicated phenomena leads to the simplification as well as to the deepening of our education and culture; only by causal conception dead knowledge becomes living science. Not the quantity of empirical knowledge, but the quality of its causal conception, is the true measure of the education of the mind.

How far the outlines of the general doctrine of evolution are now to be introduced into schools, in what succession its most important branches—cosmogony, geology, phylogeny of animals and plants, anthropogeny—are to be taught in the different classes, this we must leave to practical pedagogues to determine. But we believe that a far-reaching reform of education is unavoidable in this direction, and that it will be crowned with the most perfect success. How infinitely, for instance, the important teaching of languages will gain in educational value, if it is done comparatively and genetically! How the interest in physical geography will grow if it is genetically taught together with geology! How the tedious, dead systematics of the species of animals and plants will gain life and light if the two are explained as different branches of a common pedigree! And what a different conception we will, before all else, obtain of our own organism if we recognise it no longer as the fictitious likeness of an anthropomorphous creator, but in the clear daylight of phylogeny as the highest developed form of the animal kingdom; as an organism, which in the course of many millions of years has developed itself gradually from the line of vertebrate ancestors, and has far surpassed all its relatives in the struggle for existence!

As the doctrine of evolution will thus act in a fertilising and furthering way upon all branches of education, it will at the same time produce the consciousness of their monistic connection in the minds of both teachers and pupils. As historical natural science it will step as mediator and conciliator between the two opposed directions which to-day compete for power in the higher educational schools; on the one side the older, classical, historical, philosophical direction, on the other the newer, exact mathematical, physical direction. Both directions of education we think equally justified and equally indispensable; the human mind will only reach its full harmonious education, if both are equally taken into account. If formerly classical education was favoured too exclusively and one-sidedly, this has happened only too often recently with exact education. Both excesses the doctrine of evolution reduces to their proper measure, as it steps as a uniting bond between exact and classical science, between that of nature and that of the mind. Everywhere it teaches the living course of the connected, monistic, and uninterrupted development. Everywhere it shows to the zealous investigator new scientific aims beyond those already attained, and thus "gently leads the striving mind nearer and nearer to truth." The infinite perspective of progressive perfection which the doctrine of evolution thus opens before us is at the same time the best protest against the unfortunate "Ignorabimus," which it is obliged to hear now from many quarters, because nobody can predict what "limits of natural understanding" the human mind in the further course of its astonishing development will yet overstep in future!

By far the most important and most difficult demand which practical philosophy addresses to the evolution doctrine seems to be that of a new doctrine of morals (*Sittenlehre*). It is certain that afterwards, as before, the careful training of the moral character will remain the principal task of education. But up to the present the widest circles held the conviction firmly that this most important problem could only be solved in connection with certain ecclesiastical dogmas. Now as these dogmas, particularly in their union with very old myths of creation, directly contradict the principles of the doctrine of evolution, it was believed that through the latter religion and morals were endangered in the highest degree.

We consider this fear an erroneous one. It arises from the constant mixing up of the true and reasonable natural religion with the dogmatic, mythological church religion. The comparative history of religions, an important branch of anthropology, acquaints us with the great variety of external shells, in which the different people and times, according to their individual character and requirements, clothe religious thought. It shows us that the dogmatic teachings of church religions themselves are in a slow uninterrupted course of development. New churches and sects arise, old ones perish; at the best a certain form of creed lasts a few thousand years, an insignificantly small lapse of time in the æon-series of geological periods. Finally we are also taught by the comparative history of culture, how little true morality is necessarily united with a certain ecclesiastical creed. Often the greatest coarseness and decay of morals go hand in hand with the absolute power of an almighty church. We need only think of the middle ages! On the other hand we see the highest stage of moral perfection attained by men who have separated themselves from all ecclesiastical beliefs.

Independently of all church creeds, the germ of a true religion of nature lives in the breast of every man; it is connected inseparably with the noblest features of human existence itself. Its highest command is *love*, the restriction of our natural egotism in favour of our fellow men, for the benefit of human society, of which we are the members. This natural moral law is far older than all church religion. It has developed from the social instincts of animals. With animals of very different classes, particularly with mammals, birds and insects, we find its beginnings. According to the laws of association and of division of labour, many individuals here unite to form the higher community, called a state or hive. Its existence is necessarily connected with the reciprocal action of the members of the community, and with the sacrifices they make to the whole at the expense of their egotism. The consciousness of this necessity, *the feeling of duty*, is nothing else but a social instinct. But instinct is always a psychic habit, which, acquired originally by adaptation, has become inheritable in the course of generations, and finally appears as innate.

To convince ourselves of the admirable power of the animal feeling of duty, we need only destroy an ant-hill. There we at once see in the midst of destruction thousands of zealous state citizens occupied not with the salvation of their own dear lives, but with the protection of the cherished community to which they belong. Courageous warriors of the ant state set themselves up in powerful defence against our interfering finger; those that tend the young save the so-called "ants' eggs," the beloved pupæ, upon which rests the future of the state; diligent workers at once begin with undaunted courage to clear away the *débris*, and to construct new dwellings. The admirable organisation of these ants, of bees and other social animals, have originally developed from the crudest beginnings, just in the same manner as did our own human civilisation.

Even those most tender and most beautiful features of the human mind, which we principally glorify in poetry, we find already formed in the animal kingdom. Have the intense maternal love of the lioness, the touching matrimonial love of parrots ("inseparables"), the sacrificing faithfulness of the dog not been proverbial for ages? The most noble feelings of compassion and love, which determine actions, are here as with man, nothing but ennobled instincts. In connection with this conception, the ethics of the evolution doctrine need not look for new maxims, but reduce the very old commands of duty to their natural scientific base. Long before the origin of all church religion these natural commands of duty ruled the lawful living together of mankind as well as of social animals. Church religion ought to profit by this significant principle, not to combat it; for the future does not belong to that theology which conducts a fruitless battle against the victorious doctrine of evolution, but to that one which takes possession of it, recognises and uses it.

Therefore, far from fearing a shaking of all valid moral laws, and an obnoxious emancipation of egotism by the influence of the evolution doctrine upon our religious convictions, we, on the contrary, expect from it a reasonable confirmation of the moral doctrine on the unshakable basis of firm natural laws; for with the clear conception of our true position in nature, anthropogeny opens to us at the same time an insight into the necessity of our very old precepts of social duty. Henceforth practical philosophy and pedagogics will, like theoretical general science, deduce their most important maxims, not from supposed revelations, but from the natural principles of the doctrine of evolution. This victory of monism over dualism opens to us the most hopeful prospect for an infinite progress of our moral as well as of our intellectual development. In this sense we greet the evolution doctrine of to-day, as recently founded by Darwin, as the most important impulse of the whole of our pure and applied sciences.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Oxford University Commissioners having decided upon suspending two out of the three fellowships now vacant at All Souls' College, only one fellow will be elected in November.

BRISTOL.—The introductory lectures at the opening of University College, Bristol, commence on the 9th inst. Prof. Letts opens the chemistry class on the 10th with an address on "Old and New Views on the Nature of Matter," and Prof. S. P. Thompson the class of experimental physics on the 12th, with an address on "The Methods of Physical Science." The evening classes will be opened about a week later. Mr. J. F. Main, B.A., D.Sc., Scholar of Trinity College, Cambridge, has been appointed Lecturer in Mathematics and Applied Mechanics.

LEEDS.—The Yorkshire College, as it is now called, has published a neat calendar of about 100 pages in the orthodox grey colour characteristic of similar publications. The calendar contains all needful information on the organisation and business of the College, which now possesses six chairs, representing the main departments of science and literature, besides a chair of civil and mechanical engineering and one of textile industries. Judging from the course of study laid down for each class, and from the reputation of the professors, a high-class liberal education is now within easy reach of all Yorkshiremen. The calendar includes a prospectus of the Leeds' School of Medicine. For the coming session a much extended system of outside lecturing is announced, especially the arrangement made with the Gilchrist Trustees, through their secretary, Dr. W. B. Carpenter, F.R.S., by which some of the college professors will deliver four series of "Science Lectures for the People" in Leeds, Bradford, Halifax, and Keighley.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, September 5.—Prof. J. O. Westwood, M.A., president, in the chair.—Mr. F. Smith exhibited, on behalf of Mr. G. A. J. Rothney, a remarkably fine collection of Hymenoptera from Calcutta. Among them were several new species of *Cerceris* and a few new species of *Apidæ*.—Mr. McLachlan exhibited drawings with details of *Himantopterus fuscinervis*, an extraordinary insect from Java, described by Wesmael, in 1836, as belonging to the Lepidoptera. Dr. Hagen transferred the genus to the Neuroptera, in 1866, but Mr. McLachlan had recently examined the unique specimen in the Brussels Museum, and had decided that it was truly lepidopterous. Mr. McLachlan also exhibited leaves of a large species of *Acer* from trees growing in a garden in the neighbourhood of Brussels. Almost every leaf had been mined by the larva of a small saw-fly (*Phyllotoma aceris*), a species occurring in England. This insect only appeared in the locality mentioned last year, and yet was found by Mr. McLachlan in enormous numbers.—Prof. Westwood exhibited specimens of a minute Hymenopteron from Ceylon allied to the British *Mymar pulchellus*.—Prof. Westwood also exhibited males and females of the rare beetle *Narycius smaragdulus*, from India. This insect had remained almost unknown since the time of its description by the exhibitor in 1842.—Mr. James Wood-Mason, of the Calcutta Museum, exhibited the two sexes of *Phyllothelys Westwoodi* (*Mantidæ*), which species was remarkable on account of the presence of a large frontal horn in the female not represented in the male.—Mr. Wood-Mason also exhibited a beautifully-executed drawing of a stridulating spider (*Mygale stridulans*) in a stridulating attitude, and likewise specimens of stridulating scorpions, from India. Mr. Mason also handed to the president for identification, an homopterous insect with what appeared to be the larva of some case-bearing lepidopterous insect attached to it.—Mr. P. Wormald exhibited, on behalf of Mr. Pryer, a small collection of Chinese Lepidoptera.—Mr. G. C. Champion exhibited some rare beetles from Aviemore, Invernesshire; among them a new British Longicorn, *Pachyta sexmaculata*.—Mr. J. Jenner Weir mentioned a case of parthenogenesis in *Lasiocampa quercus* which had recently come under his notice.—The president read a letter from Herr Grevelink, of the Hague, relating to the insect which destroys the West Indian cocoa-nut trees (*Aleyrodes cocoïs*).—The Secretary exhibited a Longicorn beetle, which had been forwarded from Birkenhead by Mr. David Henderson.—Mr. J. W. Slater read a paper entitled "Vivarium Notes on some Common Coleoptera."

GÖTTINGEN

Royal Academy of Sciences, April 23.—The dates of Genesis, by M. Oppert.

April 30.—Celebration of the centenary of Gauss's birthday.

May 5.—On the mutual relations of magnetising force, temporary and permanent magnetism, by M. Fromme.—Experiments on the apparent attraction and repulsion between bodies moving in water, by M. Schiötz.—On the same, by M. Bjerknes.—Experimental investigation on the resistance of flames to the galvanic current, by M. Hopper.

July 7.—Demonstration of a tangent multiplier constructed on a new principle, by M. Riecke.—Remarks on some transformations of surfaces, by M. Enneper.—On the border-angle of the expansion of liquids on solid bodies, by M. Quincke.—On geometrical extensions of the Bezout fundamental law, by M. Schubert.—On the structure and systematic position of the genus Carludovica, by M. Drude.—Communication on the pyroelectricity of tourmaline, by M. Hoppe.

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THURSDAY, OCTOBER 11, 1877

FLEISCHER'S "VOLUMETRIC ANALYSIS"

A System of Volumetric Analysis. By Dr. Emil Fleischer. Translated by M. M. Pattison Muir, F.R.S.E. (Macmillan and Co., 1877.)

THERE is no question that volumetric analysis does not yet play that important part in quantitative chemical analysis which it merits, and which on the appearance of Mohr's well-known "Titrirmethode" it was confidently anticipated that it would assume. The method of instruction commonly pursued in many of our large public laboratories is in a great measure to be blamed for this result. It is, of course, necessary that the student should be put through a thorough course of gravimetric analysis, in order that he may attain that manipulative dexterity without which he cannot hope to become a successful operator, and perhaps no branch of practical chemistry is better calculated to afford the requisite training and practice than the somewhat tedious process of weight analysis, with its innumerable separations, filtrations, washings, and weighings. But however excellent may be their educational value, and however accurate their results, there is no doubt that many of the gravimetric methods at present in common use when viewed as practical processes for every-day application, fall very far short of what is required of them. It not unfrequently happens that the busy chemist, uncertain whether a lengthy analysis will afford him, after all, the requisite information, hesitates to incur what he fears may turn out to be a useless sacrifice of valuable time, and hence, from the want of rapid and sufficiently accurate analytical methods, many weighty facts may have been, doubtless actually have been, overlooked. Indeed, it is a question whether some of these analytical processes have not done as much to retard the progress of chemical science as to advance it. The majority of chemical workers, especially those engaged in scientific research, have not made analysis a special study, and hence when they are under the necessity of making a particular determination, they are content with the time-honoured processes which they have learned in the course of their laboratory-training. It is only by the appearance of such works as the one before us that the greater number of chemists are made aware of the advances which quantitative chemistry has really made.

Mr. Muir has done a very considerable service to his chemical brethren by his translation of Dr. Fleischer's work, for if we are not mistaken, it is the first attempt at a rational system of volumetric analysis which has been offered to us in this country. The work is divided into three distinct parts. In Part I., which treats of the volumetric method, we have, in Section I., an account of the principles on which this branch of the art of analysis rests. The several forms of burettes, pipettes, and measuring flasks recommended by the author, are described, together with the methods for their calibration and verification. The modes of preparing and standardising the normal solutions are next described. Section II. treats of the ordinary alkalimetric and acidimetric processes. Section III. gives an account of the various methods depending upon oxidation and reduction; these are respectively designated as oxidimetric and iodometric methods. Lastly, in Section IV., we have a description of the more important precipitation methods, *e.g.*, Mohr's method of determining chlorine by standard silver solution, and Wildenstein's process for estimating sulphuric acid by a standardised solution of potassium chromate. There is little in this part which calls for special remark; we would, however, point out that in the discussion on experimental errors, the magnitude of which, as the student is vaguely informed, may in certain cases be calculated by the mathematical method of least squares, it is not very apparent from the description how certain of the errors are eliminated. Dr. Fleischer's language is either not very clear on the subject, or his translator has failed to catch its exact meaning. The account of a method of verifying a pipette given on p. 23 will be apt to puzzle a beginner, on account of the unfortunate confusion between burette and pipette. We fail to see the necessity for the retention of the Réaumur scale of temperature in a work intended for English readers; and we are constrained to protest, with all possible energy, against the introduction of a new standard of temperature. What particular significance has 14°·5 R. to us in this country? If we are not content to take the melting-point of ice or the point of maximum density of water as our standard, let us at least maintain our credit as a law-abiding people by conforming to the enactments of our Legislature. In these respects Mr. Muir has undoubtedly sacrificed his independence to his loyalty to his author.

Unquestionably the most distinctive feature of the work is seen in Part II.; indeed, this constitutes a most valuable addition to the art of chemical analysis. In this part the author describes a variety of volumetric processes by means of which a large number of acids and bases may be determined, either separately or when mixed. He has not attempted to describe all the methods which have been proposed for the determinations of the several constituents, but has given only those which he has himself found to be reliable and capable of general application. In Section II. of this part it is shown how each base may be determined by volumetric methods without previous group-separation. The substance to be analysed is obtained in solution by the appropriate methods (which are fully described), and is divided into as many portions as there are constituents to be determined. As the author tells us, "the process of analysis is thus much shortened, not only by the omission of group-separations, but also by the fact that but one or two filtrations at the most are necessary; in many instances no filtrations are required. The precipitates do not require the same long-continued washing which consumes so much time in the ordinary processes. Two circumstances more especially recommend the methods under consideration. Every estimation is readily controlled by repeating the process on the original liquid; the analyses of technical products in which one or more, but not all, of the constituents is to be determined, becomes a matter of ease, and can be carried out much more rapidly than when it is necessary to make a systematic separation of the metallic groups.' Lastly, in Part III. it is shown how these methods are applied to the analysis of a number of important technical products such as potashes, soap, gunpowder, bone-ash, superphosphates, metallic ores, alloys, &c.

We cannot close this notice of a book which is really a solid contribution to chemical literature without referring to a circumstance which greatly detracts from its value; indeed, we fear that in many cases it may prevent the recognition of its great merit. Dr. Fleischer, like the great apostle whose worthy disciple he is, praises the times which are past; he is of opinion that our modern system of notation is founded on the most daring hypotheses, and he believes that the distressing complexity which the formulæ erroneously styled "modern" have produced, and the phraseology which has accompanied them, far outweigh any slight advantage which they have bestowed upon science; he thinks that such formulæ, "even supposing that there is a 'shadow of a reason' for their existence as Mohr trenchantly remarks, are peculiarly unfitted for analytical chemistry and for mineralogy." *O tempora, O mores!* With such convictions we are not surprised that Dr. Fleischer should have insisted on the retention of the old formulæ, although he has not actually prohibited the introduction of the newer notation in the translation. Mr. Muir has something to say for himself on this point; we entirely agree with him that the objections raised by the author have been answered times without number. *Il sabio muda conscio.* We hope therefore that, should a second edition be called for, Dr. Fleischer may be persuaded to put the work more in harmony with the time; we feel bound to say that had he done so in the outset the appearance of this second edition might have been considerably accelerated. T.

HARTLAUB'S BIRDS OF MADAGASCAR

Die Vögel Madagascars und der benachbarten Inselgruppen. Ein Beitrag zur Zoologie der äthiopischen Region. Von G. Hartlaub. Pp. 425, 8vo. (Halle: Druck und Verlag von H. W. Schmidt, 1877.)

NOTHING can be more conducive to the progress of zoological science in any country than the issue of handbooks of the different branches of its fauna in a cheap and convenient form. Such publications bring home to a multitude of observers a *résumé* of the facts previously known only to a few, and such as are too often scattered over the pages of periodicals and other works which can only be consulted in an extensive library. Those who are acquainted with the vast advance made towards our knowledge of the Birds of India since the issue of Dr. Jerdon's Handbook will readily admit the truth of what we say and many other examples might be adduced of the beneficial effects of similar publications.

Dr. Hartlaub's "Birds of Madagascar," although an excellent and original scientific work, is quite of the "Handbook" character—that is it gives us a *résumé* of all that is yet known concerning the Avifauna of Madagascar and the appendent islands in a cheap and portable volume—such as may be conveniently carried in the hand of any naturalist visiting those regions. Fifteen years ago Dr. Hartlaub issued a volume of similar character,[1] but much smaller in dimensions. To understand how great has been the advance lately made in our knowledge of the birds of these regions, we have only to compare the "Ornithologischer Beitrag" of 1861 with the "Vögel Madagascars" of 1877. Since the publication of the former work Holland has sent forth Pollen and Van Dam, France Grandidier, and England Crossley and Newton, into that rich and still imperfectly explored field, from which every one of them has reaped an abundant harvest.

The "Lemurian Avifauna,"[1] according to Dr. Hartlaub, is now known to contain 284 species of birds. Of these 220 are found in Madagascar itself, and 104 out of these 220 are absolutely restricted to that island. Moreover, of these 104 birds not less than ninety are so abnormal in structure that it has been found necessary to refer them to peculiar genera. Compared with Madagascar itself the appendent island groups are poor in species, although in every case there are many interesting forms amongst their winged inhabitants. The Comoro Islands muster only some forty-four species[2] of birds, Mauritius about sixty, of which fifteen or sixteen have been introduced by man's agency, and Bourbon about the same number, while Rodriguez appears to have only about twenty-five species now existing in it, of which four or five are certainly recent introductions. But we cannot speak of the recent ornithology of these islands without a passing allusion to the singular forms—now mostly known to us by their fossil remains—which have become but very recently extinct, and the gradual rediscovery of which must ever rank among the most interesting scientific achievements of the present epoch. Besides the Dodo of Mauritius and its brother, the *Pezophaps*, of Rodriguez, we now know that divers curious parrots (*Necropsittacus* and *Lophopsittacus*) and extraordinary rails (*Miserythrus* and *Aphanopteryx*) lived in those islands not long ago, and that other strange fowls were found in the same company. Two of the remarkable forms of the Mascarene Islands (*Coracopsis mascarina* and *Fregilupus varius*) have indeed become exterminated so recently that examples of their skins are still to be found in some of our older museums.

Let us now see what Dr. Hartlaub's conclusions as to the general facies of the avifauna of Madagascar and its appendent islands point to.

"Many years ago," he tells us, "the late distinguished naturalist, Isidore Geoffroy St. Hilaire, remarked that, if one had to classify the island of Madagascar exclusively on zoological considerations, and without reference to its geographical situation, it could be shown to be neither Asiatic nor African, but quite different from either, and almost a fourth continent. And this fourth continent could be further proved to be, as regards its fauna, much more different from Africa, which lies so near to it, than from India, which is so far away. With these words, the correctness and pregnancy of which later investigations tend to bring into their full light, the French naturalist first stated the interesting problem for the solution of which an hypothesis based on scientific knowledge has recently been propounded.

"For this fourth continent of Isidore Geoffroy is Sclater's 'LEMURIA'—that sunken land which, containing parts of Africa, must have extended far eastwards over

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[1] "Ornithologischer Beitrag zur Fauna Madagascars. Mit Berücksichtigung der Inseln Mayotte, Nossi-Bé und St. Marie, sowie der Mascarenen und Seychellen." 8vo. Bremen: 1861.

[1] Madagascar and its islands were proposed to be called Lemuria, in 1864, by Sclater, as being supposed remnants of the old "Terra Lemurum," wherein this peculiar form of mammalian life had its origin. The name has been adopted by Haeckel and other writers on Distribution.

[2] Since Dr. Hartlaub's work was published, an important addition has been made to the Avifauna of the Comoros by Mr. Edward Newton, in his memoir of the birds of the Island of Anjuian. (P.Z.S. 1877, p. 295 *et. seqq.*)

Southern India and Ceylon, and the highest points of which we recognise in the volcanic peaks of Bourbon and Mauritius, and in the central range of Madagascar itself —the last resorts of the mostly extinct Lemurine race which formerly peopled it. When Wallace, whose utterances on this subject everyone must read with the greatest interest, puts forward a former junction of Madagascar with Africa as beyond doubt—a junction which, however, must have terminated before the inroad into Africa of the more highly organised mammals—everyone will allow this opinion to be at all events well founded. But when he proceeds to state that the fauna of Madagascar is manifestly of African origin, his assurances are based upon very slender grounds. In truth the individuality of the fauna of Madagascar is so unique that even that of New Zealand can hardly be compared with it. Wallace's attempted parallel between Madagascar and Africa, and the Antilles and South America is, in our eyes, sufficiently disproved by the occurrence in the Antilles of Trochilidæ, one of the most characteristic forms of South America. But in Madagascar not one single one of the genera most characteristic of Africa occurs. This originality of the fauna is much too pronounced to allow Madagascar to be treated of only as a 'sub-region' or as an 'aberrant part' of the Æthiopian region."

To prove this position, Dr. Hartlaub in his interesting introduction to the present work, recapitulates the points in which the avifauna of "Lemuria" approximates to that of India and diverges from that of Africa."

"But the negative evidence," he adds, "is still stronger in the same direction. The groups of Musophagidæ, Coliidæ, Lamprotornithinæ, Buphagidæ, Capitonidæ, Indicatoridæ, Bucerotidæ, and Otidinæ, so eminently characteristic of Africa, are entirely absent in Madagascar, besides the genera *Gypogeranus*, *Helotarsus*, *Coracias*, *Crateropus*, *Irrisor*, *Bradyornis*, *Dryoscopus*, *Laniarius*, *Telephonus*, *Prionops*, *Platystira*, *Saxicola*, *Picathartes*, *Balæniceps*, and others, which are remarkably rich in individuals and species in Africa. Besides this, Larks and Chats, which in the African fauna are specially prominent on account of their numerous forms as well as regards their individual and specific abundance, are only represented by a single species in Madagascar itself, and in the rest of the sub-region not at all.

"In conclusion," Dr. Hartlaub adds, "if we take a glance at the families of the Madagascar sub-region as compared with those of Africa, four of these (Mesitidæ, Paictidæ, Eurycerotidæ, and Leptosomidæ) are peculiar, whilst the Diurnal Accipitres, Pigeons, Honeyeaters, and Cuckoos, are richest in species. In a considerable degree this is also the case with the orders Grallæ and Anseres. As contrasted with Africa the Fringillidæ, Meropidæ, and Sturnidæ (represented by only one genus), are extraordinarily poor. On the other hand, the Coraciidæ, Laniidæ, Artamidæ, Turdidæ, Muscicapidæ, Pycnonotidæ, and Lusciniidæ, are remarkable for their peculiarly modified types, and the Sittidæ, which are quite unrepresented in Africa, for the anomalous form *Hypherpes*."

Such are Dr. Hartlaub's matured views on a subject which he has long had before him, and is, above all persons, qualified to speak.

In concluding our notice we have only to thank him on the part of ornithologists for his convenient and useful volume, and to wish that the Avifauna of many other countries were treated of in a similar manner.

OUR BOOK SHELF

Pollen. By M. P. Edgeworth, F.L.S. Illustrated with 446 figures. (London: Hardwicke and Bogue, 1877.)

MR. EDGEWORTH informs us in the preface that this work is a considerably altered edition of a paper laid before the Linnean Society last year, but withdrawn by the author, on account of his omitting to notice the work of other botanists, British and foreign, on the same subject. The work chiefly consists of plates with the explanations and a list of forms of pollen figured by other authors, as well as some general remarks on the forms of pollen in different families. The figures are all drawn to scale, are fairly done, and there can be little doubt that the microscopist who loves pretty objects will promptly avail himself of Mr. Edgeworth's assistance in following up the subject. Very much valuable information is given in this book and it cannot fail to be useful to the scientific botanist. We feel, however, that Mr. Edgeworth does not wholly command our confidence on account of certain blunders he makes. Most of the German botanists have their names misspelt. Thus he always calls Purkinje "Purjinke," Naegeli "Nagili," Rosanoff "Romanoff," Pollender "Pollenden," Luerssen "Leursen." Surely if Mr. Edgeworth had been familiar with the writings of these men, he from merely seeing their names on their papers, would not have blundered so strangely. Then we feel rather doubtful about his references as we have failed to find any paper by "Nagili" in Pringsheim's "Jahrbücher," vol. iii. Naegeli's name does not occur at all in the index to the first ten volumes of Pringsheim's "Jahrbücher." The third volume of the "Jahrbücher" was published in 1863, while Naegeli's paper on the development of the pollen was published at Zurich in 1842. We think the student would hardly find the papers of "Purjinke in Latin," "Fritsche in German," "Pollenden Bonn." Why not refer to the proper title of the book or paper? Pollender has published two papers on pollen, at Bonn, one in 1867, in quarto; another in 1868, in folio. To which does Mr. Edgeworth refer? Then surely it is too late in the day to describe the pollen of the pine as consisting of "2 grains of pollen connected as it were by a broad band" (p. 8); or the pollen of some Acanthaceæ as existing "in a peculiar coil, which can be unwound," in both cases the peculiar development of the extine being overlooked. Altogether, then, the work has slightly disappointed us, but perhaps we should not judge so much by the blemishes we notice in it, as by the undoubted worth both to the amateur and scientific botanist of the figures and references.

Die Auster und die Austernwirthschaft. Von Karl Moebius. (Berlin: Wiegandt, Hempel, and Parey, 1877.)

MUCH has been said and more has been written about oysters and their culture. Astonished by large figures many writers wished to astonish their readers in a similar way, and to induce the coast population of all civilised countries to undertake the culture of enormous masses of this most costly of all molluscs. Thus a belief has been widely spread that wherever there was a coast and seawater, oyster-beds could be established and quantities of oysters could annually be obtained without much trouble. The little book we have under notice is therefore well timed. It reduces to their proper and reasonable measure all ideas on this subject in speculative heads, and, as the author owns himself, it will for that reason be hardly welcome to these would-be oyster cultivators. But it will be all the more so to biologists, proprietors of oyster-beds, and the educated public generally, since it contains valuable details of the biology, the peculiarities, and the life-conditions of oysters. It will, we have no doubt, also find a favourable reception amongst those government departments of the various states of Europe and America, whose duty it is to superintend the oyster-fisheries and the natural oyster-beds, since it will offer them a reliable basis for their judgment in adopting or rejecting measures relating thereto. Prof. Moebius gives a very able account of the artificial oyster culture in France, and of the attempts made in this country to introduce the French system of artificial culture, which unfortunately all ended in failure. He then asks the question whether artificial culture after the French method would be possible on the German coasts of the German Ocean, and in a well-written chapter arrives at the conclusion that this question must be answered in the negative. An important query is whether natural oyster-beds can be artificially enlarged, and whether new oyster-beds can be established. Prof. Moebius thoroughly ventilates this question, and an attentive perusal of the little work will not leave anybody in doubt as to whether any intended experiments will or will not be crowned with success. The author quotes several examples of natural beds which were ruined by over-fishing; he also gives an account of the repeated experiments made in the Baltic with a view of establishing natural oyster-beds, all of which failed, the last with 50,000 oysters deposited in 1843 near the Island of Rügen, of which only two years afterwards not a single one remained alive. One of the most interesting chapters in the book is the one treating of the increase in the number of oyster-eaters, the rise in the price and the decrease in the quantity of oysters; it contains numerous statistical data showing how, in 1740, fresh oysters were sold at Hamburg at 3*d.* per hundred! Even as late as 1830 they were sold at 1*s.* per tub (about 1,600) at Falmouth; but Prof. Mœbius doubts whether in this age of railways and steamboats we shall ever return to such a state of things. A chapter on the chemical constituents and the taste of oysters, and another on the object and results of a rational culture of oysters, form the conclusion to this clever little work.

Die Naturkräfte.—Die Gesetzmässzigkeit im Gesellschaftsleben. Statische Studien von Dr. Georg Mayr, Ministerialrath und Universitäts Professor. (München: R. Oldenbourg, 1877.)

THIS small and readable volume contains a slight but comprehensive sketch of the main features of political and social statistics. It shows how the necessary data have to be obtained, how they should be discussed, and how the final results may be most clearly published, whether in a graphic or a tabular form. It enters into no technicalities, it is of very little value as a storehouse of information, and it contains perhaps no remarks that are strikingly original, but being written by a very competent statistician it has the merit of giving a good, general idea of the range of statistical inquiry and of its national value. It is well calculated to instruct those who may desire to obtain a broad and just view of the efforts, the difficulties, and the achievements of modern statisticians. F. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Potential Energy

MOST persons must agree with your correspondent, "X.," that the term "potential energy" has been used with considerable vagueness and with some difference of meaning by various writers. They may even go further, and doubt at times whether they are quite clear with respect to the cases to which the terms "force" and "energy" are respectively applicable. But this arises, I am inclined to think, from the difficulty of understanding what is force, and would certainly not be removed by bringing these two terms into more frequent and closer juxtaposition than that in which they are now found.

Without attempting to reconcile the somewhat conflicting views of different writers on the subject of potential energy, which, however, I must own, are not so far apart from one another as "X." seems to suppose, I should like to make a few remarks with respect to his proposal to transfer "potential energy" from the body in which it is said to exist to the force to which it owes its existence.

Energy, as generally understood, is of two kinds: the one is energy of motion, and the other energy of configuration. In both cases the system possessing energy has the power of doing work; in the one case actually, in the other potentially. Now it is against this potential power of doing work that "X." protests, for he considers the expression tautological. But here I disagree with him. I can see nothing incomprehensible in the statement that a body has the power of acquiring the power of doing work; and, to restrict myself to the very simple illustration of a stone raised to a certain height, I should say that the system of the stone and earth, by the action of a certain force through a certain distance, had gained the power of acquiring a certain amount of kinetic energy, *i.e.*, the power of acquiring the power of doing a certain amount of work.

This seemingly tautological phrase is more in the language than in the idea; for we often use, with respect to other subjects than physics, similar expressions. We may say that the possession of wealth confers the power of purchasing, and a wealthy man is one who possesses a certain power which he may have gotten himself, or which may have been transferred to him from another. But a youth with a certain education, and placed in a certain position, though not yet wealthy, may be said to be in a position to acquire wealth,—to have in himself the power of acquiring the power of purchasing.

One other point I would notice before considering "X.'s" new proposal.

But first I would say that justice is hardly done to every "doctor" (I am none myself) when we are told, in connection with the projection of a stone upwards, that "the gravitation attraction is usually and conveniently conceived and spoken of as all the earth's; and the stone is usually regarded as being simply attract*ed*," and that "every doctor will frequently speak thus." Certainly one doctor whom "X." has quoted is careful to say that "when a stone has been lifted to a certain height above the earth's surface, the system of two bodies, the stone and the earth has potential energy, and is able to do a certain amount of work during the descent of the stone."[1]

"X." finds considerable difficulty in understanding that if the leaden weight of a clock is raised by winding it up the energy of the clock is thereby increased. He says that "the weight sets-to and works with E., which it has not in possession, but only has the power of acquiring, and which it loses the power of acquiring!!" and in a note we are told that "the weight never acquires more than a quite insensible amount of actual E. so called." This is quite true, because the "actual E." is continuously used up as fast as it is acquired. If the weight could have fallen freely it would have possessed a store of "actual E." at its lowest point; but instead of being able to do this it has been continuously transferring its actual energy to the machinery of the clock which it has set in motion. The weight at its highest point was in a position for doing work, and during its descent work has been done.

Let us see now if there are no difficulties connected with the proposal to call potential energy "energy of tension," and to locate it in the force rather than in the body. We are told if you spend E. "in raising a stone to a certain height, you have bestowed your E. on that attraction, you have transferred your E. to gravity." This is not very clear, but "X." goes on to say "that attraction was beforehand pressing at the stone as hard as it could (this looks as if "X." placed the attraction in the earth only), but it had no power of doing work." Well, why not? Because, practically, there was no force acting on the stone. The force of gravitation was counteracted by the reaction of the surface on which the stone was resting. A force free to act has always the power of doing work; but the existence of a force presupposes the existence of a body, and the confusion is, therefore, considerably increased by speaking of the transference of the power of doing work from a body to a force. There seems to be no harm in speaking of the energy of a force, but then we mean the energy due to a force; and this can be as well said of kinetic as of potential energy.

With the metaphysical difficulty about force staring us in the face, it surely would be very unwise without the gain of some much more solid advantage than "X." has made out, to complicate the idea by giving it the attribute of possessing energy; the connection between force and matter is so intimate that let energy mean what it may, the idea of its transference from the one to the other is unintelligible There are many difficulties connected with the subject of potential energy which the progress of science is likely to remove, but they are not to be got over by the verbal alteration proposed by your correspondent. The energy of compressed air was at one time supposed to be potential and is now regarded as kinetic. Further inquiries into the constitution of matter may enable us to see that many forms of energy which are still regarded as potential are really "actual." Meanwhile we may, I think, suppose potential energy to mean the power of acquiring the power of doing work and to be located in the system possessing this power. P. M.

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[1] Clerk Maxwell, "Matter and Motion," p. 81.

The Arts Club, October 7

Indications of the Ice-age in Shetland

SHETLAND will now be narrowly searched for proofs of glacial action by every tourist who takes an interest in such vestiges of a bygone era. Smoothed surfaces, striæ, and grooves are so abundant and distinct on Mr Peach's ground—the sandstones on the shores of the Loch of Clickhemin, and of the immediately adjoining bay—as to have long ago suggested the innocent or waggish notion that the last were scratches made by the prows of the Norsemen! Still on the mainland, but some forty miles distant, on the shores of the magnificent bay of St. Magnus, striæ are to be seen on the sandstones of the hääf-fishing station at Stennis, and till, or boulder-clay, lies in patches on the Tuans of Hillswick. Ice has made distinct markings, running east and west on the gneissose rock close by the door of the farm-house of Ailesburgh, which is perhaps about a mile north of the narrow isthmus of Mavis-Grind. The huge moraine-looking mound, which lies between the south-east foot of Ronas-hill and the head of Ronas-voe, claims a special examination by those who wish to be further satisfied as to the former existence, or otherwise, of glacial action in Shetland. G. G.

The Discoverer of Photography

IN your account of the death of Mr. Fox Talbot (NATURE, vol. xvi. p. 464), you state that he first entertained the idea of the art of what is now called photography in 1833, and that it was not till 1839 that he and Daguerre first made known the principles of photography under the name, I think, first of Daguerrotype, followed by Talbotype. I therefore think the following notes concerning Niepce may interest some of your readers:—

I cannot now from memory give exact dates, but I think it was at least ten years previous to 1839 that there lodged in a neighbouring house to where I now reside a Frenchman of the name of Niepce; he was, I think, engaged on a perpetual motion machine. He died, which necessitated his brother coming from Paris to Kew. The brother was a theatre scene-painter, and had discovered the art of fixing upon metal the pictures of objects reflected by the sun. On arriving at Kew he put up at the "Coach and Horses" Inn, then kept by Mr. Cusel, and not being able to speak English, Mr. Cusel introduced him to Mr. Francis Bauer, the celebrated botanical artist, then residing at Kew. Niepce had brought with him three pictures, specimens of his discovery, which he showed to Mr. Bauer, who became much interested in them. He deemed the discovery worthy of being made known to the Royal Society, but as the method of obtaining the pictures was not described in the notice sent to the Society, they would not entertain it, and nothing was done in the matter. Niepce returned to Paris, leaving two of the pictures with Mr. Bauer, and the third with Mr. Cusel in part payment of his bill, he being a poor man. Being a frequent visitor to Mr. Bauer, the latter naturally called my attention to the two pictures that hung in his room for at least ten years. In time Niepce let the secret of his discovery become known to M. Daguerre, and in 1839 this discovery came before the public under the name of "Daguerreotype," and about the same time "Talbotype" was announced. This led Mr. Bauer to write a letter to the *Athenæum*, fully explaining all particulars of what I have here stated from memory. In his letter Mr. Bauer said he should be happy to show the pictures to those interested in the subject. Consequently he had many callers, one of the earliest being Dr. Percy, whom I remember coming to me, wanting to know where he could find Mr. Cusel, who had then retired and was living at Richmond. Dr. Percy went off to Richmond with the intention of buying the picture, but I remember telling him Mr. Cusel would not sell it as he was not in need of money. Such was the case, as Mr. Cusel told me some time after "that he would not sell it; no! not if he was offered 100*l*. for it." Mr. Cusel is long since dead, and what became of his picture I know not. After Mr. Bauer's death, in 1840, these two pictures came into the possession of his friend, Mr. Robert Brown, and I believe are now in the British Museum.

If you consider what I have now stated worthy of a place in NATURE, it is at your service. J. SMITH

Park House, Kew, October 9

The Portrait of Tycho Brahe

IN reference to the portrait of Brahe engraved in NATURE (vol. xv. p. 406), and to Mr. Dreyer's remarks on it (vol. xv. p. 530), I have the pleasure of sending you the following particulars. In the first place I have permission from Herr Friis, of Copenhagen, the learned editor of *Tichonis Brahei et ad eum doctorum virorum Epistolæ. Havniæ*, 1876, &c., to publish an important letter from himself:—

Copenhagen, June 9, 1877

DEAR SIR,—I have seen in NATURE (vol. xv. p. 405) an article on Tycho Brahe, with a portrait of him after a painting in your possession. On that account I take the liberty of addressing myself to you.

In a book printed in Copenhagen in the year 1668 is mentioned a portrait of Tycho Brahe which once belonged to King Frederick III., and which, no doubt, has had an emblematic figure and inscription similar to that of the portrait you own. The title of this book is "Inscriptiones Haffnienses latinæ, danicæ et germanicæ una cum inscriptionibus Amagriensibus, Uraniburgicis et Stellæburgicis, &c., edi curavit Petrus Johannis Resenius," and in that you read at page 335 the following:—

"Sub pyramide tegumento quodam cooperta ad effigiem ejus quæ in Augustissimi Regis Daniæ Friderici III. Bibliotheca hodie reservatur depicta hæc legitur inscriptio:—

STANS TEGOR IN SOLIDO VENTUS FREMAT IGNIS ET UNDA
VANDESBECHI
AN. MDXCVII QUO POST DIVTINUM IN PATRIA EXILIUM DEMUM
PRISTINÆ LIBERTATI RESTITUTUS FUI
TYCHO BRAHE, OT.

On leaving Denmark T. Brahe sent his portrait to his friend, the learned Holger Rosenkrantz. This has, I suppose, been one resembling the one you now possess, even if it should not be just the same. Compare T. Brahe, "Astronomiæ instauratæ mechanica." Wandesburgi, 1598, fol 4.

The German letters on the order M. H. Z. G. A. indicate Frederick the Second's motto: My hope (is) in God alone (Meine Hoffnung zu Gott allein), which is often seen in buildings, &c., from his time.

If you will be kind enough to send me a photograph of the before-mentioned portrait, I should feel very grateful to you, as I have made the biography of T. Brahe my special study, and just recently began to publish his correspondence with his learned contemporaries.

Hoping that you will not deny me this favour, I am, dear sir,
Yours obediently,
F. R. FRIIS

Cortadelers Gade, 7, Copenhagen

I have referred to an exquisite copy of the Inscriptiones Haffnienses, from the library of Colbert, belonging to Chetham's Library, in this city, and on the same page referred to by Herr Friis I find a poem by Oliger Rosenkrantz addressed to T. Brahe, and prefixed to the Mechanica, of which the last two lines are very interesting, as alluding, in my opinion, to the emblem on my portrait. They are:—

"Pectora quam Divi dispensant tramite justo,
Stansq. vado fluctus, imbres et flamina temne."

I wish to add a few remarks, and before I proceed farther I would observe that in your engraving the dress of Brahe is not given correctly, not from any fault of yours or of your engraver, but because the detailed drawings sent in answer to a request from him to me for details did not reach him until the plate was too far advanced. I have had the picture re-photographed, the photograph worked upon from the picture in a strong light, and a satisfactory result will be published in the *Memoirs* of the Literary and Philosophical Society of Manchester, and also, I believe, in Herr Friis's very interesting and important work, two fasciculi of which he has kindly sent me.

It is now certain that Brahe, whilst at Wandesbeck, or Wandesburg, near Hamburg, sat to a painter, for here we have evidence in a book published at Copenhagen, in 1668, that King Frederick III. had *that* picture and that it was dated Vandesbechi, 1597; and moreover, that that portrait had an emblem upon it, which, from the motto, was presumably very like that on mine, but the position and the words of the motto differing, the motto and also the inscription on King Frederick's portrait being *below* the emblem, whilst on mine the motto is on a ribbon or label wound round the pyramid, and the inscription is on the other side of the picture. In King Frederick's the emblem consisted of a pyramid with some kind of covering ("sub pyramide tegumento quodam cooperta"), and so it is in mine. That wind, fire, and water were also represented in that emblem, as in mine, is clear from the words "ventus, ignis, et unda" in the motto, which are precisely the words employed in mine, the only difference in the two cases being that in the king's there is the word "fremat," instead of "strepat" as on mine. In my portrait the year 1597 is inferred from the inscription saying "Anno 50 completo," Brahe being fifty years old on December 13, 1596. By a careful examination of Brahe's Latin Life by Gassendi, 1656, I found that Brahe wrote a remarkable poem addressed to Ranzovius, in which the words "exilium in patria" occur; and as he stayed at Ranzovius's from the end of October, 1597, I conjectured (*Proceedings* of Lit. and Phil. Soc. of Manchester, October 31, 1876) that my portrait was painted between that date and his next birth-day (December 13, 1597), a supposition confirmed by Herr Friis pointing out that the lost picture of King Frederick's is dated at Wandesburg (Vandesbechi).

That mine is no copy of that picture is manifest from the differences which the notice in the "Inscriptiones Haffnienses" has enabled me to point out. My conjecture is that Brahe sent his portrait to King Frederick, who is expressly absolved by Brahe from the blame of Brahe's expulsion from Denmark, and that he advisedly wrote "pristinæ libertati" instead of "libertati desideratæ" as on mine; and further I have little doubt that the same artist painted both pictures.

I have examined the portraits in the print room of the British Museum as well as the oil painting at the Royal Society, and have taken much pains to ascertain the existence of any other portrait than mine representing Brahe later than 1587; ten years earlier than mine. That it does not agree with the engraving after Gumperlin's portrait is no proof whatever that mine is not a good representation of him in his fifty-first year, when we consider how much a man's features change in the ten years between forty-one and fifty-one, and moreover Brahe may have been in the meantime to the Promontory of Noses for a fresh one. But whatever be the reasonableness of these conjectures, it is almost certain that he sat twice at Wandesburgh to this portrait painter, and that one of these portraits was considered worthy of a place in the king's library.

SAMUEL CROMPTON

Manchester

Lumière Cendrée

SCHRÖTER pointed out that it is towards the third day of the new moon that the ashy light has the most intensity and that it is stronger before the new moon than after.

Schröter's explanation is that during the waning of the moon the ashy light is stronger because the moon is enlightened by the continents of Asia, Africa, and Europe, but after the new moon by the Atlantic and Pacific Oceans.

Godfray in his Astronomy says:—Supposing this difference to exist, and this explanation to be the correct one, the phenomenon must be just reversed in China and Japan.

Has anything been done to test the accuracy of Schröter's theory? If it is correct the ashy light cannot present the same appearance to an astronomer in New York, because there would be a greater proportion of reflecting surface in the hemisphere of the earth turned towards the moon in the one case than the other.

Schröter, I believe, found that the ashy light was stronger in autumn than in spring. This cannot be accounted for by his explanation, for the distribution of land and water remains the same.

I shall be obliged to any of your correspondents who can tell me where there are any records of observations on this subject.

B. G. JENKINS

4, Buccleuch Road, Dulwich, October 1

Lightning Conductors

IN a paper on lightning conductors, communicated by us to the *Journal* of the Society of Telegraph Engineers, we gave at full length our reasons for believing that the wire cage first suggested some years ago, and recently proposed by Prof. Clerk Maxwell, as a protection against lightning, would not act as a complete protection, since, although there is no resultant force inside a closed conductor due to exterior *statical* electrification, experiment shows the existence of such a force when electric currents are passing either near or through a closed conductor. The recent case of deaths by lightning in a mine, communicated to the Asiatic Society of Bengal, on April 4 of this year, by J. J. Whitty, Esq., superintendent of the Kurhurbari Collieries, Giridhi, India, appears to add experimental proof to the reasoning advanced in our paper. Mr. Whitty says:—"The mine is a shallow one, worked by levels driven on the side of a flat-topped hill, only twenty feet from the surface, which is, therefore, the thickness of rock above the coal-seam. The working-face where the accident occurred is about 130 feet from the opening. There were a number of miners in the drift at the time. Those near the entrance were unaffected. The two who were killed (a man and a woman) were at the working-face in adjoining galleries, separated by about twelve feet of coal. A young *sâl* tree, standing as nearly as possible over the position of the accident, was slightly damaged, and in the ground at its base a hole, about one inch in diameter, seemed to have been formed by lightning. The little hill, or plateau, in which the mine is situated is one of a small irregular group in the centre of the coal field, about 200 feet high. It is formed of the coal-measure sandstone. The drainage is thorough, and the mine was quite dry. From the presence of the workmen the sides of the gallery and the air in it were probably damper than the rock. The tree or other vegetation on the hill is scanty. On the day of the accident 0·96 inches of rain fell."

It would therefore appear that the two people who were killed were practically entirely surrounded by a partial conductor in connection with the earth. It will no doubt be objected that twenty feet thickness of coal-measure sandstone, even when damp on the surface, is not a good closed conductor, but we think it is certainly as good a protection as would be afforded by the wires Prof. Clerk Maxwell proposes to lead *merely* along the edges of a building.

JOHN PERRY
W. E. AYRTON

The Imperial College of Engineering, Tôkiô, Japan,
August 6

Electric Lighting

I HAVE examined the patent (No. 10,919, November 4, 1845, Edward Augustin King) which Prof. Mattieu Williams drew attention to in NATURE, vol. xvi. p. 459, as anticipating the invention of Lodighin's electric wick, and I think Lodighin has been clearly forestalled in principle, the practical details alone being different in the two cases.

I do not think, however, that Mr. King's patent includes Kosloff's improvement, whatever value may attach to the latter. I think it is very plain that porcelain is employed in King's patent merely as an insulating bar to connect the two forceps rigidly together without shunting any of the current between them past the carbon.

J. MUNRO

West Croydon, October 2

Caterpillars

LAST year (NATURE, vol. xv. p. 7) I communicated the result of some experiments on the caterpillars of *Pieris brassicæ* from which it appeared that, when these are artificially converted from *succincti* into *suspensi* by cutting the loop before the exclusion of the chrysalis, a certain number (a third or fourth of the whole succeed in attaching themselves to the silk by the hooks in the tail of the chrysalis in the manner of the true *suspensi*. I have repeated the experiment this year with a like result, and I have also had the satisfaction of witnessing the process of successful exclusion, and comparing it with that of the chrysalis of *Vanessa urticæ*. The method is essentially the same, except that the rapid and assured precision with which the Vanessa chrysalis thrusts up its tail and lays hold upon the silk, is replaced in Pieris by long and laborious efforts, as if the tail were just a little too short to reach the silk.

I have likewise made similar experiments with another of the *succincti—Anthocharis cardamines*—with the following results:—In seven instances I cut the loop (and sometimes a second one) which the caterpillar had spun; and in all the chrysalis was excluded without falling down; but in no case was the tail of the chrysalis withdrawn from the pocket of the old caterpillar-skin, so that its suspension is directly from the latter. In eleven cases in which I did not interfere, only two chrysalids were excluded in the normal way, *i.e.*, vertically, with the head up, a girdle round the insect and the chrysalis-tail withdrawn from the old skin and attached immediately to the silk on the stem of the plant. In three other cases in which a loop was spun by the caterpillar, the chrysalis seems to have turned upside-down during exclusion, the tail being now uppermost, the *loop twisted*, and the hooks fastened in loose silk upon the plant-stem. Six caterpillars either spun no loop at all or one so insufficient that they became *suspensi* of themselves before exclusion began, and were all but one (which fell down) successfully excluded in this position—the tail of the chrysalis, however, being still retained within the pocket of the old skin.

The most interesting and curious point in the transformation of a caterpillar of the *suspensi* is the manner in which the newly-excluded chrysalis is kept from falling, while its hook-furnished tail is being withdrawn from the old skin of the caterpillar and made fast in the cone of silk to which the latter was attached. I am ignorant whether any other explanation of this process has been given than that, I believe, originally communicated by Réaumur and detailed in Kirby and Spence, vol. iii. pp. 208–209, and repeated in such recent works as Figuier's "Insect World," from the English edition of which work by Prof. P. Martin Duncan (1872), p. 148, I quote the following account of the pupation of *Vanessa urticæ*:—"But here comes the culminating point, the most difficult part of the operation. The chrysalis, which is shorter than the caterpillar, is at some distance from the silky network to which it must fix itself; it is only supported by that extremity of the caterpillar's skin which had not been split open. It has neither legs nor arms, and yet it must free itself from this remaining part of the skin, and reach the threads to which it is to suspend itself. *The supple and contractile segments of the chrysalis serve for the limbs which are wanting to it. Between two of these segments, as with a pair of pincers, the insect seizes a portion of the folded skin, and with such a firm hold that it is able to support the whole of its body on it.* It now curves the hinder parts slightly, and draws its tail entirely out of the sheath in which it was inclosed," &c. (The italics are mine.) How this can be conceived possible, considering the utterly soft condition of the newly-excluded pupa, and that the caterpillar skin is now "reduced to a packet so small that it covers only the end of the tail of the chrysalis" (*loc. cit.*), in which, moreover, there are no longer any free segments, I cannot understand. On the other hand, it is very easy to show that the last and sufficient bond of connection between the chrysalis and the old larva-skin is *a membrane extending from the lining of the latter to the anterior horns of the two lateral ridges bounding the anal area of the chrysalis.* I have prepared several specimens showing this membrane still intact, and should be happy to forward one or two, if required, for inspection. I find it in all three species of butterfly mentioned above, and I believe it is to the persistence of it unbroken that is owing the continued suspension of my chrysalides of Anthocharis. I have tested its strength to sustain the weight of the chrysalis, and the time during which it resists desiccation and the writhings of the insect, the obvious object of which is, not to get rid of the old caterpillar-skin, but to rupture this membrane after the chrysalis has made good its tail-attachment to the silk.

J. A. OSBORNE

Milford, Letterkenny

The Satellites of Mars

IT is not necessary to have an enormous telescope in order to see the outer satellite of Mars. I had a very satisfactory view of it on September 15 at 9h. 20m. with a nine-inch reflector, and only lost it in the planet's glare at about 10h. 50m. I would have written to you on the subject earlier, but was not aware that it was considered so extremely difficult an object until I read the letters in your paper of the 27th ult.

JOHN BRETT

The Lizard, Cornwall, October 6

Rate of Mound-Building

THE papers announce that Mr. Layard has obtained permission to renew excavations in the Mesopotamian Valley. Several other explorations will be in progress during the coming season in countries where no trained labour can be obtained. I write to beg the gentlemen having the work in charge to make some accurate observations as to the amount of dirt which a man can move in a day with rude implements, noting the distance as well. A discussion has sprung up concerning the time required to build our Mississippi Valley mounds. The investigation of which I speak will throw some light on the subject.

Washington, D C., September 26 OTIS T. MASON

OUR ASTRONOMICAL COLUMN

THE MELBOURNE OBSERVATORY.—The twelfth Report of the Board of Visitors of this Observatory, addressed to the Governor of Victoria, with the Annual Report of the Government Astronomer, is before us. It presents an outline of the work accomplished between June 20, 1876, and May 22, 1877, and of the work in progress and in prospective. With the great reflector, which is in charge of Mr. Turner, the observation and drawing of Sir John Herschel's figured nebulæ has been continued. A finished drawing of the Horse-shoe Nebula, M. 17, has been made, together with drawings of fifty-seven of the smaller nebulæ. The publication of this work is in progress; out of ninety-three drawings which it is intended to publish, sixty-one are already lithographed; they are representations of the nebulæ on a black ground, and Mr. Ellery states that they render the telescopic appearance of the objects in a most effective and truthful style, and if the lithographic printers succeed in obtaining the requisite number of copies as perfect as the proof copies which were submitted to the Board of Visitors, he considers that "the whole difficulty of economically and satisfactorily reproducing these astronomical drawings will be surmounted." The descriptive letter-press will be ready by the time the lithography is finished, and it is expected that before the next annual inspection of the Observatory this first instalment of results furnished by the great telescope will have been distributed over the colonies and throughout Europe and America. With the "South equatorial" Mr. Ellery has been engaged upon a work of no small interest and astronomical value, viz., the re-measurement of the double-stars contained in Sir John Herschel's Cape Catalogue, 1834–38, in which revision he is promised the co-operation of Mr. Todd with the Adelaide refractor. Mr. Ellery further mentions that he hoped to utilise the present opposition of Mars, in connection with northern observatories, for a determination of the solar parallax. The transit-circle observations, which are regarded as the main work of the establishment, are zealously continued. The magnetic and meteorological work is upon the same general plan as hitherto, but the former was likely, at the date of the Report, to suffer some interruption from the necessity of erecting a new magnetic-house.

THE OUTER SATELLITE OF MARS.—Though this object will no doubt be growing fainter with the increasing distance of the planet from the earth, a few positions are subjoined which have been deduced from elements fairly representing measures made by Mr. Common, at Ealing, to the end of September. The two or three days when the moon will be near to Mars are omitted:—

At 8h. 30m. Greenwich Mean Time.

Oct. 11 ...	Pos. 69° ...	Dist. 66″	Oct. 20 ..	Pos. 42° ...	Dist. 37″
,, 12 ...	,, 107 ...	,, 38	,, 21 ...	,, 80 ..	,, 58
,, 13 ...	,, 235 ...	,, 51	,, 22 ...	,, 168 ...	,, 22
,, 14 ...	,, 266 ...	,, 57	,, 23 ..	,, 248 ...	,, 58
,, 15 ...	,, 23 ...	,, 29	,, 24 ...	,, 288 ...	,, 34
,, 16 ...	,, 74 ...	,, 64	,, 25 ...	,, 53 ...	,, 45

At the times mentioned in Lord Rosse's letter (NATURE, vol. xvi. p. 457) the calculated places of the satellite were as follows:—September 8, at 11h. 45m., pos. 70°, dist. 85″, and September 15, at 11h. 30m., pos. 246°, dist. 79″.

The period of revolution given by measures between

August 11 and September 30 appears to be 1d. 6h. 18m. 12s.

THE NEAR APPROACH OF SATURN AND MARS, NOVEMBER 3.—At the times of meridian transit at Greenwich the position of Saturn with reference to Mars near the conjunction of those bodies at the beginning of the ensuing month will be—

November	2	...	Angle	80°	...	Distance	24'·5
,,	3	...	,,	138	...	,,	9·4
,,	4	...	,,	212	...	,,	22·0

It will be seen that on November 3, about 8 P.M., the distance is about equal to the greatest elongation of the Saturnian satellite Japetus, but the satellite is not on this occasion in a position to be occulted by Mars.

NEW COMET.—A telescopic comet was discovered on the evening of October 2, at Florence, by M. Tempel, to whom we already owed the discovery of the remarkable comet of January, 1866, which is found to be associated with the November meteor-stream, and the comets of short period of 1867 and 1873. Its position at 9h. is stated to have been in R.A. 23h. 51m., N.P.D. 100° 19'. It was observed by Prof. Winnecke at Strasburg on the 6th, and is described by him as pretty bright, about 0'·4 in diameter with a star-like nucleus 10·11m., and a faint tail 4' in length on an angle of 25°. The diurnal motion appears to be about 3·5 minutes in R.A. diminishing, and in N.P.D. about 64' increasing.

It may be noted that the position of this comet on October 2 was not far from that which would be occupied by the short-period comet of De Vico, due about this time, if it had arrived at perihelion at the end of the first week in September, but the observed direction of motion of the new comet is contrary to that which De Vico's must have under such condition, so that there can be no suspicion of identity. Prof. Winnecke's observations on October 6 give for the comet's apparent place at 11h. 15m. 5s. mean time at Strasburg, right ascension 23h. 36m. 21·59s., south declination 14° 36' 33"·0.

BIOLOGICAL NOTES

THE GOMBI ARROW POISON.—In a recent number of the *Bulletin Mensuel de la Société d'Acclimatation* of Paris, M. M. E. Hardy gives a detailed account of researches and experiments on the active principle of the poison obtained from the seeds of *Strophanthus hispidus*. This plant, which belongs to the poisonous order Apocynaceæ, was first observed by Houdetot, a French naturalist in Senegambia, afterwards by Smeathmann near Sierra Leone, by Baikie at Nupé, by Griffon du Bellay at Gaboon, and by Gustav Mann in Western Tropical Africa. It is a climber with a hollow cylindrical stem, and grows in the forests, where it ascends to the summits of the highest trees. The oblong, nearly sessile, opposite leaves are from ten to twelve centimetres long by five wide, and are covered with hairs, particularly on the under surface. The yellow flowers are borne on terminal cymes. The fruit is a cylindrical follicle somewhat thicker that the thumb, and contains from 100 to 200 oval seeds. By means of a fruit given them by the Paris Society, MM. Hardy and Gallois have discovered that the active principle is not, as was supposed, an alkaloid, and for it the name *Strophantine*, given to it some years ago by Dr. Fraser, is retained. Besides, they succeeded in isolating a substance presenting the characters of an alkaloid, but which did not seem to possess any marked physiological properties; for this they propose the name *Inéine*. The former is very poisonous, a single crystal placed under the skin of a frog's foot causing the cessation of the heart's action in a few moments. Even after this has taken place the animal still possesses the power of motion, and it is only after respiration has become impossible, owing to the interruption of circulation in the nervous centres, that death ensues from paralysis of the heart. These observations, though yet incomplete, accord pretty well with facts recorded by different authors, and seem to prove that *Strophantine* is really the poisonous agent in *Strophanthus hispidus*. The most elaborate experiments on the poison found at the extremity of the arrows (used by the natives both in war and in hunting) are those conducted by MM. Carville and Polaillon in the laboratory of M. Vulpian. They were made on various classes of animals and show that the deadly action is much more rapid in mammals and birds than in molluscs, crustaceans, and fishes. On frogs under the influence of curare the poison acts much more slowly, though the respective actions of the two substances do not neutralise each other.

THE GELADA.—Several living specimens of this extremely rare Abyssinian monkey, first described by Dr. Ruppell in 1835, have quite recently reached this country for the first time, and are being exhibited at the Alexandra Park. The exact affinities of the species have never been fully determined, different biologists placing it, some with the Macaques, others with the Baboons. It is peculiar in that the male is covered with very lengthy air, like that of the Wanderoo, whilst the female is a much more ordinary-looking monkey. In the male, also, there is a bare spot in shape like an inverted T, upon the breast, which is of a bright-pink colour, becoming red and expanded into an inverted heart-shaped patch upon excitement. The tail is long and like that of a lion, having a bushy tuft at the extremity. The colour is a sooty dark-grey brown, verging upon black; the hands and feet are black; the nails are powerful and long. The size of the male is about that of a Chimpanzee four years old. The eyes are close together, and the snout prolonged. The living animal has a habit of everting the whole upper lip when irritated, and thus exposing its formidable array of teeth.

AMERICAN INSECTIVORA.—Precursory notes on American insectivorous mammals, with description of new species, by Dr. Elliott Coues, have reached us. A new sub-genus of *Blarina* is named *Loriciscus*. *Sorex sphagnicola* and *S. evotis* are new species determined by the author, whilst descriptions of *S. pacificus*, *S.* (*Notiosorex*) *crawfordi*, and *Blarina mexicana* are given from manuscripts of Prof. Baird.

COAGULATION OF BLOOD.—We notice an interesting paper by M. Fredericq, "On the Coagulation of the Blood," in the seventh number of the *Bulletin* of the Belgian Academy. The paper deals especially with fibrinogen and its transformation into fibrine. The author having discovered that fibrinogen coagulates at 56° C., *i.e.*, at a temperature far lower than the temperature of coagulation of other albuminoids of the blood, this property of fibrinogen enabled him to study the transformation of that body into fibrine, and to throw some new light on the obscure problem of coagulation of blood. The researches are to be continued.

PERSIAN AND SARDINIAN OPILIONES.—A memoir by Dr. Thorell, professor of Zoology at Upsala, has been published at Genoa containing descriptions of certain species of *Opiliones* from Persia and Sardinia preserved in the museum at Genoa, together with diagnoses of additional forms in the collection of the author, which are interesting, either as being new to science, or as having hitherto been imperfectly known. In order to advance the study of the *Phalangidea* Dr. Thorell has incorporated in this treatise a revision of the European genera, thus rendering it invaluable to every arachnologist who is desirous of studying the group.

THE DAPHNIADÆ.—In the *Berichte der Verhandlungen* of the Freiburg Society of Naturalists Prof. Dr. August

Weismann, the eminent zoologist, and Herr August Gruber publish an interesting article on their joint researches with regard to *Daphniadæ*, a family of *Entomostraca*. These investigations were principally confined to the species *Moina*, of which *M. rectirostris* has been well known for a long time. Weismann now describes two new varieties and names them *M. brachiata* and *M. paradoxa*. Other researches relate to the male forms of the species *Macrothrix* and *Pasithea*.

THE FISHES OF LAKE NICARAGUA.—Drs. Gill and Bransford have recently investigated the fauna of this lake and contributed a paper to the Philadelphia Academy of Natural Sciences. The element of especial interest is the association of characteristically marine forms with fresh-water types. Thus, together with cichlids and characinæ, none of which are marine, we have a species of megalops, a shark, and a saw-fish. A similar combination occurs in the Philippines, where, in a fresh-water lake, a saw-fish and a dog-fish are found. The megalops, however, is not known elsewhere in fresh water so isolated from the sea as Lake Nicaragua. These instances suggest caution in generalising on physiographical conditions from fossil remains. The most probable cause of such a combination is the detention and survival of salt-water fishes in inlets of the sea that have become isolated and gradually transformed into fresh-water lakes.

SOLAR RADIATION AND SUN-SPOTS

IN the year 1875 two articles by Mr. H. F. Blanford on the connection between solar heat and sun-spots appeared in the pages of NATURE (vol. xii. pp. 147 and 188), in which it was shown that Mr. Baxendell's conclusion that the sun's heat undergoes a periodical variation coinciding directly with that of the spots, appeared to be supported by the evidence of observations of the black-bulb thermometer taken at certain stations in Bengal and the neighbouring provinces. My attention has been recalled to the subject by the almost complete failure of the rainy monsoon this year in Upper India, and by the excessively high temperature ever since the middle of June, and I have been thereby led to attempt to discover whether any evidence in favour of Mr. Baxendell's conclusion, or against it, is to be obtained from the registers of meteorological stations in Upper India. I have therefore gone over the registers of certain stations in the North-West Provinces and Oudh, where solar radiation temperatures have been recorded since 1869, and at which not more than one change of instrument occurred in the interval 1869–1876. The inference I draw from these records is exactly the opposite of Mr. Blanford's. They do not afford any support to Mr. Baxendell's theory, but the energy of solar radiation appears from them to be most intense when the spots are fewest.

The reason of this discrepancy in the two sets of results will probably be found in the different modes of treatment we have adopted in extracting from the registers their evidence regarding this question. Mr. Blanford's treatment of the Silchar register, the results of which are given in his first paper, consisted in picking out certain "clear-days" on which the mean serenity at 10 A.M. and 4 P.M. was 6-10ths of the expanse, or more, tabulating the maximum temperatures of solar radiation for all these days in each month, and taking the average, neglecting the months of the south-west monsoon which are almost entirely wanting in clear days, as above defined. The results of the examination of the Darjiling register, given in the second paper, were obtained by deducting from the three highest recorded temperatures of solar radiation in each half-month the corresponding maximum temperatures in shade, tabulating these differences for each month, and taking the average. Both these devices, I think, introduce new elements of error, probably as great as those they were intended to obviate; for, as Mr. Blanford himself points out, it constantly happens that the solar radiation thermometer records much higher temperatures when the sky is partly covered with broken cloud than when it is perfectly clear, the reason being, doubtless, that the instrument is then screened to a great extent from radiation into space, while the sun comes out from time to time, and exerts his full heating power upon it. I am therefore inclined to think that the somewhat higher radiation temperatures recorded at the Bengal stations in 1870, 1871, and 1872 were probably due to the larger number of partially cloudy days in those years as compared with the years immediately preceding and succeeding them.

In the clear atmosphere of Upper India the months of March, April, May, October, and November are generally almost without a cloud, the mean serenity at stations on the plains during those months being over 7-10ths. There can, therefore be little error in taking the mean excess of the maximum temperature of solar radiation above the maximum in shade during those months in each year, as the measure of the intensity of solar radiation during the year; for the two irregularities introduced by occasional cloudy days, prevention of direct radiation from the sun to the thermometer and prevention of radiation from the latter into space, will to some extent counterbalance each other.

The following table gives these yearly means for three stations:—Chakráta, lat. 30° 40′ N., long 77° 55′ E., elevation above sea-level, 7050 feet; Roorkee, lat. 29° 52′ N., long. 77° 56′ E., elevation, 890 feet; and Lucknow, lat. 26° 50′ N., long. 81° 0′ E., elevation 370 feet.

	1869	1870	1871	1872	1873	1874	1875	1876
Chakráta ...	60°·7	57°·2	62°·7	61°·2	63°·0	65°·4	67°·6	64°·6
Roorkee ...	51·9	39·0	41·5	47·4	54·0	52·2	51·6	55·9
Lucknow ...	44·5	43·5	47·2	47·6	47·0	47·6	49·1	54·2
Mean ...	52·4	46·6	50·5	52·1	54·7	55·1	56·1	58·2

It will be seen that the lowest of these numbers is that corresponding to the year of sun-spot maximum, 1870, and the highest is that for 1876, a year of very few spots.

Unfortunately none of the thermometers in use at these stations between 1869 and 1876 had been compared either with a standard or with the others, and as the instruments at some of the stations were in the meantime replaced, these results are doubtful within the limits of the error of such thermometers. This error probably never exceeds 5°, but the difference between the numbers for 1870 and 1876, given in the table, amounts, on the average, to 11·6°. It is believed, too, that during the whole period, 1869-76, the thermometer at Chakráta was never changed, and one thermometer was in continuous use at Roorkee from 1872 to 1876. The differences in the table must therefore be the effect either of a real variation in the sun's heat, or of a greater degree of absorption than usual during the wet and cloudy years, about the sun-spot minimum. Lest they should be attributed to this latter cause, I have examined the registers of the same three stations in much the same way as Mr. Blanford did that of Silchar, and find that owing to the proportion of cloud being so very small, especially in October and November, the results are very little changed. The months of the south-west monsoon, June, July, August, and September, and those of the winter rains, December, January, and February, have been left out, and the only days counted during the remaining months are those on which the cloud proportion at four P.M., when the maximum thermometers were read, did not exceed 2-10ths. The only exception to this rule is the hill station Chakráta, where the cloud proportion for the spring months had to be fixed at one-half. The exact temperature of solar radiation thus determined varies also in the way shown above, the only apparent effect of the treatment being the introduction of slight irregularities for each station which almost disappear when the mean of the three is taken.

	1869	1870	1871	1872	1873	1874	1875	1876
Chakráta ...	60°·6	57°·6	65°·8	61°·6	64°·2	67°·0	68°·3	65°·5
Roorkee ...	51·4	38·8	42·8	50·2	53·9	50·6	51·1	53·7
Lucknow ...	44·0	43·0	45·0	48·7	47·8	51·1	49·8	53·6
Mean ...	52·0	46·5	51·2	53·5	55·3	56·2	56·4	57·6

Physicists appear to agree in the opinion that the temperature of parts of the solar atmosphere, as indicated by the great outbursts of hydrogen and other well-known phenomena, must be highest about the time of maximum spot-area; but I think the above figures will show that the question whether the amount of radiation which escapes into space is then at a maximum or not is still an open one. It must, at least, be admitted that the relative darkness of the spots is an indication of low temperature and consequent absorption. The registers of the Indian meteorological stations during the next ten years will probably give the data for determining the question, all the solar thermometers employed since the beginning of the present year having been carefully compared by exposing them side by side with an arbitrary standard before they were issued to the stations. The readings of one year will therefore be strictly comparable with those of another, notwithstanding the fact that breakages frequently occur.

It will probably be said that the very fact, now pretty well established, that rainfall is greatest in maximum sun-spot years argues increased evaporation and increased solar radiation during those years. The stations whose rainfall returns have been examined by Mr. Meldrum and those others who have worked at the subject are, however, not by any means uniformly distributed over the earth. The great majority of them are situated within or near the tropics, or in the maritime districts of temperate regions, and their more abundant rainfall in maximum sun-spot years might be easily explained by the diminished carrying power of the winds at that epoch of the solar cycle. Prof. Köppen has shown that the periods of maximum and minimum terrestrial temperature coincide approximately with the minima and maxima of sun-spots, and that both the maximum and minimum annual temperatures are reached somewhat sooner in the tropics than in the temperate zones. One would think, therefore, that the great convection currents of the atmosphere, depending on differences of temperature, would be least powerful a little after the maximum of sun-spots which is the period of heaviest tropical rainfall, and blow most strongly after the sun-spot minimum, the period of least rainfall in the tropics. The only data I know which would enable one to form an estimate of the rainfall of a large inland area in the temperate zone during a long term of years, are embodied in a diagram of the fluctuations of level of the North American lakes, given in a paper by Mr. G. M. Dawson, in NATURE, vol. ix. p. 506. The diagram shows a remarkable coincidence between the variations of the level of the lakes and those of the sun-spot area, and the inference Mr. Dawson draws from a comparison of the two curves appears to be that high water in the lakes is the result of great solar activity when the spot-area is large. Since the appearance of this paper in 1874, it does not appear to have struck any of the readers of NATURE that, in every instance except one, high water in the lakes preceded the sun-spot maximum by two or three years, and, in like manner, the lowest level was reached several years before the sun-spot minimum; but a reference to the diagram will show that such is the case. It is evident, therefore, that high water in the lakes cannot be a consequence of numerous sun-spots, but it may be a commutative effect of greater evaporation than usual and greater carrying power in the winds during the few years of high temperature succeeding the sun-spot minimum.

I have not at hand any means of ascertaining the relative velocities of the wind at European stations during the years about the maximum and minimum epochs of the solar cycle, but the anemometer records of Indian stations shows that the wind velocity varies directly with the temperature. The following table gives the mean velocity each year at five stations, as measured by a Robinson's small anemometer. The figures represent miles per diem:—

	1869	1870	1871	1872	1873	1874	1875	1876
Calcutta	—	—	122	125	133	140	120	133
Hazáribágh ...	—	—	128	140	157	160	172	188
Benares	79	58	54	68	74	93	116	128
Agra	121	180	100	94	86	97	102	103
Bareilly	124	114	89	64	67	72	75	—

If the yearly average for the first four stations be taken, it will be seen that there is a regular increase in the velocity of the wind from 1871, the probable year of lowest terrestrial temperature, to 1876, which was probably the hottest year of the period. It is, therefore, I think, at least possible that the excess of tropical and oceanic rainfall in maximum sun-spot years may be caused by precipitation near the place of evaporation, owing to the diminished force of the trade-winds and anti-trades at those periods, and that if the *winter* rainfall of Europe and America were examined, it might show an excess in *minimum* sun-spot years, derived from vapour brought by an unusually strong upper current from regions of great evaporation in the South Atlantic.

The registers of nearly twenty years show that the winter rainfall of India, north of the tropic, is probably subject to such a periodic variation, and if this surmise be verified in the future it may prove to be of the greatest economic importance. Last cold weather these rains were unusually abundant, and enabled the cultivators of Northern India to grow a spring crop sufficient not only for their own wants, but for export to Europe as well as to the famine-stricken districts of Madras and Bombay. Both this year and last the regular summer rains have been far below the average, and almost any day since last June the vapour that in an ordinary year would have come down to fertilise the soil might have been seen passing overhead in the form of light cirrus drifted by a strong south-west wind. The moist easterly current from the Bay of Bengal, from which a large proportion of the rainfall of the Gangetic valley is generally derived, has this year scarcely penetrated as far west as Benares. The natural consequence of this failure of the rains will be a famine in Northern India, unless, next cold weather, we get the heavy rains which the experience of past years leads us to expect.

A comparison of the mean temperature, vapour tension, humidity, and rainfall for the month of July in the years 1875, 1876, and 1877, shows that the extraordinary dryness of the present year is the result not so much of the absence of aqueous vapour from even the lowest stratum of the atmosphere as of the abnormally high temperature which prevents its precipitation. This will be seen from the following table:—

		Mean Temperature.	Vapour Tension.	Humidity.	Rainfall.
		°	″	per cent.	inches.
July,	1875 ...	81·4 ...	·984 ...	84 ...	26·33
,,	1876 ...	85·4 ...	·807 ...	75 ...	10·05
,,	1877 ...	90·3 ...	·862 ...	63 ...	2·23

The figures in the table are deduced from observations taken at Allahabad four times daily, viz., at 10 and 4 A.M. and P.M. The year 1875 was marked by unusually heavy local rains in July which laid a great part of the surrounding district under water, so a comparison of 1876 and 1877 with 1875 is hardly fair. The average rainfall of the month is 14·65 inches.

S. A. HILL.

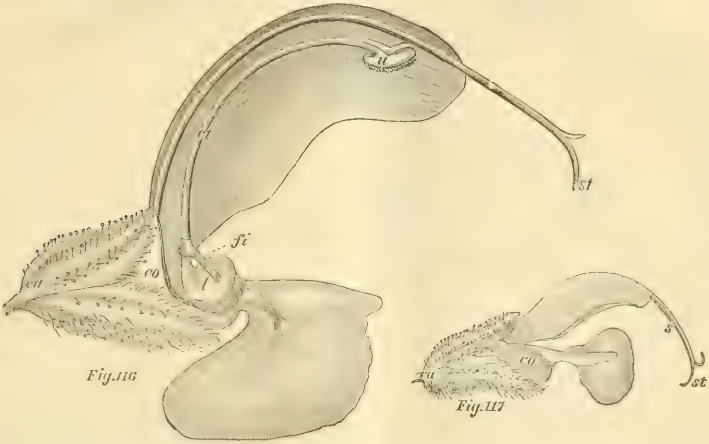
Allahabad, August 29

FERTILISATION OF FLOWERS BY INSECTS[1]

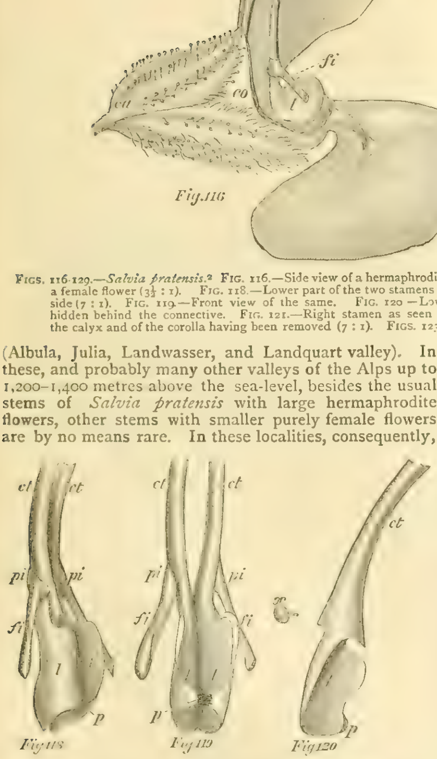
XVII.

Abortion of all the Stamens in a Flower in Four Successive Periods

IN the theory of the development of the organic world useless and aborted organs are always of especial interest, as no other plausible explanation of them can be given except that they are inherited from ancestors to which, in other conditions of life, they were useful; it may therefore be worth referring to a flower in which, in four successive periods, all stamens have been aborted, and accordingly four different degrees of abortion are to be distinguished. The species in which these flowers are found, *Salvia pratensis*, is a very common one, but the flowers alluded to either do not occur at all in the usual habitats of this species, or have hitherto been overlooked by most botanists. I found them during my last excursions in the Alps in some valleys of Switzerland (Albula, Julia, Landwasser, and Landquart valley). In these, and probably many other valleys of the Alps up to 1,200–1,400 metres above the sea-level, besides the usual stems of *Salvia pratensis* with large hermaphrodite flowers, other stems with smaller purely female flowers are by no means rare. In these localities, consequently, *Salvia pratensis* is in the same state as Glechoma,[3] Thymus, and some other Labiatæ in all or most of their habitats; *Salvia pratensis* is here, as Mr. Darwin calls it, in his late work,[1] a *gynodiœcious* plant. In all other gynodiœcious Labiatæ two abortions of stamens have occurred in two successive periods; in *Salvia pratensis*, as I shall show, four.



FIGS. 116-129.—*Salvia pratensis*.[2] FIG. 116.—Side view of a hermaphrodite flower with the corolla partly removed ($3\frac{1}{2}$: 1). FIG. 117.—Side view of a female flower ($3\frac{1}{2}$: 1). FIG. 118.—Lower part of the two stamens of a hermaphrodite flower viewed obliquely from the front and from the right side (7 : 1). FIG. 119.—Front view of the same. FIG. 120.—Lower part of the left stamen alone seen on the inner side; the filament being hidden behind the connective. FIG. 121.—Right stamen as seen on the outside (7 : 1). FIG. 122.—Side view of a female flower, the half of the calyx and of the corolla having been removed (7 : 1). FIGS. 123-129.—Gradations of abortion of the two last stamens (7 : 1).



1. The Labiatæ, as well as the Scrophulariaceæ, have apparently descended from plants with five stamens. But as soon as the common ancestors of the Labiate family adapted their flowers to cross-fertilisation by bees in such a manner that their stigmas and anthers must necessarily be touched by the backs of these visitors, the uppermost of the five stamens stood in the way of the style, which for the purpose of this cross-fertilisation must stretch along the middle line of the upper side of the corolla and bend one of its two stigmatic branches downwards. Thus the uppermost stamen having become not only useless, but even directly disadvantageous, was doomed to abortion, and in the long time that has elapsed since then, has been so completely eliminated by natural selection, that not the smallest trace of it has remained, and only very exceptionally does it reappear by atavism. In those Labiatæ in which the adaptation described has been perfected, the reappearance of the fifth stamen happens, indeed, so extremely rarely that I have only once had the opportunity of seeing it, in a single flower of *Lamium album*, in which the upper lip was wanting, and, instead of it the fifth stamen was present. In the flowers of Mentha, however, in which the peculiarities of the Labiatæ just-mentioned are much less developed, the fifth stamen, as I am informed by Dr. E. Krause, of Berlin, reappears more frequently.

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[1] Continued from vol. xv. p. 475.

[2] In all figures *ca* = calyx, *co* = corolla, *n* = nectary, *ov* = ovary, *s* = style, *st* = stigma, *x* = rudiments of the two aborted upper stamens, *fi* = filaments of the two lower stamens, *ct* = connective, *u* = upper anther-cell, *l* = lower anther-cell, *p* = point of union of the two metamorphosed lower anther-cells, *pi* = point of the filament on which the connective rotates.

[3] See NATURE, vol. viii., pp. 121, 143, 161.

[1] "On the Different Forms of Flowers in Plants of the Same Species."

2. Whilst the flowers of Labiatæ are generally adapted to be fertilised only by bees of a certain size, smaller ones entering the flowers without touching either the stigma or the anthers; in the genus Salvia, on the contrary, larger and smaller bees have been equally engaged in the service of intercrossing. This has been effected by the following modifications :—The stigma bends further downwards, and the connective of each of the two lower stamens has been transformed to an upright, two-armed lever, which, at its two opposite ends, bears the two anther-cells, and, by a slight pressure on either of them, turns on the filament, so that any bee entering the flower cannot but strike against the two lower anther-cells with its head, cause the connective to rotate, and thus bring the dehiscent surfaces of the upper anther-cells into close contact with its back. In direct connection with this transformation of the two lower stamens, the two upper ones, which would hinder the rotation of the levers, have aborted. But in contrast with the uppermost stamen, which has become superfluous at a much earlier period in the ancestors of the whole family Labiatæ, and has since completely disappeared, the two upper lateral stamens, which have become useless not earlier than in the ancestors of the genus Salvia, in all or most of the species of Salvia still exist in the form of two little knobs (*x*, Figs. 120-130).

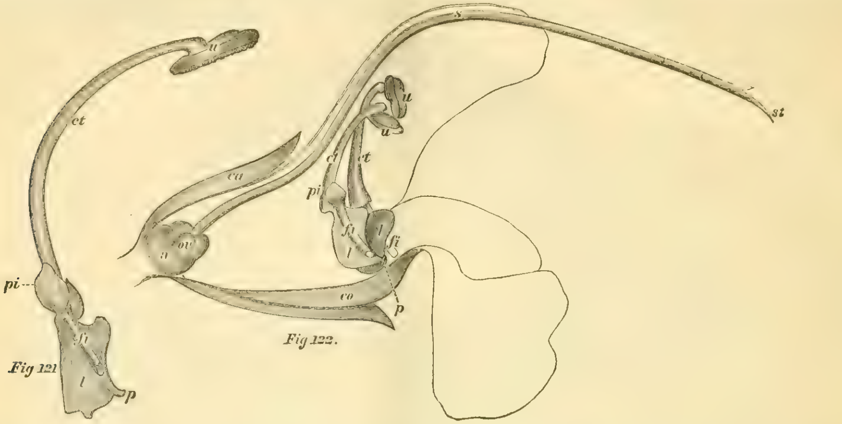

Fig 121

Fig 122.

3. By the transformation just-mentioned of the two lower stamens in the genus Salvia not only have the two upper stamens become a hindrance, and thus been aborted, but at the same time the lower anther-cells of the two lower stamens have been alienated from their original function

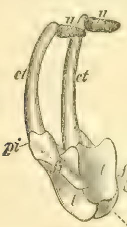

Fig 123.

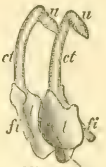

Fig 124.

and engaged in a new service, by which a sterilisation and metamorphosis of these has also been occasioned. *Salvia officinalis* and *pratensis* show us two steps of this further modification. In *S. officinalis*[1] the connective (*ct*, Fig. 130) is but moderately lengthened, the two lower anther-cells, although reduced in size, still commonly produce some pollen, but sometimes they are quite sterile; they are not yet grown together, but only adherent to each other so as generally to move in company. In *Salvia pratensis*, on the contrary, the two lower anther-cells are not only always completely sterile, but also metamorphosed into two concave plates (*l*, Fig. 118-120) firmly grown together in front (at the point *p*, Fig. 118-120), so that they act as a simple plate, which, when pressed by the head of a humble-bee, causes the two connectives to rotate, and brings the pollen of the two upper anther-cells into contact with the back of the visitor.

4. In the small female flowers of *Salvia pratensis*, also, whatever may have been their origin,[1] the last two

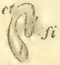

Fig 125

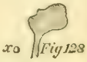

Fig 128

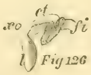

Fig 126

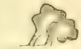

Fig 129

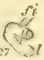

Fig 127

anther-cells have become sterile, increased fertility of the small-flowered plants probably also in this species, as in other gynodiœcious Labiatæ,[2] compensating for the decrease in the size of the flowers and the loss of pollen. With the loss of pollen the whole machinery of the two-armed levers, which had been so gradually acquired and so exactly brought the pollen on the back of the visiting humble-bees, has become useless and begun to abort, and, according to its new origin, this last abortion, as is shown by Figs. 123–129, still offers various gradations from the perfect mechanism to an insignificant little flap. In this gradual succession of more and more reduced stamens of *Salvia pratensis*, we find some forms (Figs. 125, 126) with a striking resemblance to the stamens of *Salvia officinalis* (Fig. 130), and some of the steps which are run through by this process of abortion seem to be quite analogous to those by which in former periods the stamens of *Salvia pratensis* have reached their astonishing singularity.

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[1] The peculiarities of *Salvia officinalis* have been fully and excellently described and explained by Dr. William Ogle. (*Popular Science Review*, July 1869, p. 261-267.)

[1] I have attempted to give an explanation of the origin of the small-flowered female form of the gynodiœcious Labiatæ in NATURE, vol. viii., p. 161. This explanation, however, is not in accordance with Mr. Darwin's views published in his newest work, "On the Different Forms of Flowers."

[2] As shown by Mr. Darwin, "On the Different Forms of Flowers," pp. 299-309.

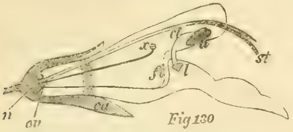

FIG. 130.—*Salvia officinalis.* Flower of *Salvia officinalis* bisected longitudinally.

Briefly, the original five stamens of the flower have aborted at the following four successive periods:—

1. The uppermost stamen, in the ancestors of the family Labiatæ (complete disappearance).
2. The two upper lateral stamens, in the ancestors of the genus Salvia (reduction to little knobs).
3. The two lower anther-cells of the two lower stamens, beginning to abort in *Salvia officinalis*; abortion and metamorphosis perfected in *S. pratensis*.
4. The two upper anther-cells of the two lower stamens, in the small-flowered plants of *S. pratensis* (abortion of the pollen perfected, abortion of the anther-cells and the whole stamens beginning).

HERMANN MÜLLER

Lippstadt

THE RESTORATION OF THE ANCIENT SYSTEM OF TANK IRRIGATION IN CEYLON

A WORK apparently pregnant with the largest and most beneficent results to the native population of Ceylon is in process of being carried out by the Colonial Government of that island. More than a thousand years ago a system of irrigation, the most complete and remarkable that the world has ever seen was in successful operation in the Low Country, and the object which the Government has in view is to restore to something like its pristine fertility a large proportion of the immense tracts of land—many hundreds of thousands of acres in extent—that for want of water have fallen into a condition of the most utter sterility. Sir Emerson Tennant, writing twenty years ago on this subject, says, "The difficulties attendant on any attempt to bring back cultivation by the repair of the tanks are too apparent to escape notice. The system to be restored was the growth of 1,000 years of freedom, which a brief interval of anarchy sufficed to destroy, and it would require the lapse of long periods to reproduce the population and recreate the wealth in cattle and manual labour essential to realise again the agricultural prosperity which prevailed under the Singhalese dynasties. But the experiment is worthy of the beneficent rule of the British Crown under whose auspices the ancient organisation may be restored amongst the native Singhalese."

The origin of the system of irrigation spoken of dates as far back as the year 504 B.C., when, according to the Singhalese Chronicle, Mahawanso, the first tank was built in the neighbourhood of his new capital, Anuradhapoora, by Panduwasa, the second of the Hindu Kings. This was succeeded about seventy years later by two others formed in the same neighbourhood. In the year 459 A.D. the Kalawewe Tank, the largest of all, was completed. The retaining bund of this immense sheet of water is twelve miles long, and the circumference of the lake which it formed was no less than forty miles, the water being backed up for a distance of fifteen miles and conducted from the tank by means of a conduit sixty miles in length to the capital. Sir Emerson Tennant in describing these remarkable reservoirs, says, "Excepting the exaggerated dimensions of Lake Mœris in Central Egypt, which is not an artificial lake, and the mysterious basin of Al Aram in Arabia, no similar constructions formed by any race whether ancient or modern exceed in colossal magnitude the stupendous tanks of Ceylon." The same author estimates that at the time of its greatest prosperity the island contained a population of from fifteen to twenty millions, nearly all of whom must have derived their means of sustenance from irrigated lands. At the present moment, after all the care bestowed through three-quarters of a century by a paternal government, the population only amounts to 2,400,000, whilst even for this a large proportion of the food—6,000,000 bushels of rice annually among other things—has to be imported from India, and the population itself must be considered to have been somewhat unnaturally increased during the last fifty years by the stimulus of European enterprise. The mass of the people too have changed their place of residence from the interior to the neighbourhood of the sea-coast, where trading and fishing instead of rice-cultivation furnish them a livelihood. The vast areas which formerly under the magic influence of a sufficient supply of water and a hot sun, produced their two or three crops of rice in a year are now absolutely deserted, frequently not a single inhabitant surviving where once a thousand found ample means of subsistence. The city of Anaradhapoora, if its ruins afford us any means of estimating its magnitude, must have covered an immense area—no less than from thirty to forty square miles, and the population living on the spot and drawing its supplies of food from the immediate neighbourhood must have been correspondingly immense. Now it is a mere village in the midst of vast heaps of ruins.

One of the most gigantic of these early irrigation works is supposed to have been originated by Maha Sen about the year 275 A.D., and, having been enlarged by Prakrama, Bahu I., who reigned in 1153, to have received from him the name of "The Sea of Prakrama." It consisted of a series of lakes formed by an embankment twenty-four miles in length and from forty to ninety feet high, by which the water of a large river and many considerable streams was hemmed in along the base of a range of hills and so forced into the valleys that a series of lagoons or lakes was formed extending for the above-mentioned distance and frequently several miles in width. A canal five miles in length conducted the waters of "the sea" to the Minery Lake, another of the works of Maha Sen, to be mentioned presently, and a further canal from Minery led the waters to the neighbourhood of Trincomalie, in all a distance of fifty-seven miles. When it is remembered how sudden and torrential the rains are in a country like Ceylon—the writer has known 18 inches of rainfall in forty-eight hours over a very large extent of country, and at one spot as much as 18·9 inches in twenty-four hours,—we cannot too much admire the vastness of such a work and the skill which enabled the native engineers to use the natural features of the country in such a manner that for a distance of twenty-four miles a single embankment sufficed not only to hem in the water for purposes of irrigation but also to provide a water-way for the transport of produce and merchandise. Along the whole course of this embankment and canal and wherever its tributaries carried the life-giving water there would be without doubt a teeming population; for irrigable land in

Ceylon is capable of supporting, according to official calculation, 1,000 persons to the square mile. In 1855 there was not a single inhabited village, although a few patches of land were occasionally cultivated by people from a distance. The contrast between the remote past and the present condition of this half of the island is a painful one to contemplate, but it is to be hoped that the Colonial Government will never stay its hand until all the useful works of ancient times have been restored and improved—but this will be a work of centuries.

Long before the Christian era the main ambition of the kings of Ceylon appears to have manifested itself in the formation of tanks, and many kings are mentioned in the Mahawanso who, "for the benefit of the country," and "out of compassion of living creatures," built a dozen or more of these splendid, but absolutely necessary, irrigation works. The Minery tank, some twenty miles in circumference, and irrigating an enormous area of fertile land now entirely barren, owed its origin, along with sixteen others, to Maha Sen, who reigned about the year 250 A.D. It is now merely a swamp, resorted to by enormous numbers of wild fowl. Up to the twelfth and thirteenth centuries Ceylon produced her own supplies of food, but in the fourteenth it appears that the island was obliged to import a portion of it from India. In 1301, it is related that there were 1,470,000 villages in Ceylon. In 1410, as many as 1,540,000, the term village implying hamlet, or even a single house where there are people resident. Of the vast majority of these, if they ever really existed, not a vestige is left except the ruined tanks, which show unmistakably where the foci of population formerly were. This was shortly after the conquest of the island by the Malabars, who are believed not to have actually destroyed the fabric of the embankments, but by their system of government to have disorganised the village communities to such an extent that the works connected with the tanks fell into disrepair through neglect, the land became imperfectly irrigated, and the population gradually died out. That this process was a perfectly natural one seems evident from the fact that the tanks do not show any traces of wilful damage, and also from the consideration of the almost innumerable evils resulting in death, of which a scarcity of water in a tropical country like Ceylon is productive. Indeed one of the most frighful diseases that have ever scourged the human race is believed to have been developed in these very localities chiefly through the want of proper food, caused by the absence of a system of irrigation. It is believed, too, and there is strong evidence, based on experience, for the belief that the disease entirely disappears wherever irrigation is restored. It will naturally be asked, "If the advantages of a plentiful supply of water are so enormous, why have not the tanks been restored before this, and what hinders their immediate restoration at the present time?" The reply is, that the creation of this magnificent system of irrigation was not the work of a decade, or even of a century, but of a thousand years of successful national development, and that therefore the restoration of it must be also a work of time.

The object of this paper is to draw attention to the fact that the experiment of restoration is at the present moment in process of being tried, and bids fair, after the lapse of half a century or so, to alter entirely the character of the island. The most remarkable success has already attended the efforts to afford irrigation facilities to the Singhalese on the East Coast. Where but a few years ago the natives where half-starved and the land apparently in a hopeless condition, the re-introduction of irrigation through the assistance of the Government, has transformed not only the people, but the country, as if by magic. Rice-fields, palms, and other fruit-trees abound, and the population is increasing at a rapid rate. Of this particular district the present Governor of Ceylon (Sir William Gregory), reported some four years ago to the Legislative Council of the island in the following terms:—"In the month of April I visited the rice-growing regions of the Eastern Province, which are the creation of the irrigation works carried out by the Government. I never before saw such an unbroken sheet of grain. Save where some isolated trees, part of a recent forest broke the view, the eye wandered over some 20,000 acres of green paddy. I saw, wherever I went, a sleek, vigorous, well-fed, and thoroughly healthy population. Up to 1864 the lands under cultivation in this province were 54,000 acres, the chief impetus to the irrigation scheme having been given in 1857. In 1871 the lands in cultivation were 77,000 acres. The Crown lands to be additionally reclaimed under works already completed or in course of completion, amount to 15,900 acres, equal to the support of 23,850 persons." Again, speaking in the same report on the subject of the Great Tank already mentioned, he says: "I am most anxious to put the full strength of the department at work in restoring irrigation to Nuwara Kalawia. This magnificent district has the strongest claims upon us. It was once the granary of the island. It is now utterly neglected. It has a population of 60,000 persons and over 1,600 villages, which have each of them their tank. There are at least 1,700 of these tanks, and I am credibly informed not one of them has a sluice in order. I trust that a few years hence the population may present the same vigorous and thriving appearance as the population of the Eastern Province, and from the same causes—namely, good and plentiful food." Of this same district a gentleman of very great experience told the writer that in travelling through it many years ago he came to a village where, of the thirty inhabitants, only one of them was able to carry water, all the others having been stricken down by hunger or disease. This destitution was caused by the failure of three successive rice-crops, and was not specially exceptional, but fairly representative of what takes place frequently in the district. If we compare the scenes of plenty and contentment as they exist in the Eastern Province at the present moment with what meets us in the Wanni, or in any of the northern districts, where tanks have not been extensively repaired, the contrast is most striking. We find an almost depopulated country, with here and there a wretched village peopled by a few miserable and more than half-starved inhabitants, who, in times of scarcity, which are not infrequent, are obliged to live on roots and wild herbs, who are periodically decimated by a frightful disease, yet who seem bound to the spot where they were born, and prefer to die there rather than move away to a more fertile and healthy district. It is, indeed, this disinclination which possesses the agricultural Singhalese to move more than a day's journey from his home that presents the greatest of all difficulties to the scheme for the restoration of the tanks. It is on this account that the process of restoration is always in advance of the supply of natives to take up the new land, unless the works happen to be in the immediate neighbourhood of population. The only plan, therefore, that has proved really successful under present conditions is to restore the tanks in the vicinity of villages, and induce the population to creep slowly onwards step by step, cultivating the more fertile pieces of ground as it advances, until the depopulated districts shall have been partially reclaimed, when the completion of the work will be a matter of comparative ease. Two typical instances of this mode of procedure have been mentioned to me by an official high in the Government service, as showing the effect of a well-regulated expenditure of labour and money in restoring irrigation works. In the year 1854 Mr. Bailey, whose name will ever be associated with this scheme for benefiting the natives, spent less than 100*l.* on a canal some miles to the north of Matalé, a country town a few miles north of Kandy. The village thus supplied with water had previously dwindled away until only three houses were left, the rice-fields were deserted, and the famine-stricken inhabitants declared that they would die where their fathers had lived and died rather than migrate to a part of the country that was unknown to them. Ten years after the improvement was made the spot had become a little oasis in the desert; nearly 200 acres of rice were under cultivation, yielding about thirty bushels per acre, and supporting a population of several hundreds.[1] Almost in the same neighbourhood a sum of between 200*l.* and 300*l.* was spent on an old canal fifteen miles in length by the same zealous Government official already mentioned. Many hundreds of acres were brought under cultivation, and in ten years' time, instead of a starved and fever-stricken population of 150 inhabitants, no less than 500 able-bodied men were on the list as liable to the road-tax. The changes in these, as in other instances, took place as if by magic, yet the means employed in effecting them were of the most limited and simple nature. The secret of the success lay in the fact that a famishing and disease-smitten population was within a few miles of the spot, and the remnants of ancient engineering skill were ready at hand to guide the labourers on to certain success. Since the above tentative experiments were made, very great changes for the better have taken place in the condition of the agricultural part of the native population. The carrying out of the scheme for the restoration of irrigation works is recognised as one of the chief duties of the Colonial Government, and there is little danger that, after the real success which has attended it so far, any future Government will allow it to be interrupted. The policy of the Colonial authorities may be summed up in the pregnant words of Sir Wm. Gregory's address to the Legislative Council in 1876:—"I consider that at least 100 tanks should be supplied with sluices, and properly repaired each year; and I have asked the Secretary of State to furnish me with an additional number of well-trained officers, by whom these works will be carried on with vigour. There is no boon which the Government can confer on the villagers more legitimately than this. It is a reward for their own exertions, and I am confident that each year, as it becomes better understood, it will be more appreciated, and that it will be recognised everywhere that the Government have no other object in it than to increase the comfort and resources of the people." It will appear, from what has been quoted, that the tanks are not repaired free of cost and then handed over gratuitously to the villagers, but the natives are required to give a certain amount of labour in restoring the tanks, and also to pay a small rent or tax on the land cultivated, so that, whilst the native cultivator is the chief gainer by the undertaking, the Government is no loser. If there could have been a doubt as to the wisdom of the Tank Restoration scheme, the experience of the last three years must have dispelled it and proved how absolutely necessary a system of irrigation is to the welfare of the natives. In the address above quoted, whilst speaking of the cholera and other diseases which had visited several of the provinces, the Governor says:—"It is remarkable that the inhabitants of the Eastern Province enjoyed perfect immunity from epidemics of all kinds. It is an interesting question, on which I do not give an opinion, whether this general immunity from disease in the Eastern Province is due to the abundant supply of food throughout the populous part of it, the result of irrigation works." At the same time he speaks of the restoration of two of the large tanks as complete. One of these will irrigate 23,000 acres, equal to supporting a population of 35,000 persons; the other will bring large tracts of magnificent land into cultivation, and dissipate the unhealthiness of the district which has hitherto prevented settlement.

To look back over the early history of the attempts under Sir Henry Ward to restore the above system of irrigation, is like reading the accounts of the commencement of a successful campaign. The difficulties encountered were sufficient to discourage even enthusiastic philanthropists, chief amongst them being the utter disorganisation of the village communities through the abolition of compulsory labour and the rooted dislike of the natives to migrate from one spot to another. For the recent part of the evil caused by this disorganisation the British Government was alone to blame, for in abolishing Rajekaria they abolished the right of compelling villagers to keep their tanks and watercourses in repair. By doing this they practically placed the distribution of the most valuable property of which the natives were possessed in the hands of the strongest, and consequently the most unscrupulous, inhabitants of each district. In a dry season, when there was barely sufficient water to irrigate the fields along the course of a canal, those who who were nearest to the source of supply would probably get more than their share, whilst those who were furthest from it and had an equal claim on it might get none; but, generally, the strongest party would get the advantage, to the ruin of the weaker. Dams would be built at various points along the course of the stream by one party, and as quickly destroyed by another. Interminable feuds were the results, and appeals to the courts of law, which, not being guided by native customs, only made matters worse. The canal, too, which ought to have been kept in proper repair by the united efforts of all who benefited by it, was allowed to fall year by year into a more ruinous condition, after compulsory assistance had been abolished, the residents on the upper portion of it refusing to aid those on the lower to repair the breaches made by the annual floods. Consequently the work that was done was ill done, and only of a temporary character. Soon it became beyond the power of isolated communities to effect the necessary repairs; the lands fell out of cultivation, and the population, after a long struggle with their neighbours, either died out or sought a living elsewhere. The early legislation in 1856 was based on a revival of the native customs and a compulsory distribution of the necessary work among th different villages, a majority of two-thirds of the inhabitants being enabled to place the lands under the Irrigation Ordinance, and to compel the assistance of all who benefited by the supply of water. The scheme resulted in complete success. It met the great want of the natives and the interminable disputes about boundaries and rights of water, which was as much property to the natives as the land itself, soon ceased. The Government claimed its own and sold large portions of it by auction at a very reasonable rate, the upset price being generally 1*l.* per acre, the land continuing to be chargeable with a yearly tithe to the Government of from 3*s.* to 4*s.* per acre. In special cases the Government granted even easier terms in order to induce the natives to settle in particular localities. Newly-purchased land was allowed to be free from tithes for four years, and the purchase-money was spread over an equal period from the time of sale. The pecuniary result was most gratifying to the Government, and the benefit conferred on the natives inestimable.

A few words will be sufficient to describe the character of the cultivation which this system of irrigation is intended to promote. A crop of rice, or paddy, as the undressed grain is called, requires about ninety days to come to perfection, and during this time it must be supplied with about thirty inches in depth of water, or a little over 4,000 cubic yards to the acre. The first and second watering of the paddy takes place within a fortnight of the sowing of the seed, and the water is only allowed to remain on the land for a short time. The three subsequent waterings take place about the twentieth, the fortieth, and the sixtieth days after sowing, from eight to ten inches of water being used each time, and the water is allowed to remain on the land until it has evaporated. This system, though more or less modified according to the climate and the supply of water, is fairly representative of rice-cultivation in the lowlands of Ceylon. The official estimate of the produce is about thirty bushels per acre. It is probable that exactly the same system existed in the very earliest times, and that the Singhalese engineers were able to regulate the flow of water through the tank sluices just as they wished. It certainly seems unreasonable to suppose that the men who could design such a vast irrigation system with no better means of levelling than that of leading water by actual experiment from one point to another, should fail in minor matters such as sluice-gates. Yet the writer believes that nothing is known as to the manner in which the flow of water was regulated. It is true that in some of the sluices a square masonry well is found leading upwards from the sluice soon after it has entered the embankment from the tank, but there is nothing left to show how it was used. Captain Sim, R.E., some years ago suggested that it was intended to break the force of the water rushing in flood-time towards the sluice and reduce the velocity of the water in the sluice to that due to the pressure in the well only. I am however inclined to think that a frame of wood somewhat in the shape of a box strongly braced together was fitted into the well so that it could rise and fall readily under the influence of the water in the tank, and that by placing weights on the top the frame might be forced down so as to cut off either partially or wholly the water issuing through the sluice. Wherever rocky foundations could be found for a dam or a ledge of rocks for a spillwater, the native engineers, as if distrusting artificial constructions, would be sure to utilise them. In some cases, where it was possible to include masses of rock in the embankment, the sluices themselves would be cut out of the solid gneiss and the work thereby rendered as indestructible as the rock itself.

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[1] Irrigated rice-lands in the low country will support population at about the rate of 1,000 persons to the square mile.

It will no doubt be somewhat surprising to persons who are only acquainted with the system of rotation of crops in vogue in Europe, that these rice-lands can be made to produce year by year for hundreds of years consecutively, one or two crops of grain annually without the land becoming exhausted or requiring to be continually renovated by manure. The explanation, however, seems to be that sufficient vegetable matter is carried down from the hills partly in solution and partly in suspension in water to supply all the waste produced by the continuous cropping. Those who have visited the richest alluvial valleys of California and Australia will no doubt have been struck by the fact that the most fertile soil is always found where the alluvium has been deposited in extremely fine particles and in water practically at rest, conditions which obtain in the paddy fields of Ceylon, and must have obtained formerly on the Hunter River in New South Wales, and in the valleys opening on the Bay of San Francisco.

I cannot better conclude this paper than with an extract from a minute by Sir Henry Ward, after a tour of inspection in 1859:—

"The village of Samantorre is a very fine one, and stands on the borders of the richest plain in Ceylon, containing, as it does, nearly 15,000 acres of paddy. Mr. Birch and Mr. Cumming informed me that the scene of joy and excitement exhibited by the whole population when the water first came down from the Ericammam, in July, 1858, and saved a magnificent crop from destruction by drought, was one of the most striking things ever witnessed. Hundreds of people had collected at Samantorre as soon as they knew that the sluices were to be opened; and when the water was actually seen advancing down the bed of the dried-up river, the shouts, the firing of guns, the screams of the women, the darting off of messengers bearing the news in every direction, made a deep impression on all who saw it. They felt that a great work had been done, a great benefit conferred. But I feel also that under British rule this benefit ought to have been conferred thirty years ago upon a people so capable of appreciating it. Indeed, knowing what I now know of the history of the Eastern Province, I hold that what the Government is doing in 1859 is simply the payment of a debt incurred by our rash interference with a people of whose habits and wants we knew nothing. This error is now in part repaired. 44,000 acres of land are already under paddy cultivation, and I see reason to believe that the amount will be not less than 60,000 acres in 1861, when the irrigation works have obtained their full development. But this will require constant attention on the part of the Government and of its local representative. The maintenance of the *system* must never be lost sight of, and should unforeseen demands for assistance arise they must be met liberally and promptly." The words of so successful a governor have not been forgotten. The present governor, Sir William Gregory, has devoted all his energies to the carrying out of what was so well begun. The survey and engineering staff of the colony has been considerably increased, and the restoration of nearly the whole of the ancient irrigation works, besides the creation of new ones, may now be considered to be only a question of time.

R. ABBAY

NOTES

THE *Times* devoted a leading article last Thursday to Mr. Forster's remarkable speech at Bradford, in which he attempted to indicate the latest ideal of what elementary schools and universities ought to be. The *Times*' summary of the points of Mr. Forster's address is very satisfactory. "Mr. Forster's notion of a public elementary school is very unlike that which has been commonly entertained. The school is to be for the benefit of all classes. All subjects are to be taught at it, with no other limitation than such as may be imposed by the wants or capacity of the scholars. The secondary school is to supplement the teaching of the primary school, and to do for boys of a more advanced age the same sort of work which the primary school has done for them up to the age of thirteen or fourteen. That science should be introduced as a regular part of the school course is, in Mr. Forster's opinion, most desirable. History and geography he considers, indeed, as of even greater importance than science, but he places the claims of science above those of grammar, and seemingly above those of the study of language in any form. He is very hopeful that the older universities will consent to model themselves on the plan he suggests, and to grant degrees for science without insisting on Greek and Latin in addition to it. If they will not do this, or if they are very long about it, he will look to younger bodies, untrammelled with literary traditions, to take their place in this matter. What Oxford and Cambridge may refuse, Manchester and Leeds will make no difficulty in granting, and a combined university for the North of England is to be set up accordingly and invested with the necessary powers." The *Times* does not seem to know very well its own mind on the subject referred to by Mr. Forster. It clings to the old ways, and virtually confesses that the new ways are as indispensable as the old, that some knowledge of science is now indispensable to all. Mr. Forster declares we have no right to erect Board Schools and compel children to attend them, only to give them a smattering of the three R's. The purpose of these schools—which he, with many others, thinks ought to be open to all classes, and afford an elementary education which would be considered adequate by any class—is to fit the children who attend them to make a fair start in life, and in this scientific age, as the *Times* virtually admits, no one can be said to have a fair start if he be ignorant of at least some of the results of science. Mr.

Forster rightly believes that science affords at least as good a mental discipline as the study of languages as the latter is carried on at our schools and universities; and indeed, there need be no dispute on the matter as there now exists abundance of material for comparing the mental power of the scientific man with that of the pure literary man. We need scarcely repeat the argument that to omit science from education either in the school or university, is to leave at least one half of the mind untrained, and that too in more respects than one, the most important half. In the issue of the *Times* that contains the leader referred to, it is reported that the School Board for London is to move Government to establish one or more secondary schools in each School Board district, to which such children may be transferred as prove an aptitude for carrying their studies further. Thus so experienced a body as the London School Board are driven to the conclusion that elementary education as at present conducted is an inadequate provision for the wants of our youth. It is simply a question of time, and a very short time too, the introduction of science into schools of all grades. The old universities have been driven to it, and even Mr. Forster is struck with their modern liberality.

The French Geological Society has appointed a large committee of organisation in connection with the International Geological Congress to be held in Paris in 1878, the plan of which we described in vol. xv. p. 87. The president of the committee is Prof. Ed. Hébert, and the secretary Dr. Jannettaz. We trust that English geology will be well represented at this congress. At the recent meeting of the American Association Prof. Sterry Hunt presented the report of the general committee of the proposed congress. A circular in English, French, and German, had been sent by the secretary to the principal scientific societies and academies, as well as to the workers in geology throughout the world. The response to this invitation has been most gratifying. The Geological Society of France has formally recognised the great importance of the objects proposed, and promised its hearty co-operation. Spanish and Italian geologists have translated and published the circular in their respective languages, and have communicated to the secretary their strong approval of the plan. The Geological Society of London and the Geological Survey of Great Britain have also formally signified their approval of the objects, and the co-operation of Norway, Sweden, Russia, and Austro-Hungary is promised. It is to be regretted that Germany has declined to take a part in the International Exhibition of 1878, but it is hoped that this will not prevent her geologists from joining in the proposed congress. The director of the Geological Survey of Japan promises to take a part in the work, and the same assurance comes from Brazil, where the circular has been translated into Portuguese. Chili and Mexico have also responded, and promise an ample representation of their geology at Paris next year; while Canada, both through her Geological Survey and in the person of Dr. Dawson, will probably be represented. The Government of the United States has as yet failed to accept the invitation of France to take part in the Exhibition of 1878, so that American geologists are not certain that they will be able to participate in the International Geological Exhibition. In any event it is probable that several members of the American committee will be present at the proposed Geological Congress. It is recommended by the Standing Committee of the Association that in addition to the names of Prof. J. P. Lesley, of Philadelphia, and Prof. A. C. Ramsey, Director of the Geological Survey of Great Britain, already added to the International Committee, the presidents for the time being of the Geological Societies of France, of London, Edinburgh, and Dublin, of Berlin, of Belgium, Italy, Spain, and Portugal, and of the Imperial Geological Institute of Vienna, be invited to form part of the committee.

The subscription list for the Liebig Memorials is now closed. For that at Munich, 5,750*l.* has been subscribed, and for that at Giessen, 1,200*l.*

Dr. Hermann Karsten, Professor of Mathematics and Mineralogy in the University of Rostock, died on August 26.

It may interest mathematical readers to know that the lecture given by Prof. Voss, of Darmstadt, on the occasion of the Gauss centenary, has been published by Bergstraesser of Darmstadt.

Dr. Werner Siemens has contributed to the *National Zeitung* some very interesting notes on the history of the torpedo. Dr. Siemens maintains that the first idea of protecting navigable waters by means of sinking mines is due to C. Hemly, Professor of Chemistry at Kiel, along with whom he laid the first submarine mines in Kiel Bay, during the Schleswig-Holstein war. The service of Prof. Jacobi, of St. Petersburg, who was not aware of the earlier works at Kiel, consisted in the introduction of contact-torpedoes, and the application of the name torpedo to submarine mines.

M. Faye, who is a candidate for the post vacant by the death of M. Leverrier, is a Government candidate at the present election.

Preparations are being actively made for Prof. Nordenskjöld's voyage along the north coast of Europe and Asia, and out by Behring's Straits, which even in summer are by no means free of drift ice. Already a steamer, the *Vega*, has been purchased for the expedition. The greater part of the cost of the undertaking will be defrayed by Mr. Dickson of Gothenburg, King Oscar of Sweden being also a contributor to a considerable amount.

A Paris telegram states that Capt. Wiggins's vessel, the *Thames*, after wintering at Jenissei, grounded at the mouth of the river on starting, and has not yet been floated, though its cargo was thrown overboard, including, it is feared, the collection of Mr. Seebohm, the English naturalist, who was a passenger.

It is stated that it is the intention of Sir Allen Young to have the *Pandora* refitted, with a view to another start for the Arctic regions next spring. Sir Allen will most probably try the Spitzbergen route in preference to Smith's Sound. Should Sir Allen decide on carrying out his views, the *Pandora* will be fitted with a hurricane deck over her spar deck, and undergo a variety of alterations which the great experience of her owner has shown to be necessary.

Mr. Barclay, a naval officer recently arrived in South Australia, has been engaged to take charge of a party about to explore the country from Alice Springs, on the telegraph line, towards Queensland.

On October 2nd, the Annual *Conversazione* of the Chester Society of Natural Science was held in the Town Hall of that city. The objects exhibited were of great interest, illustrating the work of the members in examining the natural history, geology, and botany of the Society's district, and were well appreciated by a very numerous gathering. During the evening a bust, executed by Mr. Belt, of the late Canon Kingsley, was unveiled by the Dean, who announced the conditions under which a "Kingsley Memorial" prize and medal are offered annually to residents in the Society's district for research into the botany, geology, and natural history of the area in question, these subjects being taken in rotation. There can be no doubt a stimulus will be given by these prizes to original and local research, which will be the means of not only increasing the taste for natural science in the district, but of yielding results of real scientific value.

The observatory on the Pic du Midi, France, has been connected with Bagnères by an electric telegraph, and Gen.

Nansouty has resumed his former post with the power of sending warnings to the lowlands. The telegraphic line from Bagnères to the Pic is 28,000 metres long; the altitude of the Pic is 2,866 metres, and of Bagnères about 550; consequently, the difference of altitude exceeds 2,300 metres. The laying of the telegraph was a very difficult operation, and a portion of the wire has been placed underground. A number of lightning-conductors have been established for protection, and the extremity of the line has been immersed in the lake of Oncet at a small distance from the final slope. Warnings and regular observations will not be sent to the international head office at the observatory, until a final decision has been made as to Leverrier's successor and the organisation of French meteorology. Great efforts are being made by the Meteorological Society to establish a special meteorological office.

BUNSEN'S "Gasometrische Methoden" have appeared in a second revised and enlarged edition.

THERE was a severe earthquake shock at Geneva on Monday morning. Clocks were stopped, bells were rung, buildings cracked, and the English and Russian churches were rather shaken. No great damage was done. The shock extended to Berne, Mulhouse, and Malesina in North Italy.

THE meeting of the Sanitary Congress at Leamington last week was decidedly successful so far as the value and appropriateness of the papers read are concerned, and we hope that substantial practical results will soon follow. A very interesting paper was read by Surgeon-Major De Chaumont on the effects of climate upon health. His conclusions were:—(1) That with proper hygienic precautions there is hardly a place on the earth where man may not enjoy good health, and that where this is not found possible it is from the existence of malaria; (2) that, admitting this much, there are, however, still differences existing which render residence in certain climates more desirable than in others, as most conducing to the fullest health and vigour; (3) that the possibility of acclimatisation has been greatly exaggerated, but that there still remains a residuum of truth in the idea; (4) that there is still a certain importance to be attached to the climatic treatment of disease, although the particular factor or factors that produce the influence are still involved in much obscurity.

THE Prefect of the Seine has issued a decree forbidding bakers and pastrycooks to burn in their ovens wood which had been painted or impregnated with any metallic salt. This measure has been taken in conformity with the advice of the Council of Hygiene, which is said to be giving other signs of its renewed life and activity.

M. GASTON TISSANDIER and his brother have made an ascent from Giffard's aëronautical gas-works, for the purpose of collecting the dust floating in the atmosphere. The method employed has been to condense the moisture of the air and analyse the water and ice thus obtained with a microscope.

THE English price of the *International Review* has been reduced from 4*s*. 6*d*. to half-a-crown. This is presumably done to bring it on a level with the *Contemporary* and *Nineteenth Century* in price as well as in general aim.

THE *Gentleman's Magazine* for October contains an account, with a map, of the missionary colony, Livingstonia, on Lake Nyassa, by Mr. F. A. Edwards.

THE members of the Woolhope Club, struck with the absence of any good illustrated English work on the apple and pear, have decided to publish a "Pomona," in which a carefully-coloured illustration will be given of all the best varieties of apples and pears grown in Herefordshire—and therefore in England—so as to call the special attention of all fruit-growers to those varieties which are most worthy of cultivation. Every apple or pear described will have its outline and coloured representation, whilst the descriptive letterpress and general production of the work will be under the supervision of Robert Hogg, LL.D., F.L.S., &c., &c. The Woolhope Club proposes to publish the "Herefordshire Pomona" in annual parts, of full quarto size, one at the close of each year. Each part will consis of six or more coloured plates, according to the amount of annua subscriptions received. The Club guarantees the publication of the first part at the close of the present year, 1877, and it will contain an introductory paper on "The Early History of the Apple and Pear," and also one on the "Life of Thomas Andrew Knight," president of the Royal Horticultural Society, "and his Work in the Orchard."

IN a new form of the Sprengel air-pump described in a paper at the British Association by C. H. Stearn and J. W. Swan, the mercury reservoirs at the top and bottom of the pump are closed so that the external atmosphere exerts no pressure on the surface of the mercury contained within them. In consequence of this the fall-tube may be much shortened while the efficiency of the instrument is retained. At the commencement of the exhaustion of a receiver the mercury supply reservoir is filled to the top and closed by a stopper; a small exhausting syringe attached to the reservoir at the bottom of the fall-tube is then set in action, which removes a considerable portion of the air from the receiver to be exhausted, and also very much reduces the pressure on the mercury in the lower reservoir; the flow of mercury through the pump rapidly completes the exhaustion. A small vacuum tube with aluminium wires a quarter of an inch apart was exhausted in twelve minutes to such an extent that an induction coil giving sparks half an inch long in air failed to produce the faintest luminosity, the fall-tube of the pump being only nine or ten inches long.

AT one of this year's meetings of the Dresden Naturalists' Society "Isis," Herr Schuster read an interesting extract from a chronicle of the town of Meissen, dating from the year 1590, and written by Peter Albinus, in which the mines in the environs of Meissen are described. Amongst the *natural* products of the district the author mentions the numerous vases and urns which were frequently excavated, and were superstitiously believed to have grown in the ground. People at that time believed them to be inhabited by dwarfs, and that when winter approached they sank down deeper into the earth; while in spring, and particularly in May, they again rose to the surface, and thus formed a flat little cone above themselves. Although Albinus himself thinks this belief rather too coarse, and ventures his opinion that the objects in question are artificial—thus showing that already three hundred years ago the interest in these remains of prehistoric times was a vivid one—it is to be regretted that the superstition we have mentioned has even up to this day not yet died out entirely, since a great part of the uneducated masses in Saxony are still of the same opinion with regard to the vases and urns.

AT another meeting of the same society some interesting statistical data were given showing the total quantities of the various products obtained from a single Saxon mine, the *Himmelfahrt Fundgrube*, near Freiberg, since its opening in the year 1524. This mine up to the end of 1875 had yielded about 535 tons of silver, 54,125 tons of lead, 1,785 tons of copper, 13,585 tons of sulphur, 2,175 tons of arsenic, and nearly the same quantity of zinc.

A THIRD note of interest read at a meeting of the same society was by Herr L. H. Zeitteles, and treated of the prehistorical ancestors of our common house-dog. The author, who studied the subject for eleven years, arrived at the conclusion that neither wolves nor foxes had any part in the phylogeny of the dog, but that jackals and the so-called Indian wolf, *Canis pallipes*, Sykes, were the original ancestors of *Canis familiaris*.

THE first volume of the *Annals* of the Royal Belgian Museum of Natural History contains the first part of Van Beneden's work, "Description des Ossements fossiles des environs d'Anvers," which deals with the Pinnipeds, and is illustrated by fourteen engraved plates.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseo-viridis*) from West Africa, presented by Mr. R. Dudgeon; a Lion (*Felis leo*) from Persia, presented by Mr. F. Pollock; a Common Seal (*Phoca vitulina*) from the British seas, presented by Mr. G. Mellin; a Great Kangaroo (*Macropus giganteus*) from New South Wales, presented by Mr. T. Phillips; a Collared Peccary (*Dicotyles tajaçu*) from South America, presented by Mrs. E. J. Barrett; two Emus (*Dromæus novæ-hollandiæ*) from Australia, presented by Lord Francis Conyngham, M.P., F.Z.S.; two Peregrine Falcons (*Falco perigrinus*), European, presented by Mr. Darill Stephens; a Black-headed Partridge (*Caccabis melanocephala*) from Hedgar, a Hyacinthine Porphyris (*Porphyrio hyacinthinus*) from Mesopotamia, presented by Capt. Burke, S.S. *Arcot*; a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, an Arabian Gazelle (*Gazella arabica*) from Arabia, deposited.

AMERICAN SCIENCE

IN the last number of the *American Journal* Mr. Charles Wachsmuth continues his notes on the internal and external structure of palæozoic corals, and discusses the construction of the summit and its value in classification. He believes that while the construction of the ventral disc or actinal side of the calyx has not received the attention it deserves, it affords a clear and important distinction between recent and ancient crinoids. Commenting on Rœmer's classification of "the true crinoids which are supported by an articulated or jointed column" given in *Lethæa geognostica*, 1855, Mr. Wachsmuth says he expects from a dissection of *Synbathocrinus* that in other of the Cupressocrinidæ the central opening was closed, and that the consolidating plates were further overlaid with plates forming the floor of a passage in connection with the arm furrows and visceral cavity. In speaking of the Cyathocrinidæ he refers to the covering of Cyathocrinus, as throwing light on the summit structure of other genera, and remarks: "It is worthy of note that the Cyathocrinidæ, in the structure of their vault, bear closer resemblance to the recent crinoids than almost any other group, and seem to hold an intermediate position between modern and palæozoic types. The Cupressocrinidæ and Cyathocrinidæ thus fall naturally into a group by themselves, having the vault supported by consolidating plates and covered by an immovable arch of small plates." In the Taxocrinidæ Mr. Wachsmuth has found that there are solid plates, though they have been before described as covered by some soft material. Among the spheroidæ, which range from the silurian to the subcarboniferous, the summit is found well preserved in most genera. Besides details of obsevations some generalisations are added. "Closely related as the recent crinoids are to their palæozoic ancestors in some points, the solid vault of the latter cannot in the remotest degree be homologised with the soft peristome of the former." Many facts tend to prove that the palæozoic crinoids embracing therein all true crinoids in which the actinal side is closed, represent the young stage of growth of living types. They form a distinct group of crinoids, and it is proposed to call these paleocrinoideæ.

Prof. Draper advocates the use of the cylinders of zirconia for the oxyhydrogen light in such cases as the employment of the microscope to throw objects on a screen for lecture demonstrations. A high brilliancy with the least variability in the light, and a fixity of its position in the optical axis of the apparatus, are needed for success. Prof. Draper gives his reasons for believing that the oxy-zirconium light fulfils all requirements better than any other known light. "It has the intrinsic brilliancy, the invariable brilliancy, the fixity of position in the optical axis of the apparatus, and it does not volatilise under the heat employed. The condensing lenses remain free from deposit, and after the light is once adjusted the experimenter can carry on his demonstrations without the distraction of his attention that attends the use of the other lights." He gives direction for the preparation of zirconium oxide, and for the preparation of the cylinders.

Mrs. M. S. Cheney and Mrs. Ellen S. Richards, dating from the women's laboratory, Massachusetts Institute, describe a new and ready method for the estimation of nickel in pyrrohotites and mattes.

Prof. J. D. Dana is publishing his conclusions as to the relations of Vermont and Berkshire geology, and Ed. S. Dana has recently described garnets from the trap of New Haven.

Mr. G. B. Grinnell, who has devoted attention to the annelids of the Cincinnati group, describes a new genus from the Lower Silurian. They have hitherto been inferred from their trails, and the hard chitinous parts now found do not seem to belong to any recognised genus.

Mr. Joseph Le Conte, criticising Dr. Hermann's paper on "The Passage of Luminous Pencils obliquely through Lenses and on a Related Property of the Crystalline Lens of the Human Eye," points out that the periscopic structure of the lens is useless, because periscope perception of the retina is wanting. It must be regarded as an example of a structure which has outlived its usefulness.

Prof. O. C. Marsh (in the Appendix to the September number of the *American Journal*) describes new fossil mammals, birds, reptiles, and fishes, from the Rocky Mountain region. Among the mammals are two miocene edentates, the first detected in the country, and a third species from the lower pliocene. The names of the new species are *Moropus distans*, *Moropus senex*, *Moropus elatus*, *Amynodon* (gen.), *Tapiravus rarus*, *Bison ferox*, *Allomys nitens*, *Graculavus lentus* (a bird the size of a duck), *Diplosaurus felix*, *Crocodilus solaris*, *Nanosaurus agilis* (a dinosaur no larger than a cat), *Nanosaurus victor*, *Apatodon mirus*, *Heliobatus radians*.

We have already referred to an important exploration of the natural history and ethnology of the West Indies, now in course of prosecution by Frederick A. Ober, under the auspices of the Smithsonian Institution. Some interesting collections of specimens have already been received at Washington from Mr. Ober, embracing particularly a series of the birds of the island of Dominica, including several species new to science, and others of excessive rarity. Among the latter is a huge parrot, one of the largest of its genus. The latest advices from Mr. Ober are dated Antigua, August 6. He was then about proceeding to St. Kitts, and thence through the chain of English islands to Granada, including the Dutch islands of Saba and Eustatius. The region to be explored by him extends over six degrees of latitude, and will occupy him at least two years. Not the least important results of Mr. Ober's work have been the studies made during a long residence among the Carib tribe of Dominica. He has been able to secure numerous photographs of this little-known people, and many illustrations of their manners and customs, all of which will be hereafter the subject of a popular article for some one of our leading journals.

The Kansas University scientific expedition of 1877 has found a number of a very rare species of beetle of the genus *Amblychila*, the acquisition of which has long been an object by collectors of coleoptera. For the purpose of securing funds to defray the expenses of their explorations, the authorities of the university offer specimens for sale at a moderate price.

A precious limestone has been found at Tehachepa, Kern County, California, which is said to be identical with the "giallo antico" (ancient yellow) marble of Italy. The latter is highly prized by antiquarians, as the location of the quarry from which it was procured has been unknown for several centuries. The California stone is described as white with amber-coloured veins. A specimen has been presented to the State Geological Society.

Mr. Edward Bicknell, a gentleman well known among American microscopists, died on March 19, at Lynn, Massachusetts, at the age of forty-seven. Originally a resident of Salem, he joined the scientific corps of workers at the Museum of Comparative Zoology in Cambridge, with which he was connected until the death of Prof. Agassiz. His sections of shells and rocks were of extraordinary beauty, and he was also specially skilled in the preparation of injected objects.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Oxford.—The Oxford University Commissioners will sit for a fortnight at Oxford, from the 22nd of this month. They will occupy rooms at the Clarendon Hotel, the University being unable to place sufficient accommodation at their disposal. It is understood that evidence will be received during this sitting as to the requirements of the University. The *Academy* states that it is rumoured that the Oxford University Commissioners propose to devote the first year to taking evidence; they will then lay down principles, and, lastly, will receive and adjudicate upon the schemes of the various colleges.

Cambridge.—Mr. William James Sell, B.A., Scholar of Christ's College, has been appointed joint demonstrator of Chemistry in the University in conjunction with Mr. Hicks.

Mr. J. Aiken, of Liverpool, who a few years ago gave a donation of 1,000*l.* to the Association for the Higher Education of Women at Cambridge, has signified his intention of placing at the disposal of the Association an exhibition of 30*l.* for two years.

Manchester.—The session of the Owens College was opened on Tuesday, the 2nd inst., with an introductory address by Prof. Williamson, F.R.S., on the present aspect of the evolution theory. So far as can yet be judged the attendance of students promises to be very good during the session.

Ireland.—The thirteenth annual meeting of the Convocation of the Queen's University in Ireland was held on October 5 in Dublin Castle, under the presidency of Sir Dominic Corrigan, Vice-Chancellor. The annual report referred to the great necessity that now was manifested for the supply of central buildings for the University in the Irish metropolis. The report was unanimously adopted, and it was urged by the speakers that a representation should be made to the Government for a grant in aid of the erection of necessary buildings. A motion in favour of the admission of women as medical students to the colleges of the University was lost.

Glasgow.—It is stated that Dr. Cleland, of Galway, has been appointed to the Chair of Anatomy in Glasgow University, recently vacated by Dr. Allen Thomson.

Mr. A. Orr Ewing, M.P. for Dumbartonshire, has announced his attention of founding, in connection with the Glasgow University, four bursaries of 25*l.* per annum each, tenable for four years. Mr. Orr Ewing expressly declares this to be an experiment to test the working and results of the bursary system before resolving upon a permanent endowment. To this end he has decided to place the sum of 1,600*l.*, payable in seven annual instalments, at the disposal of the Senatus.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, October 1.—M. Peligot in the chair.—The following papers were read:—On the order of appearance of the first vessels in the shoots of Lysimachia and of Ruta, by M. Trécul.—Reply to M. Angot's last note on the system of winds in the region of the Algerian chotts, by M. Roudaire.—Boric acid; methods of investigation; origin and mode of formation, by M. Dieulafait. *Inter alia*, he considers (in opposition to some high authorities) that the boric acid and accompanying substances in the *lagoni* of Tuscany and in analogous beds, are (with exception of the carbonic acid) products exclusively sedimentary, their mode of formation being fully explained from a study of the mother-waters of salt-marshes. It is unnecessary to call in any volcanic action. Boric acid seems to have existed in seas from the earliest ages, and to have been deposited wherever portions of sea got separated under suitable conditions. This occurred on an immense scale at two epochs widely apart, viz., in the trias, and at a certain horizon of the tertiary formation. It is in the *last* mother-waters of salt-marshes that boric acid is concentrated.—Employment of pyritous earths for treatment of phylloxerised vines, by M. Dufresnoy.—Integrals of oblique developers of any order, by Abbé Aoust.—Discovery of oxygen in the sun, and new theory of the solar spectrum, by Prof. Draper.—Note on the magnetisation of tubes of steel, by M. Gaugain. If a neutral cylindrical bar of steel, at ordinary temperature, be introduced into a magnetised tube of steel and withdrawn after a few seconds, it will be found weakly magnetised in the same sense as the tube. But if, after insertion, the system be heated with a lamp to about 300 deg., allowed to cool, and the core then drawn from the tube, the tube will be found to have lost a large part of its original magnetism, and the core to have taken an inverse magnetism.—On the exact measurement of the heat of solution of sulphuric acid in water, by M. Croullebois. The hitherto divergent results are attributed to a fact observed by M. Kirchhoff, viz., that the thermal effect is intimately connected with the tension of aqueous vapours emitted by the solution, and consequently with the temperature. Taking this into account the author gives a table of calories corresponding to different temperatures from 10° to 24°.—Continuation of researches on the effects of electric currents of high tension, and their analogies to natural phenomena, by M. Planté. This relates to effects had on placing the positive electrode of a battery of 800 secondary couples in distilled water, and bringing the negative platinum wire near the surface, a column of water having been inserted in the circuit to obviate fusion. A small globe of fire appears, taking an ovoid form when the electrode is raised a little, while a number of blue luminous points in concentric circles are seen at the surface of the water. Rays presently start from the centre and join the points; they go into gyration in one direction or the other, and describe spirals; sometimes they disappear on one side. Lastly, with increased velocity of gyration, all vanish, and only the blue concentric rings are left. The experiment bears on the formation of globular lightning.—Some new researches on the metal davyum, by M. Kern. New researches on the density confirm the former. From preliminary experiments the equivalent is shown to be greater than 100, and probably near 150-154.—New modes of formation of oxide of ethylene, by M. Greene.—Note on the wire-drawing of platinum, by M. Gaiffe. He has got stronger fine wire by excluding atmospheric dust more completely.—On the fecundation of echinoderms (continued), by M. Fol.—Metamorphoses of cantharides (*Cantharis vesicatoria*), by M. Lichtenstein.—On the mutual antagonism of atropine and muscarine, by M. Prevost. He asserts (contrary to some) that large doses of muscarine will produce toxical effects in animals previously atropinised.—Trajectory of the bolide of June 14, 1877, by M. Gruey.—Meteorological observations in a balloon, by MM. Tissandier. They found a layer of air 400 m. thick, at a height of 400 m., moving pretty rapidly between two other layers almost motionless; a rare phenomenon.—On a halo observed at Brest on August 31, 1877, by M. Salicis.—Reflections on the meteorological works of M. Brault, by M. Buys-Ballot.

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THURSDAY, OCTOBER 18, 1877

HUXLEY'S "ANATOMY OF INVERTEBRATED ANIMALS"

A Manual of the Anatomy of Invertebrated Animals. By Thomas H. Huxley, LL.D., F.R.S. (London: Churchill, 1877.)

IN the year 1871 Prof. Huxley published a Manual of the Anatomy of Vertebrated Animals as the first part of a treatise on Comparative Anatomy. By the publication during the present autumn of the work now about to be noticed, he has fulfilled his undertaking to produce a treatise for students on this extensive and complex branch of anatomical inquiry. As might be expected from the author's well-won reputation, not only as a philosophical thinker and scientific observer, but as extensively read in the literature of his subject, the work is one which, in proportion to its size, furnishes the student with the most compact account of the present aspect of the science of comparative anatomy in the English language.

In writing a work on comparative anatomy, two modes of arranging and classifying the multitude of facts that are to be considered may be pursued by an author. In the one, which may be called the anatomico-physiological method, he may take as the basis of his arrangement the several organs found in the animal body, and may trace out the various modifications exhibited by each organ in different animals. This plan has been pursued in the well-known systematic treatises of Cuvier, Meckel, and Milne-Edwards. In the other, or zoological method, animals are grouped in "natural orders" according to a taxonomic system, and the anatomical characters of the animals belonging to each of these orders are described and compared with each other. Siebold, Stannius, Owen, and Gegenbaur adopted this mode of arrangement, and a somewhat similar plan has been carried out by the author of the work before us.

Although the anatomico-physiological method is not without its advantages to a particular class of readers, yet the zoological basis of arrangement is, we think, more generally useful, as it enables the reader to acquire by the perusal of a single chapter, a knowledge of the anatomy of a given natural order of animals, instead of having to pick out from a number of different chapters the various facts bearing on their structure.

In carrying out this plan Prof. Huxley has selected for special and more elaborate description a characteristic specimen of each group or order, whilst the other members of the same group or order are either only incidentally alluded to, or merely the chief features of difference between them and the selected specimen are pointed out. For a student's book this is unquestionably the most suitable plan, as it gives him the means of perusing, within the compass of a moderate-sized volume, a carefully drawn-up account of well-marked examples of the various orders, without confusing him with a multitude of subordinate facts which it would be difficult to keep in mind, whilst it facilitates practical study by supplying a somewhat full description of individual forms. In addition to the description of adult forms much information is also communicated on the development of the animals described, and the facts of embryology are made to throw much valuable light on the details of structure.

To the general reader the most interesting sections of the book are the introduction and the first and last chapters. The introduction commences by pointing out that the distinctive properties of living matter are due to its chemical composition, to its universal disintegration and waste by oxidation, and its concomitant reintegration by the intus-susception of new matter, and to its tendency to undergo cyclical changes. A number of interesting examples are then given of the dependence of all the activities of living matter upon moisture, and upon heat within a limited range of temperature. The arguments which have been advanced in favour of the origin of living from non-living matter, or abiogenesis, as it is now termed, are then considered. The conclusion is come to that there is no ground for assuming, as is done by the supporters of the hypothesis of abiogenesis, that all living matter is killed at some given temperature—between 104° and 208° F., so that the evidence adduced in its favour, from the experiments where organic infusions have been subjected to this high temperature, is logically insufficient to furnish proof of its occurrence. Prof. Huxley abides, therefore, by the opinion expressed in his well-known Liverpool Address to the British Association on this subject. There is no necessary connection as has sometimes been assumed between the theory of evolution and a belief in the occurrence of abiogenesis as a mode of origin of living things at the present day. Life must at one time have been breathed into non-living matter, but there is nothing to show that existing organisms, or those occurring in any recorded epoch of geological time, have had any other origin than from pre-existing forms of life.

It has been assumed by many writers that the developmental changes, which an organism passes through during its embryonic existence, furnish a key to decipher the full pedigree of the organism and proclaim its family history. So keenly has this branch of biological speculation or phylogeny, as it is termed, been followed out by some naturalists, that they have based their systems of classification on the supposed ancestral history of animals. We are glad to find that Prof. Huxley interposes some wise words of caution on this matter. The reconstruction of the pedigree of a group from the developmental history of its existing members is, he says, fraught with difficulties. And again, of the numerous phylogenic hypotheses which have of late come into existence, few have any other significance than as suggesting new lines of investigation; in the absence of any adequate palæontological history of the *Invertebrata*, any attempt to construct their phylogeny must be mere speculation. It is to be hoped that this protest against the fanciful hypotheses of some phylogenists, a protest which might have been unheeded if it had emanated from an opponent of the doctrine of evolution, may, seeing that it comes from him, who by the lucidity, vigour, and logical power of his writings, has, next to the illustrious Darwin, done more to gain credence for the doctrine, than any other writer, check the tendency to hasty speculation in this direction, in which some naturalists have of late indulged.

Had space permitted we should have liked to have given a sketch of the groups or natural orders in which Prof. Huxley arranges the *Invertebrata*, the classification of which is surrounded with so many difficulties, that scarcely any two writers on classification adopt the same taxonomic system. Prof. Huxley does not look upon the arrangement he has adopted as more than temporary, as our knowledge of the anatomy and development of the Invertebrata is increasing with such prodigious rapidity that the views of taxonomists in regard to the proper manner of expressing that knowledge by classification are undergoing, and for some time to come are likely to undergo, incessant modifications.

We heartily commend this book to all students of Comparative Anatomy.

EVERETT'S "TEXT-BOOK OF PHYSICS"

Elementary Text-Book of Physics. By J. D. Everett, M.A., D.C.L., F.R.S.E., Professor of Natural Philosophy in the Queen's College, Belfast. (Glasgow: Blackie, 1877.)

IN the preface to this book the author says: "It is primarily intended as a text-book for elementary classes of Physics. It aims at presenting, in brief space, those portions of theoretical physics which are most essential as a foundation for subsequent advances, while at the same time most fitted for exercising the learner in logical and consecutive thought. It does not give minute directions for manipulation, but, avoiding details as much as possible, presents a connected outline of the main points of theory. . . . The aim must be not so much to teach them [the bulk of the boys in our public schools] many facts, as to teach them rightly to connect a few great facts together. . . . The book is not intended to supersede oral instruction, but rather to create a demand for amplification and illustration such as the teacher will supply."

Judged from this point of view the text-book must receive almost unqualified praise. The different divisions of the book treat respectively of the subjects, dynamics (in its modern acceptation), hydrostatics, heat, light, sound, and electricity including magnetism, and in each division the author explains the leading facts in clear, concise, and accurate language. What mathematics is introduced is of the simplest possible kind, and need not prevent the veriest tyro in geometry and algebra, provided he is possessed of ordinary intelligence, from reading and understanding the book from beginning to end. The definitions are, as a rule, very exact, and the explanation of the units, as might have been expected from the author of the pamphlet, the "centimetre-gramme-second system of units" is singularly precise. Add to this that the diagrams are numerous, and, which is of rare occurrence in an English text-book, of unusual excellence, and that each division is followed by a collection of examples (except the last, which apparently has not been thought worthy of the honour) at once good and easy, and enough has been said to show that the text-book is one with many merits.

It has its demerits too, and if we dwell longer on them it is only in the hope that a truly excellent manual may be rendered still more excellent in a second edition. Why does the author make not the slightest mention of Newton's laws of motion, although all the statements made in them are asserted, but in such a casual off-hand sort of way that the student wonders what is the evidence for such important statements? For example, in Art. 9 it is said "If a body with a movement of translation (unaccompanied by rotation) is acted on either by no forces or by balancing forces, it continues to move with uniform velocity in a straight course." This assertion is introduced by no explanation, neither is it followed by any remark or illustration. The same thing may be said, and even more forcibly, of the treatment which the second law receives. This fault, of making assertions without any explanation or shadow of proof, is rather too apparent throughout the book, as a few instances will show. The examples are taken from pages that are all near together. At p. 147, line 4, we read, "the last image consists of two coincident images, as has already been shown [for 'shown' read 'asserted'] to be the case when the angle is a right angle." Again, at p. 150, line 3, it is said, "The angles of incidence and refraction increase together and the deviation increases with them." At p. 153, after a description of Airy's simple and beautiful apparatus for illustrating refraction, it is added "A B C will be the path of a ray, and a stud at C will appear in the same line with studs at A and B." At p. 158, line 9 from bottom, we find—"If the eye is moved with uniform velocity from one side of the normal to the other (in one straight line), the image moves with a velocity continually diminishing till the normal is reached, becoming zero at the normal, and then again gradually increasing. This is a general property of geometrical images, whether formed by refraction or reflection. . . . ." At p. 161, line 1:—" . . . . The rays reflected from the outer portions of the mirror will fall sensibly short of the middle point of O C. If the point of incidence be supposed to travel with uniform velocity along the arc M O from M to N, the intersection of the reflected ray with O C will move towards F with velocity gradually diminishing to zero." Once more, on p. 163, last line, and p. 164, first three lines, we read:—" . . . . It can be shown that if the angular aperture be small all the reflected rays will meet sensibly in one point, P." It can also be shown that—

$$\frac{1}{OP} + \frac{1}{OP'} = \frac{1}{OF}.$$

Although it is not to be expected that any other seventeen pages will supply as many examples as these, still such faults do occur throughout the book. We do not object to statements heralded by the words "It can be shown," as the student is at once put on his guard, and virtually referred to other sources for the proof, if he wishes it; but most frequently he is left either to take the statement on trust, or, in the words of the preface, "to exercise his mind in logical and consecutive thought." Only we think that in the latter case the text-book can hardly be called one "for elementary classes."

From inaccuracies the book is wonderfully free, but there is surely one, and not a small one, about harmonics at the bottom of p. 241. The author asserts that the origin of harmonics depends on two very different causes. Sometimes it is found in the different modes of free vibration of the body which emits the sound (and this is especially the case with the sounds of stringed instruments), but in other cases (including the tones of the siren, and the human voice) it depends on a very different cause, namely, the mathematical law that every series of precisely similar vibrations is either simple, or compounded of one set of simple vibrations, giving the fundamental tone, and other sets of simple vibrations giving the harmonics." Now the second cause—Fourier's law—is surely the only one for the existence of harmonics, and in every vibration, whether of a string or any other body, the ear analyses the motion of the air in accordance with the law, that is, hears harmonics. Is it possible that the origin of the misconception here lies in the old notion that when a string vibrates in any regular manner there are secondary waves riding on the primary ones, another set on these secondary, and so on? as—

> "Great fleas have little fleas
> Upon their backs to bite 'em,
> The little fleas have lesser fleas,
> And so ad infinitum."

By the way, why are the notes of the human voice, which are produced by the vibrations of the vocal chords, distinguished from the notes of stringed instruments?

It is not to be expected in an elementary manual that every subject, even of importance, should be noticed, but we should have thought that even in the briefest treatise on heat, some notice would have been taken of the "theory of exchanges," and yet we find no mention whatever made of it. Also a little more space than two pages might have been devoted to the electric telegraph, especially, first, as room has already been found for the description of the venerable three kinds of lever, and the antiquated three systems of pulleys (which are rarely seen except in text-books of physics); and, secondly, inasmuch as three times the space is taken up in the description of the air-pump and its modifications.

The good qualities of the book are so conspicuous, and its faults either so slight or so easily corrected by the teacher, that we have no hesitation in warmly recommending it as a good text-book for junior classes.

T. H. C.

OUR BOOK SHELF

Popular British Fungi; containing Descriptions and Histories of the Principal Fungi, both Edible and Poisonous, of our Country. Illustrated. By James Britten, F.L.S. (London: the *Bazaar* Office.)

THIS admirable little book forms an agreeable and popular introduction to a much neglected group of plants. Written in a pleasant easy style, it yet conveys a great deal of sound information. Mr. Britten having drawn on his imagination merely for the setting of his facts, not for the facts themselves. The different illustrations convey a tolerably accurate idea of the plants represented. The edible fungi are carefully described, and most useful hints and directions given as to the modes of cooking and preparing for table. The poisonous forms also receive a considerable share of attention, and the characters are carefully given, but even with all sorts of descriptions we cannot but think that there is always danger from such genera as Lactarius and Russula. Besides treating of the usual edible and poisonous fungi, Mr. Britten gives a chapter on Dry Rot, another on Luminous Fungi, and another on the Sphaeriacei. Throughout the whole book we constantly meet with quaint quotations from old authors. The book, then, is not only a very pleasant and readable one, but conveys a great deal of sound information on the subject therein treated.

Zeitschrift für das chemische Grossgewerbe. Herausgegeben von Jul. Post. II. Jahrgang. Heft I. (Berlin: Robt. Oppenheim.)

WE have already had occasion to express our high opinion of the value of Dr. Post's contributions to chemical technology. The present work is to the chemical manufacturer what the well-known "Jahresbericht" of Liebig and Kopp is to the scientific chemist. It attempts to give the technologist a systematic account of the latest advances in the several departments of manufacturing chemistry and the allied arts. As the various contributions are from the pens of men who, in the majority of cases, have made the matters upon which they write the objects of special attention and study, we can confidently recommend the work to the notice of our chemical manufacturers.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Indian Rainfall Statistics

SEVERAL correspondents in the *Times* and elsewhere have lately complained regarding the class of information furnished by the Government of India with reference to the famine. One of them pointed out that while the Indian official *Gazette* is filled with tables of food-prices, and other statistics, there seems to be no attempt to deduce the real significance of those figures, nor are any data of comparison offered by which the public might make deductions for themselves. I have just come across a very glaring instance of this. The Indian rainfall having been discussed a good deal this summer in your columns, I heard with some rejoicing that a long list of returns had been given in the Indian *Gazette* for stations in Madras and Bombay. The returns purport to bear upon the variation of rainfall in tropical India with reference to the cycle of sun-spots. As an old resident in Madras and Hyderabad, I think it would be difficult to produce a series of figures more irrelevant or more misleading with regard to the matter in hand. So far as I understand your articles, it is alleged that of the six famines in Southern India since 1810, five were caused by great droughts at the periods of minimum sun-spot frequency. It also seems to be alleged by you that the rainfall at Madras itself follows a cycle curiously coincident with the eleven-year cycle of sun-spots. These are two propositions distinct in themselves, and either of them is well worthy of investigation by the Meteorological Reporter to the Government of India.

The figures signed by that officer in the *Gazette* yield no information on the subject. He begins by completely mis-stating the case. Instead of testing the one statement as to whether drought and famine in Southern India have been coincident with periods of minimum sun-spots, or the other statement as to whether the rainfall at Madras itself forms a cycle coincident with the sun-spot cycle, he assumes that the question at issue is whether the rainfall at all the stations throughout Southern India shows a common periodicity coincident with the cycle of sun-spots.

Now, sir, it surely displays a gross ignorance with regard to the geographical facts of Southern India, to suppose that a periodicity, or any other feature of the rainfall of a place on the coast, like Madras, can be reproduced at the stations which the paper seems to take haphazard at the inner recesses beyond the Ghauts. I shall take, for example, only three stations in which I have resided, and whose monsoons I have seen. Madras derives about two-thirds, or twenty-nine inches, of its total rainfall from the north-eastern monsoon between October and December. The periodicity of its total annual rainfall is chiefly due to this monsoon. The Calcutta meteorological reporter compares this station with Secunderabad, where the north-eastern monsoon yields only three inches, and with Bangalore, where it yields only six or seven. I may add that he also compares it with Bellary, which derives only three inches from the north-east monsoon. It must be evident that any cyclic periodicity in the Madras rainfall which is chiefly derived from the north-eastern monsoon, cannot possibly be common to such stations as Bangalore, Bellary, and Secunderabad, which are scarcely reached by the north-east monsoon, and which derive an altogether insignificant rainfall from it.

I have only quoted three instances, but the paper is full of similar absurdities. The Government of India should not be surprised that we Madrassees are jealous of its interference and distrustful with regard to its ability to interfere for good, so long as it puts its official *imprimatur* on papers such as this. I do not suppose that Mr. Blanford, the meteorological reporter, is personally responsible for it. The explanation no doubt consists in the fact that the paper has been drawn up by some ignorant native clerk, and signed *pro forma* by his official superior. Any subaltern in a marching regiment who has crossed the Ghauts knows that the north-east monsoon cannot possibly exert a common influence upon the coast where it breaks in its first fury, and the walled-in plateau of Hyderabad or Bellary, where it scarcely reaches at all. OLD MADRASSEE

Potential Energy

WHEREVER the fault may lie, your correspondent, "P. M.," has quite misapprehended John O'Toole, whose amanuensis I am, in every point on which he, "P. M.," touches, but one, viz., John's wish to place the potential energy in the force instead of in the body to be moved. X.

Dublin, October 12

IN the capacity of poor P. I have suffered much at the hands of the doctors, and am glad to find in your correspondent "X." so competent an exponent of many of the difficulties with which the subject of energy, as generally taught, is beset. But while in the main I agree with the criticisms of "X." there are two or three points with reference to which I may perhaps venture an opinion.

It seems to be admitted (NATURE, vol. xvi. p. 459) by your correspondent that in the case of a moving body "the kinetic energy is undeniably in the body." This appears to me to lie at the root of the chief difficulty, for, as far as I can see, we have no more right to assert that a cannon shot possesses kinetic E. when it leaves the muzzle of the gun than to say that a clock weight possesses potential E. when it has been wound up; for it may happen that the shot is at rest relative (shall we say) to the centre of gravity of the physical universe. At any rate we are well assured that of itself it can do no work. Suppose the shot to find a home in the side of a ship; in entering the ship it does work, but the amount done depends upon the original motion of the ship, being greater if the ship were moving to meet the shot than if it were at rest; it also depends on the mass of the ship, for if the shot sensibly change the motion of the ship its own motion will be less altered by the impact. When a shot strikes a target, which we assume to be rigidly connected with the earth, the mass of the latter is so great that we may consider the target as fixed, and thus we have only to contemplate the mass and velocity of the shot. Simple problems of this nature were of course more inviting than those in which the mass and velocity of both the colliding bodies had to be taken into account; and thus it came to pass that the kinetic E. was attributed to the shot alone instead of being considered an attribute of the system consisting of the earth and shot together and due to the relative velocity of the two. Adopting this latter mode of expression, we may, if we please, suppose the shot to be at rest and the earth moving relative to it with the velocity of (say) 1,400 feet per second, and the energy of the system will be unaltered by our convention; but where should we be if in this case we supposed the kinetic E. of a rigid moving body to be an attribute of it alone?

Again, when we say that the kinetic E. of m units of mass moving with v units of velocity is $\frac{1}{2} m v^2$, this velocity must be measured with reference to some point or other which we for the time consider fixed. In order to obtain $\frac{1}{2} m v^2$ units of work from the body, we must bring it to rest relative to our point of reference, but in so doing we must take care that no motion is imparted to the point of reference itself, for if this be the case the body will come to rest relative to it without losing v units of velocity, and therefore without doing $\frac{1}{2} m v^2$ units of work. If our system consist only of two bodies, and work is to be done by bringing them finally to relative rest, this condition will be fulfilled only when the body which we choose to consider at rest is indefinitely great compared with the other, and only then may we represent the kinetic energy of the system by $\frac{1}{2} m v^2$, m being the mass of the smaller body and v its velocity relative to the other.

I think we shall avoid all difficulty if we define the kinetic energy of a system as *the energy which the system possesses in virtue of the relative motions of its parts*; we shall then never hear of the kinetic E. of a shot or other rigid body, except as an abbreviation for the kinetic E. of the system consisting of the earth and the shot, &c.

A precisely similar line of argument may be followed with reference to potential E. or the E. of position. The potential E. of a rigid body, whatever its position may be, is an absurdity. The very notion of position implies relation to other bodies, as we have no fixed points in space, and thus it is necessary, if only for this reason, that in speaking of potential E., at least two portions of matter which are capable of changing their position relative to one another should be taken into account. In the case of a raised weight, the system consisting of the earth and the weight possesses energy in virtue of the separation of its parts, and the system can be made to do an amount of work equivalent to this energy by keeping the earth fixed and letting the weight fall, or by keeping the weight fixed and letting the earth move up to it, or by letting each move to the other, as in nature. In any case the work done will be the same, though the time required may be very different; but according to the modes of expression complained of by your correspondent, I suppose that in the first case the potential E. belongs to the weight, in the second to the earth, and in the third it is divided between the two. Should we not avoid all difficulty by defining the potential energy of a system as *the energy which the system possesses in virtue of its configuration?* We should then never speak of the potential E. of a single rigid body, such as a raised weight, except as an abbreviation for the potential E. of the system consisting of the body and the earth. A strained elastic body, such as a bent bow, of course possesses potential E., for in this case the particles of the body have been moved relative to one another from their position of rest, and thus the configuration of the system has been changed.

Of course I agree with your correspondent that the potential energy of a system is just as truly *energy* as is its kinetic energy, and this brings me to the last point I proposed touching upon, viz., the term "energy of tension." Perhaps *energy of stress* might avoid some objections, since tension and its antithesis, pressure, have very special meanings; but surely in adopting such a phrase we can hardly say that the designation implies an "essential characteristic," while the term E. of configuration refers only to a "condition." In the case of two attracting bodies the potential E. is greatest when the attraction is least on account of the increased distance, and it depends not upon the actual attraction between the bodies in their existing configuration, but upon the attractions which are called into play in all the configurations assumed by the system as the bodies approach each other, and which therefore belong *in potentiality only*, to the system in its initial condition. If we define the potential of a point in the neighbourhood of a system of bodies as the amount by which the energy of the system would be increased by the introduction of the unit mass at this point, then so long as the mass is absent, the energy due to it can be only *potential*, but when the mass is placed there the energy is *actual*. In strictness, then, we ought not to apply the term potential to the energy thus introduced into the system. We, however, require some mark to distinguish this energy from kinetic energy, and the word potential serves to remind us of the condition of affairs before the mass was introduced. Again, we may have a stress as great as we please acting between the parts of a system without any consequent potential energy, so that the space "condition" seems to be at least as important as the stress "characteristic." On the whole I think the phrase *potential energy* preferable to energy of tension, stress, or configuration, although it is applied to energy which is as truly actual, and belonging to the material system, as is that of a shaft-impelled-against-an-ironclad.

Cambridge W. G.

WITH reference to the question concerning the bricks, in NATURE, vol. xvi. p. 477, it is obvious that if a man lifts a brick down from a wall and places it on the ground, the *vis viva* of the brick at the bottom is very much less than it would be if it fell from the wall under the influence of gravity. In lifting it down the man does work *against* gravity, and therefore the energy of position of the brick on top of the wall finds its equivalent (1) in the *vis viva* of the brick at the bottom, and (2) the mechanical value of the heat of oxidation of the man's muscles working against gravity. This last is of course the difference between the *vis viva* under gravity alone, and the actual *vis viva*. Hence the man wastes tissue *more*, digests *more* food, and radiates more heat than if he were at rest.

I beg to add a word upon the letter of "X." concerning the term potential energy. Used as the term is to denote energy of position, it cannot be considered "felicitous" or logically exact. Energy of position is *potential vis viva* (or kinetic energy); and *vis viva* (against a force) is *potential* energy of position. In the expression potential energy, we are led to inquire *which* energy. As it stands it properly implies the idea of possible *vis viva*, as if "energy" was used only to signify *vis viva* or kinetic energy. It may be noticed that Prof. Helmholtz uses *vis viva* for "energy" (the adjectives actual and latent being understood) in two places; viz., in his tract on "Conservation of Force," p. 128 of translation, "The *vis viva* of a single particle Δm, &c.," and in "Popular Lectures," p. 196, trans., "The *vis viva* of motion of revolution round the sun, &c.," where the algebraic expression given shows that the *whole vis viva*, actual and latent, or, as it is now called, the *whole* energy, kinetic and potential, is meant. While then the German philosopher uses *vis viva* for work-power in general, the English writer in the terms potential and actual energy employs adjectives which logically require that energy should signify *only* work-power of motion. Out of this maze "X" suggests a way by proposing to use the term "energy of tension" for potential energy, with a reminiscence of Helmholtz's "Sum of the tensions;" but if we keep the expression kinetic energy, we require a corresponding *adjective* to distinguish the other form of energy, and what more expressive, more exact, more "felicitous" word could we find than "statical," the word originally employed by Sir W. Thomson? We have here a most appropriate word, supported by a great name, and I venture to suggest that "statical energy" should come into general use.

Arnesby Vicarage, Rugby W. P. O.

Dealers in Zoological Specimens and Models

HAVING had a considerable amount of trouble in ascertaining the addresses of the various dealers in zoological specimens in this country and abroad, I think it likely that I may be doing service to others who like myself are charged with the formation of an educational museum of zoology and comparative anatomy, if I give in the columns of NATURE a complete list of such dealers as I have found useful. They are as follows:—

1. For spirit specimens and dried parts of fish, molluscs, insects, corals, &c.—Cutter, Bloomsbury Street, London.
2. For skeletons, &c., Ed. Jerard, jun., College Place, Camden Town, London.
3. For Ceratodus, insects, &c.—Higgins, 22, Bloomsbury Street, London.
4. For molluscs, and various marine forms—R. Damon, Weymouth.
5. For American fish and amphibians—Prof. Henry Ward, Rochester, New York.
6. For skeletons of fish, &c., &c.—Erber, 7, Sigmundgasse, Vienna.
7. For skeletons and exotic specimens generally—Gustav Schneider, 67, Grenzackestrasse, Basel.
8. For exotic specimens generally—Museum Godeffroy, Hamburg.
9. For Mediterranean fish, molluscs, &c.—Gal, frères, 1, Maritime, Nice.
10. For glass models of invertebrates—Blaschka, 9, Schiesgasse, Dresden.
11. For wax models of anatomy of parasitic worms and of vertebrate anatomy and embryology—Weisker, 13, Thalstrasse, Leipzig.
12. For wax models of vertebrate and invertebrate embryology—Dr. Ziegler, Freiburg, Baden.
13. For live starfishes, Myaarenaria, Cyclopterus, and other forms—J. Thompson, 11, York Place, Southend, Essex.
14. For anemones, and channel marine fauna—R. T. Smith, 25, St. Alban's Street, Weymouth.

I trust that some of your correspondents will enlarge this list, and that such as it is it may be of use.

E. RAY LANKESTER

Ornithology of Costa Rica

IN NATURE, vol. xvi. p. 446, I see that you announce my return to this country. I take the liberty of rectifying two errors in the announcement:—1st. I was five months collecting in Costa Rica (not four months), from the end of December to the end of May. 2nd. I have brought home 250 species (not 200), and it may interest your readers to know that among these 250 species, besides the female of *Carpodectes nitidus*, are also some other very rare birds many of which—one or other of the sexes—are new to science. I add a list of some of them in case you may feel disposed to give it in your journal.

Odontophorus guttatus (Gould), *Dendrortyx leucophrys* (Gould), *Geotrygon costaricensis* (Lawr.), *Tonurus hoffmanni* (Cab.), *Tetragonops frantzii* (Sclat.), *Turdus nigrescens* (Cab.), *Turdus obsoletus* (Lawr.), *Catharus frantzii* (Cab.), *Catharus gracilirostris* (Salv.), *Dendraeca vieilloti* (Cass.), *Setophaga torquata* (Baird), *Phainoptila melanoxantha* (Salv.), male and female (just described by Mr. Salvin, from a single specimen, sex unknown, sent by Mr. Rogers), *Chlorophonia callophrys* (Cab.), *Pezopetes capitalis* (Cab.), *Pyrgisoma cabanisi* (Sclat. and Salv.), *Pyrgisoma leucotis* (Cab.), *Eugenes spectabilis* (Lawr.), both sexes, *Oreopyra hemileuca* (Salv.), *Oreopyra cinereicauda* (Lawr.), *Selasphorus flammula* (Salv.), both sexes, *Panterpe insignis* (Cab.), ditto, and several new species belonging to the families *Fringillidæ*, *Trochilidæ*, and *Tyranidæ*, of which I am preparing a description, as well as a general list of all the species collected by me (with notes on many of them), for publication in the *Proceedings* of the Zoological Society.

A. BOUCARD

55, Great Russell Street, W.C.

On the Supposed Influence of Light on Combustion

BEFORE Dr. Ingleby referred to my experiments as "inconclusive," his reference should at least have been accurate. He says that I "actually used a dark cubbard into which there was no free influx of atmospheric air." So far from this being the case, the "dark closet," as I call it in my paper, was the photometer-room of Price's Candle Company, an enclosure 12½ feet long, 3½ feet wide, and 6½ feet high, with arrangements for constant ventilation both at the bottom and at the top. So far from candles "naturally burning there with inferior combustion," as Dr. Ingleby supposes, it is in constant use for testing the burning of candles, and any deficiency in the supply of air would be shown quickly by the production of smoke, and yet after being so used for many hours there is not a trace of smoke in the air.

Dr. Ingleby's assumption that the candles burnt with inferior combustion in the closet is in direct opposition to the statement made in my paper. In the first and fourth trials there is a greater consumption in the light than in the dark; and in the second and third trials the consumption is greater in the dark than in the light; but in any case the difference is so small, amounting only to from two to seven grains per hour, that it may fairly be referred to slight differences in temperature, in currents of air, and in the composition and make of the candles—the *cæteris paribus* which Dr. Ingleby, with unnecessary emphasis, says I "left *entirely* out of the experiment."

The method adopted by me has the advantage of measuring the results by actual weighing, and I attach no importance to any opinion that is not founded on a similar basis. I cannot follow Dr. Ingleby's theory. What does "insidious eclipsing the waning glimmer of expiring embers," mean? I can understand that sea-coal—a caking coal—may form hard cakes, below which the fire burns out unless the cake is broken, an action which does not occur with non-caking coals such as a great part of the Staffordshire and Lancashire coals, but I cannot see why if the "last faint gleam is invisible" in consequence of a brighter light, therefore "the fire goes out as a matter of course." That the sun puts out the fire by rarefying the air necessary for combustion I take to be pure fiction.

In my experiments differences of temperature were slight. If there was any difference one would expect the temperature to be higher in the closet than in the open room, but in the fourth trial the temperature in the sunshine was the higher. If the candle material melts down more rapidly in consequence of increased temperature, then up to a certain point, at which guttering begins, the rate of consumption of the candle material will increase. But in these experiments there was no guttering and no smoking of the wick. C. TOMLINSON

Highgate, October 13

Selective Discrimination of Insects

THE remarks of Sir John Lubbock in a late lecture on the relation of insects and flowers leads to the inference that in his opinion the brilliancy of colour rather than the odour is the attraction. My observations lead me to suppose that it is not the colour, but the particular odour of each variety or species of flower which induces the visit. With great interest, not unmixed with curiosity, I have observed (my attention was at first casually excited) that bees particularly, and also butterflies, visit a distinct variety and for the time confine their attention to it, settling on and sucking the honey of that variety only; *e.g.*, a bee settling on a scarlet geranium will not go from it to another species or variety, but gives its attention to the particular variety only, irrespective of colour, whether scarlet, pink, or white, never going from a scarlet geranium to another scarlet flower, even if in contact. Whatever the species of flower, it is the same—pelargoniums, petunias, heliotropes, lilies, &c. The visit is from pelargonium to pelargonium, not from pelargonium to geranium (both cranes bills), and from lily to lily, irrespective of colour. I never remarked a bee go from a lily to an amaryllis, or the reverse. The object of this distinctive selection appears to be fertilisation. The indiscriminate admixture of the pollens of distinct varieties would probably frustrate the ends of nature and lead to monstrosities or barrenness. What would be the effect of the admixture on its own stores is a distinct question. So far as the insect is concerned, doubtless the fact has relation to its own economy. Whatever be the reason, there appears to be the harmonious adjustment of two facts under the relations of one law. If the colour, and not the odour, was the attraction, the visits would be indiscriminately made to all flowers of a brilliant hue. The observation of the lecturer as to flies being attracted by stinking plants or carrion seems to prove the fact suggested. Flies settle indiscriminately on all putrefactions, and will go immediately from a flower to offal or from offal to a flower. With bees and butterflies there is certainly a discriminative selection guided by odour; I have also remarked that some flowers are rarely, if ever, visited by bees.

I have never in the books I have read met with this observation, and when so acute and distinguished an observer as Sir John Lubbock passes over the circumstance, I presume either the fact has not been observed, or, if observed, has been considered to be inconsequential. The observation may be worth nothing, but in these days of *minute science*, when every infinitesimal variation is noticed and invested with importance, there may be a significance in the fact which escapes me, but which, with others, may have its value. So far as I know, the occurrence is invariable; being so, the inference is that odour, and not colour, is the attraction. I have called the attention of others to the occurrence, who have, watching the results, always come to the same conclusion as myself. S. B.

OUR ASTRONOMICAL COLUMN

THE NEBULA, MESSIER 8 (G.C. 4361).—Dr. Tempel draws attention to the different appearance presented by this large nebula at the present time from that depicted in Sir J. Herschel's drawing made at the Cape of Good Hope in 1836-37, which he considers can only be explained on the assumption of a shifting of the whole nebula with respect to the stars by reference to which it was delineated at the Cape, or by great changes in the nebula itself. The case will be worthy of attention, because it appears Sir J. Herschel's drawing was made with much care, as he says "every attention has been paid to exactness." The whole area occupied by the nebula, so far as he could trace its convolutions, is stated to be about one-fifth of a square degree. The relative positions of the stars in and near it, to the number of 186, were ascertained by differential observations with 9 Sagittarii; "from these measures skeleton charts were then constructed, and being divided into convenient triangles, the nebula was worked in upon them." A drawing made under these circumstances might certainly be expected to represent its actual features, and it appears to be given with confidence by Sir J. Herschel. Dr. Tempel, observing with the large Amici-telescope at Arcetri, near Florence, finds that the reference stars entered in the Cape drawing are still as they then were, with some insignificant variations of position or brightness; but the difference of the details of the nebula as projected on these stars, from those shown by Sir J. Herschel, are so marked as to leave, in Dr. Tempel's opinion, no other explanation than is suggested above. Prof. Schiaparelli, to whom Dr. Tempel had forwarded his own delineation of the nebula for comparison with that made at the Cape, remarks, after twice examining it:—"Je dirai tout-de-suite, qu'il m'aurait été impossible de reconnaître la nébuleuse avec le seul dessin de J. Herschel." The nebula is figured on Plate I. in the Cape observations; the description will be found at p. 14. Probably Mr. Ellery, who, as was stated last week, is still occupied with new drawings of Sir J. Herschel's figured nebulæ, may be able to express an authoritative opinion with respect to the supposed changes in this object.

THE BINARY STAR *a* CENTAURI.—It appears from the supplementary number of the *Monthly Notices* of the Royal Astronomical Society that this star has not been neglected during the critical portion of its orbit at the observatory at Sydney. Mr. H. C. Russell, the director, publishes measures taken in each of the years 1870-77, excepting 1875, when he was in Europe, and expresses his intention to observe it accurately during the next few months, that the true time of the periastre, &c., may be determined. The later measures indicate the necessity of a correction for bias, the observer getting sensibly differing angles of position according as the telescope was east or west of the pier—in which, by the way, he is not singular; the amount of the necessary correction was to be investigated. It is very satisfactory to find that a sufficient number of measures of this grand binary system, for obtaining pretty good elements of its motion, are likely to be put upon record at the present periastre. The next we know will not occur until the middle of the ensuing century.

JUPITER'S SATELLITES.—On October 8, M. Yvon Villarceau laid before the Paris Academy of Sciences a memoir, by M. Glasenapp, on the satellites of Jupiter which appears to have been forwarded in competition for the *Prix Damoiseau*, and which had been found amongst the papers of M. Leverrier, one of the commission to whom the adjudication of the prize had been referred. It is known that M. Glasenapp has been occupied for some time past at Pulkowa upon investigations connected with these bodies.

THE PRESENT COMETS.—The comet discovered by M. Coggia on September 14, though faint, is still well situated for observation in the morning sky. The following elements calculated by Herr E. Hartwig, from observations on September 14, 18, and October 6 have been received from Prof. Winnecke:—

Perihelion passage September 10·7566 M.T. at Berlin.

	°	′	″	
Longitude of perihelion	108	10	57	M.Eq. 1877·0.
,, ,, ascending node	251	3	52	
Inclination	77	51	6	
Log. perihelion distance	0·197506			

Motion—retrograde.

Hence the following positions for Berlin midnight:—

	R.A. h.	R.A. m.	N.P.D. °	N.P.D. ′	Distance from the earth.	Distance from the sun.
Oct. 19	7	39·8	54	3	1·234	1·662
,, 23	7	28·1	56	16	1·166	1·680
,, 27	7	14·9	58	48	1·102	1·699
,, 31	7	0·0	61	42	1·044	1·720
Nov. 4	6	43·5	65	3	0·994	1·743
,, 8	6	25·6	68	49	0·953	1·766

Of the comet discovered by Dr. Tempel at the Observatory of Arcetri, near Florence, on October 2, the following elements by Dr. Schur are also from Prof. Winnecke :—

Perihelion passage June 27·970 M.T. at Berlin.

Longitude of perihelion	83° 30′·0
,, ,, ascending node	184 17·8
Inclination	64 54·2
Log. perihelion distance	0·00994

Motion—retrograde.

On June 28 the comet was in R.A. 5h. 51m., N.P.D. 34°·4, distant from the earth 1·71 ; on August 1 in R.A. 4h. 47m., N.P.D. 38°·8, distance 1·35 ; and on September 3 in R.A. 2h. 36m., N.P.D. 55°·4, distance 0·79, so that an earlier discovery might have been expected.

The places subjoined are from these elements for 12h. G.M.T. :—

		R.A. h. m.	N.P.D. ° ′	Distance from the Earth.	Distance from the Sun.
Oct.	18	23 5·5	112 54	1·241	2·036
,,	20	23 2·0	113 48		
,,	22	22 58·9	114 37	1·350	2·084
,,	24	22 56·0	115 20		
,,	26	22 53·5	115 58	1·462	2·132
,,	28	22 51·3	116 31		
,,	30	22 49·4	117 2	1·577	2·180
Nov.	1	22 47·7	117 29		
,,	3	22 46·3	117 54	1·695	2·227

BIOLOGICAL NOTES

BORING POWER OF MAGILUS.—We have received from Mr. Charlesworth a preliminary note giving briefly a result of his study of the genus Magilus, the remarkable testaceous gasteropod that is found immersed in the large hemispherical corals of the genus Meandrina. The current belief, as set forth by Sowerby, Owen, Woodward, and other authorities in molluscan biology who have treated of this coral-inhabiting mollusc, is that Magilus in its young state effects a lodgment in a crevice of a Meandrina, and that as the coral enlarges, the Magilus extends the margins of the mouth of its shell in the form of a cylindrical corrugated tube, the growth of this tube and of the coral proceeding together *pari passu*, and consequently that there is no penetration of the coral by the Magilus at all. Mr. Charlesworth, however, finds that Magilus not only drives through solid masses of coral in any direction with apparently the same facility that the bivalve Teredo tunnels masses of wood, but he finds that it even surpasses Teredo in its power of suddenly reflecting its shell and returning to the point from which it commenced its advance ; and this bending back of the shell upon itself is not accomplished in such natural cavities as frequently prevail in large corals of the Meandrina genus, but in the solid mass of the coral.

GREAT VITALITY OF ANTS.—Several interesting observations have been made by the Rev. H. C. McCook on the endurance of extremes of heat and cold by ants. This year a formicary of *F. pennsylvanica* was cut from an oak bough and exposed out of doors to the rigour of a mountain winter, and survived. A number were dropped separately upon ice, and were found alive after forty-eight hours, each in a little depression. *F. rufa* was found active in its formicary at 34° F., sluggish at 30°. The extreme of heat seemed also to be endured by *F. pennsylvanica ;* they did not suffer at all from the heat of stones walling in a camp fire, having been driven into this position out of a burning stump. A community of agricultural ants (*M. molefaciens*) lived in a mound upon which some smiths in Texas made their fires for heating waggon tires. Numbers of ants were seen at work by Dr. Lincecum, cleaning out the entrance to their city, before the entire extinction of the fire just used for heating tires. They had learnt all about the fire, and knew how to work in and around the dying embers without injury. A quantity of mason ants (variety of *F. rufa*) observed by Mr. McCook were accidentally flooded under five inches of water, and they appeared to be quite dead, and floated about in this condition for many hours. But subsequently most of them recovered full activity. In Texas Mr. Lincecum found that the agricultural ants are seen in great numbers in wells, forming a sort of floating mass as large as an orange, clinging together. In this condition they get drawn up in the bucket, and though they may have been in the water a day or two, they are all found alive. Yet individuals cannot survive under water more than six minutes ; and life in these balls can only be preserved by the mass revolving, either by the continued struggles of the individual insects, or by an instinctive and orderly movement of the outer tier of ants (*Proc* Acad. Nat. Sci. Philadelphia, 1877, p. 134).

THE STRIPED MULLET.—This fish, so abundant off the coast of North Carolina, seems to suffer from several serious drawbacks, which would appear to threaten its extinction. It moves through the water so slowly that a man may easily walk as fast. The young fry suffer from a disease which gradually destroys the sight, and great numbers perish ; they are also much infested with parasitic worms. To counterbalance these destructive agencies, the female has an enormously distended roe.

THE MEDITERRANEAN FLORA.—From personal observations in Italy and Greece, with the aid of literature bearing on the subject, M. Fuchs comes to the conclusion that the so-called Mediterranean flora, so far as represented by evergreen woody plants, and plants of the sage, thyme, lavender, and rosemary order therewith always associated, occurs, at least in France, Italy, Greece, Southern Russia, and Northern Asia Minor, exclusively on calcareous formations, while soils with little or no lime (granite, gneiss, flysch, sandy and muddy alluvia of rivers) in the whole of that region, and south to Sicily and Morea, bear exclusively deciduous foliaceous trees, and in general, a vegetation hardly differing from the ordinary central European flora. We are not, however (M. Fuchs says), to conceive the phenomenon as if the former class of plants required the lime as nutriment ; the correct view rather is, that the southern evergreen flora is better able to press northwards on the drier and warmer calcareous formation, than on the damper and colder clayey soil. And he finds support of this view in the fact that, in the Azores, Madeira, and the Canary Islands, with a truly subtropical climate, an evergreen shrub vegetation closely agreeing with the Mediterranean flora flourishes on various soils indifferently, even on basaltic and trachytic rocks. The same appears to be the case in Algiers.

FOX TALBOT

HAD the photographic art never been invented, Mr. W. H. Fox Talbot, whose death we recently recorded, would have a claim to take a good rank as a scientific investigator. In the popular estimation his work in connection with photography is what alone gives him a claim to remembrance ; but we are sure there are many of our readers who must be familiar with writings by him in various departments of science. He was indeed in many respects a wonderful man, and a glance at the Royal Society Catalogue will show that he has left behind him a great amount of varied work. In mathematics, in physics, in chemistry, in astronomy, in botany, in archæology, in literature, Fox Talbot at various periods of his life did substantial work, and in addition filled faithfully and liberally the responsible position of an English country gentleman on his estate of Lacock Abbey, Wiltshire.

Fox Talbot was the eldest son of Mr. William Davenpor Talbot, his mother being a daughter of the Earl of Ilchester He was born in February, 1800, and received his early education at Harrow. Thence he went to Trinity College, Cambridge, where he gained the Porson Prize in 1820, was Chancellor's Gold Medallist, and graduated in 1821 as Twelfth Wrangler. Just after the passing of the first Reform Bill he sat for two years in Parliament as member for Chippenham, when he retired from public life, and devoted himself almost entirely to work in various departments of science and literature. In the Royal Society's Catalogue alone is a list of about fifty papers by him in various domains of science, and ranging from the year 1822 the year after his graduation, down to 1872. The first paper on the list is a mathematical one contributed to Gergonne's *Ann. Math.* (1822), "On the Properties of a certain Curve drawn from the Equilateral Hyperbola." In 1822-23 he contributed six mathematical papers to the same journal, one of them being "On a Curve the Arcs of which represent Legendre's Elliptic Functions of the first kind." He was the author of at least eight other mathematical papers contributed to the Royal Society, the *Phil. Trans.*, and the *Transactions* of the Royal Society of Edinburgh. Some of these papers are very remarkable, as those on Definite Integrals, and show Fox Talbot to have been a mathematician of no small power.

He seems to have commenced his researches on light at an early period. There is, for example, in the *Edinburgh Journal of Science*, for 1826, a paper describing "Some Experiments on Coloured Flames;" and in the *Quarterly Journal of Science*, for 1827, one "On Monochromatic Light." Other papers in the same direction appear in the *Phil. Mag.*, for 1833, "On a Method of Obtaining Homogeneous Light of Great Intensity," "Experiments on Light," 1834, "On the Nature of Light," 1835 In 1861 he published in the *Chemical News* papers on "Early Researches on the Spectra of Artificial Light from Different Sources," and "Some Experiments on Coloured Flames;" and so late as 1872, we find in the *Proceedings* of the Royal Society of Edinburgh, "Notes on Some Anomalous Spectra," "On the Early History of Spectrum Analysis," and "On a New Mode of Observing Certain Spectra."

In chemistry, as might be expected, his researches were many, being mainly connected, however, with photography. One of his earliest chemical papers will be found in the *Phil. Mag.* ii. 1833: "Remarks on Chemical Changes of Colour." We find other papers contributed mainly to the *Phil. Mag.* on Nitre, Iodide of Silver, Iodide of Mercury, &c.

In January, 1839, Daguerre published his account of his process. On the 31st of the same month Fox Talbot gave an account of his own process to the Royal Society, in a paper entitled "Some Account of the Art of Photogenic Drawing, or the process by which Natural Objects may be made to delineate themselves without the aid of the artist's pencil" (*Roy. Soc. Proc.* 1839; *Phil. Mag.* xiv. 1839); and at the meeting of the British Association that year he read a paper on the subject. From that time onwards he continued to write papers in connection with his invention, though for several years before his death he seems to have lost his interest in the subject, and turned his versatile intellect to other lines of inquiry.

The original photogenic drawing is nothing more nor less than the silver printing process of the present day, which has received little or no modifications since it passed out of his hands, unless it be the application of albumen to the paper and the fixing with sodium hyposulphite. Early in 1840 a new process due to Talbot created a sensation in scientific circles, the results being a marked advance on everything that up to that time had been produced. This was no other than the Calotype or "beautiful picture" process, a patent for which he took out dated 1841. The main features of this process may be described as the production of a photographic picture on sensitised silver-odide, held *in situ* in the pores of paper, and its development by means of gallic acid. The credit of the discovery of this method of development has often been ascribed to Fox Talbot; but we believe that to the Rev. B. J. Reade it is really due, but was so modified by Fox Talbot as to render it manageable in the hands of the operator. The next patent that Fox Talbot took out was registered under the title of "Improvements in Calotype," in which, amongst other things, he included fixing the photographic image on the paper by means of sodium hyposulphite, a solvent for the haloid salts of silver which Sir John Herschel had used in February, 1840.

The third patent taken out by Talbot, in conjunction with Malone, was for the use of unglazed porcelain in lieu of glass, on which to support the photographic image, using an albumen process. In this patent also we have a protection granted for an invention which has several times since been rediscovered, viz., the use of a transparent and flexible support in lieu of glass capable of being adapted to a curved surface, by which means a panoramic view might be taken in the camera by the gradual rotation of the lens round its optical centre. This flexible support was paper rendered transparent and non-absorbent of the liquid albumen applied to its surface. The last novelty included consisted of an application of photography to the production of an image on steel plates, doubtless with a view of helping the engraver.

The fourth patent was for a process (described in the *Athenæum*, December 6, 1851) by which instantaneous pictures could be taken, and was so sensitive that an experiment undertaken at the Royal Institution to prove its value is worthy of redescription. Printed matter was fixed on a wheel which was caused to revolve at a rapid rate, and being illuminated by the spark from a battery of Leyden jars, a facsimile of it was produced in the camera, "every letter being perfectly distinct." We doubt if at the present day any greater degree of instantaneity could be secured even by the most rapid collodion processes extant. The success of the process was due to the extreme sensitiveness of silver iodide when prepared by double decomposition of the iron salt, and also to the great facility with which silver nitrate could be reduced by ferrous sulphate. The debt he owed to Dr. Woods, of Parsonstown, and to Robert Hunt, who respectively discovered these facts, Talbot duly acknowledged in his communication to the *Athenæum*.

The last patented invention in photography with which Fox Talbot's name is connected was that of photographic engraving. This process is based on the discovery by Poitivin, of the possibility, by exposure to light, of forming an image in gelatine when impregnated with bichromate of potassium. The steel-plate on which the etching was to be engraved was covered with a dried layer of thin chromated gelatine, and after exposure in the camera the plate was placed in cold water to remove part of the gelatine and as much of the bichromate as possible. It was then covered with the etching fluid which penetrated in a greater or less degree through the gelatine film and the "biting-in" thus effected enabled the plate when inked up and printed in the usual manner to give an impression on paper of the object photographed. This method was most successful in the reproduction of line engravings, and when half tones had to be produced he adopted other artifices to which we need not here refer.

It has been stated that Fox Talbot did not protect his processes, but the above list of patents at once contradicts the assertion. Not only did he—as we think quite justifiably—do so, but he strictly claimed his rights, even going so far as to bring an unsuccessful action for infringement, claiming to include in his Calotype patent—which was essentially a paper process—the collodion process of Le Gray and Archer. Mr. P. Le Neve Foster writes to us that Fox Talbot was so tenacious of his rights that the formation of the Photographic Society was for a time prevented. "I had," Mr. Foster writes, "more than one conversation with him at that time on the subject, and he only yielded, and in favour of amateurs, after much solicitation on the part of the late Lord Rosse and Sir Charles Eastlake, who thereupon became the first president of the Photographic Society."

The accompanying extract from the correspondence which appeared in the *Times* of August 13, 1852, between the inventor of the Calotype process and the presidents of the Royal Society and Royal Academy, shows the spirit in which the two latter approached the subject of the patent rights, and the generous tone in which the former responded :—

"The art of photography on paper," Lord Rosse and Sir Charles Eastlake write, "of which you are the inventor, has arrived at such a degree of perfection that it must soon become of national importance ; and we are anxious that, as the art itself originated in England, it should also receive its further perfection and development in this country. At present, however, although England continues to take the lead in some branches of the art, yet in others the French are unquestionably making more rapid progress than we are. It is very desirable that we should not be left behind by the nations of the Continent in the improvement and development of a purely British invention ; and, as you are the possessor of a patent right in this invention, which will continue for some years, and which may, perhaps, be renewed, we beg to call your attention to the subject, and to inquire whether it may not be possible for you, by making some alteration in the exercise of your patent rights, to obviate most of the difficulties which now appear to hinder the progress of art in England. Many of the finest applications of the invention will probably require the cooperation of men of science and skilful artists. But it is evident that the more freely they can use the resources of the art, the more probable it is their efforts will be attended with eminent success. As we feel no doubt that some such judicious alteration would give great satisfaction, and be the means of rapidly improving this beautiful art, we beg to make this friendly communication to you in the full confidence that you will receive it in the same spirit—the improvement of art and science being our common object."

This letter is dated "London, July," and Fox Talbot replied as follows, under date "Lacock Abbey, July 30" :—

". . . I am as desirous as any one of the lovers of science and art, whose wishes you have kindly undertaken to represent, that our country should continue to take the lead in this newly-discovered branch of the fine arts ; and, after much consideration, I think that the best thing I can do, and the most likely to stimulate to further improvements in photography, will be to invite the emulation and competition of our artists and amateurs by relaxing the patent right which I possess in this invention. I therefore beg to reply to your kind letter by offering the patent (with the exception of a single point hereafter mentioned) as a free present to the public, together with my other patents, for improvements in the same art. . . . The exception to which I refer, and which I am desirous of keeping in the hands of my own licensees, is the application of the invention to photograph taking for sale to the public. This is a branch of the art which must necessarily be in comparatively few hands. . . . . With this exception, then, I present my invention to the country, and trust that it may realise our hopes of its future utility."

In the *Phil. Mag.* iii. 1833 will be found a very curious paper, which might interest Sir Wm. Thomson (who, however, has probably read it), "On the Velocity of Electricity ; a proposed method of ascertaining the greatest depth of the ocean." Crystallography and optics came in for a considerable share of Talbot's attention. In 1836, in the *Comptes Rendus*, we find him describing researches on borax crystals, and besides various papers on the subject mentioned produced in 1836, he gave the Bakerian lecture of that year, the subject being "Facts relating to the Optical Phenomena of Crystals." In 1842 he read a paper at the British Association "On the Improvement of the Telescope," and another in 1847 "On a New Principle of Crystallisation." He describes in the Astronomical Society's *Memoirs* (xxi.) a total eclipse of the sun, July 28, 1851, observed at Marienburg, Prussia, and in the British Association Report for 1871 will be found a paper by him "On a New Method of estimating the Distances of some of the Fixed Stars."

The subject of heat also had its attractions for his many-sided mind, and in 1836 he contributed to the *Phil. Mag.* papers on the Repulsive Power of Heat and on Radiant Heat. Even botany received a share of his attention, for we find in the *Transactions* of the Edinburgh Botanical Society for 1868 a "Note on *Vellozia elegans* from the Cape of Good Hope."

But the half is not told, and it would take up more space than we can spare, even were it quite appropriate in these pages, to refer to his numerous contributions in literature and archæology to the Royal Society of Literature (of which he was vice-president), the Society of Biblical Archæology, and by other methods. Orientalists will call to mind that Talbot was one of the first who, with Sir Henry Rawlinson and Dr. Hincks, deciphered the cuneiform inscriptions brought from Nineveh. He was the author of several books of much interest and learning, and in his "Pencil of Nature," a fine quarto published in 1844, and probably the first work illustrated by photographs, he describes the origin and progress of the conception which culminated in his invention.

THE PHOTOGRAPHIC EXHIBITION

THE Photographic Exhibition which is now open at 5A, Pall Mall East, is well worthy of a visit by all lovers of the art-science, exemplifying as it does the progress that has been made in dry-plate processes. The perfecting of these processes must have a marked effect on the future of photography, as when they are capable of being employed under all circumstances, the heavy paraphernalia attendant on the wet process may be consigned to the lumber-room, and the worker in the field or laboratory need only be dependent on his box of sensitive plates and his camera. We cannot enumerate all the processes, examples of which are exhibited. We may mention, however, that the simple bromide of silver emulsion either held on the plate embedded in collodion or gelatine appears to bear away the palm for excellence, unless it be the process with which Mr. England has produced his splendid collection of Swiss views, in which (though no information is given in the catalogue regarding it), we think we can trace the delicacy due to albumen in the sensitive film, combined probably in some way or another with bromide of silver. Another feature of the exhibition are the enlargements which are shown by various exhibitors, amongst whom we may name, as being specially worthy of mention, the Woodbury Company, the Royal Engineers, and the Autotype Company. The enlargements taken by Mr. E. Viles with the microscope are also worthy of more than a passing remark. They are all beautifully executed, but perhaps the picture of the proboscis of the common blow-fly should be specially singled out, being almost perfectly enlarged to 200 diameters. We believe that a comparatively low-power objective was employed, and that from the small negative obtained by it an enlargement in Monckhoven's solar camera was produced. These pictures are hung too high to be well seen, and Mr. Viles perhaps might be persuaded to show them at some of this season's scientific *soirées*. As regards the application of photography to scientific purposes there are no other examples to be found in the exhibition, a matter which we deeply regret, seeing the large use that is made of the art-science in nearly every investigation of the present day. As regards the artistic element present, it is not in our province to dwell upon it. In many examples of portraiture it would have been well had that abomination — retouching of the negative—been avoided. As showing what a grand pencil is sunlight to the artist, we may mention the exhibits of Robinson, Blanchard, Mrs. H. Roscoe, and Slingsby, in all of which are to be found true artistic feeling and perfect manipulation. The works of Payne-Jennings, Bowness, the Royal Engineers, Stephen Thompson, and England may be classed amongst the best of the landscape work.

Amongst technical work we have examples of a capital photo-relief process by Warnerke, by which an artist's own drawing can be faithfully reproduced as a block for surface printing. The mechanical printing processes from gelatine are also admirably represented by the Autotype Company, as is that known as Woodbury-type.

This notice would be incomplete without calling attention to the photographs taken during the recent Arctic expedition under Sir G. Nares, which have been exhibited by the Admiralty, and also to those taken by Mr. Grant, who accompanied Sir Allan Young in the *Pandora*. Both sets of photographs are very good when the difficulties under which they were taken are considered.

THE NORWEGIAN DEEP-SEA EXPEDITION[1]

THE *Vöringin* left Tromsö on July 14, lay the following day, which was a Sunday, in Kjosen, by Lyngen, and we recommenced our work on the 16th, off Fuglö (71° N. lat.). From this point a cross-section was made to lat 71½°, long. 14° E., the bottom reaching nowhere more than 900 fathoms. On the 18th we steered southwards, and took up another cross-section parallel to the above, and about twelve geographical miles distant. This was finished on the 20th, and we sailed to Tromsö, where we arrived at midnight. In the last cross-section we found a depth of more than 1,200 fathoms on the north-east border of the deep-sea bay abutting on the steep bank outside Vesteralen and Lofoten.

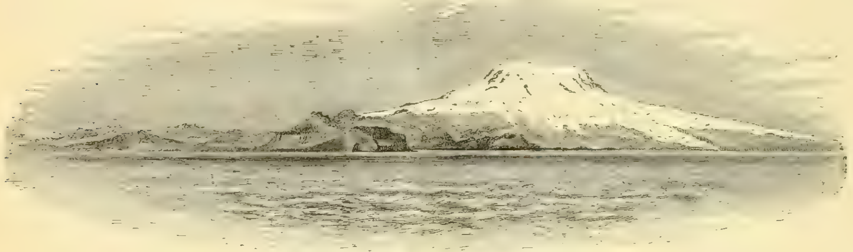

FIG. 1 —Beerenberg, Jan Mayen, from the South-west.

In Tromsö the ship was completely fitted out for our cruise to Jan Mayen. We left that town on July 24, passed out the Malangenfjord, and steered westwards. In lat. 70°, long. 5° E., we reached the cross-section, whose eastern part we had already worked out, and shaped our course directly for Jan Mayen. This was on the 26th, and the dredge came up, full of mud, biloculina clay, but almost without animals. The following day we found 0° C. in 500 fathoms depth, but farther west, in lat. 71°, long. 5° W., the isotherm of 0° C. was found, late in the night, in only twenty fathoms' depth. This proves that we were fairly in the polar current, and that the boundary between it and the warm Atlantic current (the so-called Gulf Stream) is a very steep surface, like that of the "cold wall" on the American coast. The temperature of the surface of the sea was here 4°·6 C. At night the fog came on, and the next day we steered cautiously westwards, sounding at short intervals; but the depths went on increasing to more than 1,000 fathoms, before a less depth was sounded. At last, just when we were sitting at the dinner-table, we heard the mate cry out, "I can see the glacier ahead." The ship was immediately stopped, and a sounding gave 140 fathoms. The fog began to ascend a little, and we were soon able to dis-

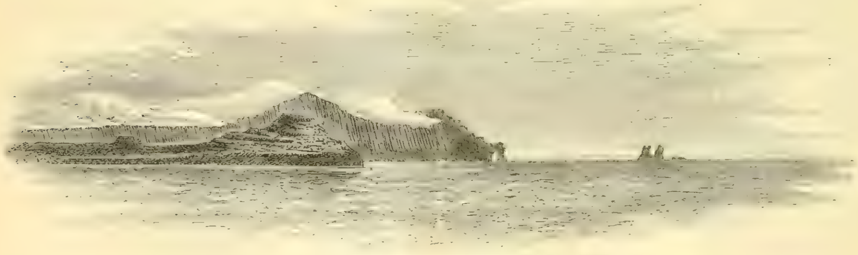

FIG. 2.—Jan Mayen—South-west Cape, and the Seven Rocks.

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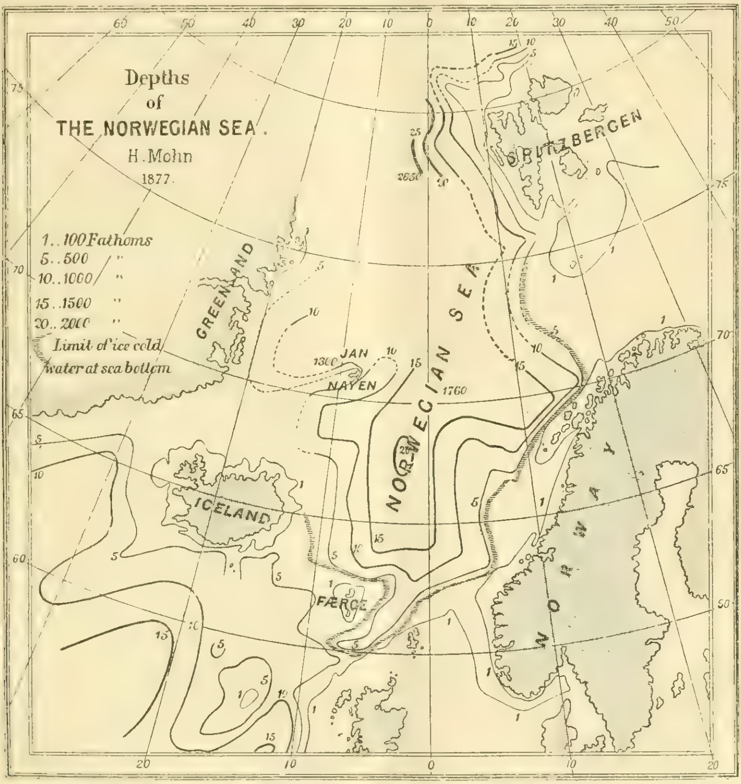
[1] See NATURE, vol. xvi. p. 271.

tinguish a huge glacier, hanging on the steep rock, and bathing its foot in the sea. In order to ascertain the distance from the shore we fired a gun, and the observation of the echo gave the distance a little less than one nautical mile. By-and-by the fog lifted higher, and we could see the two terminating points of the eastern side of the base of the great Beerenberg. In the afternoon we started anew in order to round the island and find shelter on the west side, the wind and sea coming from the north-east. But as soon as we were on a northerly course, abreast of the north point of Jan Mayen, the fog again grew so thick that we could only see a couple of ships' lengths ahead. We steered then first northwards, then westwards, southwards, and south-east, the temperature of the sea surface being taken every quarter-of-an-hour to keep us informed of the proximity of the ice. The surface temperature went down to 2°·3 once, but kept generally at 3°·5, or more. At last when we were approaching the west coast and had commenced sounding to find a suitable anchorage, the fog lifted so that we could see the shore and allowed us to choose our berth. At midnight we dropped the anchor in the northernmost of the three large bays of the west side, about half-a-mile from the nearest shore.

The next day was wonderfully calm, but the fog covered the higher parts of Jan Mayen. We went on shore, the sea being so calm that we could step on shore without any inconvenience. The shore consisted of volcanic sand, quite black, and was, higher up, covered by driftwood thickly strewn on a level surface. To the left was a steep cliff wonderfully rich in colour, the abode of thousands of sea-birds, whose inner slope, consisting of ash and scoriæ, showed it to be a part of a former crater. The scientific party spread in different directions and made the best of the time in surveying, collecting specimens of plants and rocks, and drawing; one polar fox was killed. The plants found belong to very few species, the vivid green we had seen from the ship being only a cover of moss. The flowers had just come out, and all the lower part of the island, up to about 2,500 feet, was generally free from snow, the snow lying only in patches in the lower regions where a larger mass was gathered in ravines. The rocks were all volcanic, and the peaks seemed all to be built of loose stones thrown out from craters, while solid lava and tufa were found in the lower parts. In the afternoon I went on shore again, happily, because this was the last time we were able to do so, the sea being on the following days too high, and Jan Mayen does not present any bay giving shelter for a boat. I directed my course northwards, found a lagoon filled with fresh water, and shut off from the sea by a low wall some 300 feet in breadth.



The wall was covered by driftwood. On the east side of the island there is a similar lagoon, but much longer and with brackish water. On my return to the boat I shot a fox which came suddenly upon me and showed the greatest curiosity at seeing a human being.

The next day the wind and sea had risen, and landing was impossible. We weighed, therefore, and sailed again round the north part of the island. The clouds allowed us to see the lower part of the land, and for a while Beerenberg, the huge volcano of Jan Mayen, showed its snow-white cone to our enchanted eyes. The captain, assisted by his officers and myself, surveyed the coast as far as circumstances permitted, and got abundant material for an improved map of Jan Mayen. The Admiralty chart, constructed after Scoresby and Zorgdrager, proved very good, and afforded us material aid both in navigating and as a base for the improvements we were able to obtain. At midnight we anchored on the eastern side, south of the "egg-crater," and off the great lagune.

The next day we took altitudes of the sun from the ship's deck, landing being impossible. The latitude of the map proved correct; but our chronometers, compared in Tromsö and Bodö before and after our cruise with Greenwich time, sent per telegraph from the observatory in Christiania, showed that Jan Mayen lies about half a degree of longitude farther west than in the chart. Further measures indicated also that Jan Mayen is not quite so long as given in the chart, and that its southern half is somewhat broader.

Next day we weighed, and went eastwards, sounding and dredging. Beerenberg was quite clear, and presented a magnificent aspect (Fig. 1). The winds were at times very high, up to fifteen metres per second, and very variable in direction, a phenomenon which Scoresby mentions in his description of the waters of Jan Mayen. The fauna was very interesting; it had much similitude to that of Greenland. The temperature at the bottom was about − 1° C., and in this cold water many well-known animals reach quite gigantic proportions. At night we anchored outside the lagoon.

The following day we weighed, and steered northwards. The height of Beerenberg was measured, the distance and course of the ship being taken as base line. Three different measures were in close agreement, and the mean was a height of 5,836 feet, which is nearly 1,000 feet lower than the height given by Scoresby. An appreciable current from north-east to south-west would make the number less still. This determination makes Beerenberg lower than the highest point in Iceland, viz., Oeroefa-Jökull, which is more than 6,000 feet high. We went to the north of the island, and found 1,000 fathoms in a distance of only one geographical mile from the north point, which indicates that the foot of Beerenberg continues its slope of 10° so far beneath the surface of the sea. Thence we steered westwards and took soundings, which were continued the whole night. The wind was north-west, and the temperature of the air fell to + 0°·2 C.

A depth of 1,032 fathoms having been found seven geographical miles north-west of Jan Mayen, and a serial sounding having shown 0° C. in a depth of only ten fathoms—surface water being as low as + 2° C., we steered again towards the west coast of Jan Mayen. On the morning of August 8 we were near our first anchorage, but the swell was too heavy to try to land. We then went south-westwards along the shore and studied the country as well as circumstances permitted, the fog sometimes hiding it from our view. We were nevertheless able to get a series of sketches, and our rate told us that the island, as mentioned above, must be shorter than it is shown on the map. At noon we passed Cape Southwest and got a fine view of it (Fig. 2). The point north of Cape Southwest showed two extremely regularly built volcanic cones, the outer one close at the sea, the inner one quite small, and both of a reddish tint. On the higher land between Cape Southwest and this point there is also a similar larger cone. The Cape itself is perforated by a tunnel at the level of the sea.

I got the distinct impression that Jan Mayen is from end to end of comparatively recent volcanic origin. Its aspect reminded me of parts of Iceland, which are of more recent volcanic origin, *e.g.* the peninsula of Reykjanes. It had nothing in common with the doleritic formation of the Färö Islands. In the afternoon a sounding was taken and the dredge lowered some five geographical miles south-west of Jan Mayen. The depth was only 263 fathoms. The dredge brought up lots of stones and a rich fauna. The stones were mainly volcanic, but among them I found a piece of granite, one of quartz, and of green chloritic schist. Here we bade adieu to Jan Mayen; the island was a long time out of sight, covered by the always prevailing fog, and the next morning we obtained eight geographical miles farther south, already 1,050 fathoms. Having sailed some ten geographical miles farther south we sounded again and found 1,004 fathoms and 0° C. at a depth of twenty fathoms. This achieved, all sail was set and the course shaped eastwards; the water was let off the boiler in order to get it cooled and cleaned. The next afternoon this was finished and we started under steam eastwards and homewards. When we were under sail the wind was north-west, but very feeble. The next day observations showed that we had moved south-south-east instead of east—a proof of our being in the Polar current. The following days we sailed in the finest weather eastwards, sounding and taking observations of deep-sea temperatures. On the morning of the 7th we had a depth of 2,005 fathoms. The temperature of 0° C. was later found in a depth of 450 fathoms. In the afternoon of the 9th we sighted land—the southern islands of Lofoten. Next day we entered the Vestfjord, where we dredged on the 11th, and arrived at midnight at Bodö.

In Bodö Capt. Wille took absolute magnetical observations. On the 15th we steamed into the Salton-Fjord and Skjerstad-Fjord, where we dredged and took temperature soundings, which showed, by means of Negretti and Zambra's thermometer, that the temperature was constant (3°·3) from 90 fathoms to the bottom in 270 fathoms. On August 18 we went from Bodö, out in the Vestfjord, and took a temperature series in the same place where we had been on June 22. The result was that the temperature had still its minimum in sixty fathoms, but it was now in this depth 4°·7, and at the bottom, in 140 fathoms, 5°·8. The whole mass of the lower water had consequently had its temperature raised as much as 1° C. The cause of this singular distribution of temperature so late in summer is inexplicable to me. This was our last operation this year. The course was shaped southwards, and on August 23 we arrived at Bergen, where the ship was paid off and the members of the scientific staff returned to their homes.

The expedition has this year been favoured with the best weather. Nothing has prevented the work from being carried on day and night, and the results obtained are therefore very extensive compared with last year.

The accompanying small chart shows in broad features the results of the soundings, combined with those of the Swedish expeditions to Spitzbergen and those of the *Bulldog*, *Porcupine*, and *Valorous* expeditions. The shaded line shows the boundary line at the sea-bottom between the ice-cold water of the Polar Sea and the warmer water of the Atlantic, as far as our observations hitherto have determined it.

Next year the expedition will work up the region between North Cape, Jan Mayen, and the north of Spitzbergen, and possibly make a trip eastwards in the direction of Novaya Semlya, in order to determine the site of the isothermal line of 0° C. at the sea-bottom, which may be regarded as the limiting line for the wanderings of the masses of cod, the object of the great winter and spring fisheries of Northern Norway. For this expedition the Norwegian Storthing has already voted the necessary sum of money.

H. MOHN

NOTES

THE communications from Mr. Stanley in the *Telegraph* of Thursday and Monday last, though containing few positive additions to our knowledge, are full of interest; the episode on the arrival of the starved and wretched party at Ni Sanda is quite thrilling. Notwithstanding the number of cataracts and rapids on the Lualaba—Congo, Stanley maintains it is well fitted to become a great commercial highway—2,000 miles of uninterrupted water communication, opening up an extent of country embracing 600,000 square miles. North of the equator it receives a tributary 2,000 yards wide at its mouth, coming from a little north of east, and which, according to our present imperfect knowledge, is likely enough to be the Welle. Mr. Stanley speaks of the "infamous inaccuracy" of our present charts of West Africa, an inaccuracy which cost him the lives of many of his men, but which, no doubt, he will be able to correct. Three of Stanley's letters are dated from Nyangwe, and were written about a year ago. In them he speaks in the strongest language of the manner in which the slave-trade is carried on in that region, describes the wonderful forest scenery of the country between Tanganyika and Nyangwe, and gives some tender reminiscences of Livingstone preserved among the people, among whom the great traveller sojourned for so long. Mr. Stanley also endeavours to clear up the geography of the region between the Victoria, the Albert, and Tanganyika, showing that the most erroneous and confused ideas on the subject had been accepted mainly on the reports of natives to Sir Samuel Baker. No one now believes that the Tanganyika is connected with the Albert Nyanza, and, indeed, as Stanley suspects himself, he is, in refuting this notion, slaying the slain. From the little foretaste given us in these preliminary letters, there is no doubt that there is a rich feast in store for us of new and valuable information, and of adventure scarcely paralleled in the history of geographical exploration.

THE last number of the *Bulletin* of the Belgian Academy of Sciences contains details as to the plans of the Belgian expedition for the exploration of Central Africa, which is to leave Europe in the course of this month. Dr. Maes, of Hasselt, will accompany the expedition as surgeon and naturalist. The first Belgian station in Central Africa will be placed under the arrangement of Capt. Crespel, with whom Lieut. Cambier and Dr. Maes will be joined. The travellers will start for Zanzibar, and thence reach Lake Tanganyika, where it will be definitely settled whether a station be founded on the shores of the lake, or, a simple depôt being left there, the station be fixed at Nyangwe, or elsewhere in Manyuema. The Tanganyika, or Manyuema, or Unyamwesi will become a basis for further scientific exploration; and agriculture will be carried on on the spot for the purpose of enabling the expedition to exist on its own resources.

WE would draw the attention of those of our readers who are interested in the matter to the announcement in our advertising columns with reference to the next distribution of the Government grant of 4,000*l*. Applications should be forwarded to the secretaries of the Royal Society before December 31.

VOL. VII. of the Royal Society's Catalogue of Scientific papers will be out in a few days.

THE *Gardener's Chronicle* hears that Signor Beccari is likely to succeed Prof. Parlatore as Director of the Herbarium and Botanic Garden at Florence, if arrangements can be made for some other Professor to undertake the duties of lecturing.

THE death is announced, on September 30, at the age of sixty-five, of Major-General Eardley-Wilmot, F.R.S., formerly chairman of the Council of the Society of Arts. At one time he was Director of Gun Factories at Woolwich, served on many Government committees on military matters, and was frequently consulted on scientific and educational subjects connected with the army.

THE Lords of the Admiralty have ordered that sets of the photographs taken during the Arctic Expedition of 1875–76 shall be presented to the British Museum, the South Kensington Museum, the United Service Institution, the Royal Artillery Institution at Woolwich, the Royal Engineer Institution at Chatham, and other Government or official institutions. Fifty sets only are to be prepared, and they will all be identical with the collection now on view at the Photographic Society's Exhibition, Pall Mall.

ABOUT eighty of the leading geologists of Germany assembled together in the annual meeting of the Deutsche geologische Gesellschaft, at Vienna, on September 27. Baron von Hauer, of Vienna, Herr Beyrich, of Berlin, and Prof. Gümbl, of Munich, presided over the three sessions which took place. Among the addresses were—"The Geological Constitution of the Harz," by Dr. Lossen, of Berlin; "The Fauna in the Older Deposits of the Harz and the Geological Position of the Hercynian Formation," by Dr. Kayser, of Berlin; "Phylogenetic Investigations in Phyto-paleontology," by Baron v. Ettinghausen of Graz, &c. Prof. Neumayr, of Vienna, gave an interesting report of his late trip through Greece, and exhibited the geological chart of North Greece, Thessaly, and Chalcis, based on his recent investigations.

THE administration of the Paris National Library inaugurated last Saturday a valuable addition to its internal machinery. A small pneumatic tube has been constructed to all parts of the building for conveying notes from readers asking for books. The new buildings erected on the site of the old lecture-room will be ready in a fortnight, and opened for public inspection. The space available for library purposes will be more than doubled by this addition.

THE earthquake of Monday week, to which we referred in our last number, extended from the Lago di Garda to Dijon, and from Strasburg to Grenoble.

THE French Society of Hygiene has just held its first monthly meeting at the Hôtel de la Société d'Encouragement, under the presidency of M. Chevalier, the eminent hygienist. M. Pietra Santa, the secretary, announced that the number of registered members of the new institution, modelled on the English pattern, amounted to more than 300. A letter from the Sanitary Institute announced that the Société d'Hygiène had taken a diploma of honour at the Leamington Exhibition.

THE Manchester Scientific Students' Association commenced its winter session yesterday, when a paper was read by Mr. Thomas Harrison, F.C.S., on "The Unity of the Senses," with experiments. Other papers to be read are by Mr. J. Plant, F.G.S., on "Silica;" Mr. William Gee, on "Telephones;" Mr. M. Stirrup, F.G.S., Notes on Auvergne—Puy-de-Dôme—Extinct Volcanoes; Mr. E. P. Quin, on "Vertebrate and Invertebrate Animals;" Mr. Robt. E. Holding, on a visit to the Zoological Society's Gardens, London; giving a description of some remarkable Animals and Birds—illustrated by diagrams from life; Mr. Geo. C. Yates, F.S.A., on "A Ramble amongst the Dolmens of the Morbihan."

THE annual *Conversazione* of the Whitehaven Scientific Association took place at the Town Hall of that town on October 9, when the president, Mr. R. Russell, C.E., F.G.S., delivered the annual address before a large gathering. A practical exhibition of the telephone as well as an extensive display of late scientific inventions and objects illustrative of the natural history of the district, rendered the entertainment pleasant and profitable. The programme of the session for the next six months offers an attractive list of lectures, including a series of six from the president on Geology; six from Mr. A. Kitchin, F.G.S., on Light and Spectrum Analysis, and single lectures by fourteen other gentlemen. Among the titles we notice The Chalk, its Origin, Characteristics, and Scenery; How an Animal is Built Up: Flowers, their Shapes, Perfumes, and Colours; &c., &c. The Association is in a flourishing financial condition, owning a house of its own, and is not only popular but succeeds in infusing a healthful love for science into the district about. A library, a museum, and frequent field-days in the picturesque and geologically interesting neighbourhood, evidence the activity of the Society.

THE European Bureau of Longitude held its annual conference at Stuttgart, September 27, General Ibannez, of Spain, presiding. Representatives were in attendance from Austria, Bavaria, Belgium, France, Hesse, Italy, Norway, Prussia, Saxony, Switzerland, Spain, and Würtemberg. Gen. Baeyer, of Prussia, was elected president for the coming year.

AT the meeting of the German Anthropological Society at Constance on September 24, Prof. von Virchow described the results obtained by him in his researches on the colour of eyes, hair, and skin of German school-children, and to which we have already referred. He examined no less than 2,114,153 children. In the whole of North Germany the fair, blue-eyed type with light skin is prevalent. In Mid-Germany the darker individuals become more numerous and reach their maximum frequency at the south-west and south-east corners. The passages from one type to another in a geographical sense are perfectly gradual. Upper Bavaria and Alsace are the extremes, between wichh the fairer type reaches southwards like a wedge.

AT the same meeting Dr. Gross, of Neuveville, on the Lake of Biel (Switzerland), exhibited a number of objects dating from the lake dwellings of the earlier stone period, amongst which some hatchets made of nephrite, a mineral now only found in China, were of special interest. Prof. Desor expressed his opinion that these relics were originally brought from Asia by the lake inhabitants; he believed it quite possible that they may have carried their valuables with them, and this hypothesis would explain the rarity of nephrite hatchets. A keen discussion was raised with regard to the discoveries in the Thayingen cave. It will be remembered that Herren Merk and Messikommer had found several bones from the prehistoric reindeer upon which drawings of animals were carved, besides a rough piece of sculpture representing the head of a musk buffalo. These objects are now in the Rosgarten collection, and many naturalists had believed them to be mere imitations. The result of the discussion, proved them to be perfectly genuine. This, however, is not the case with other pieces sent to France and England, and said to have been found at the same place.

A SPECIAL division of the Paris International Exhibition will be devoted to electricity, so that all the systems of electric lighting may be tested comparatively. The electric light continues to create the greatest interest in Paris. The experiments which we mentioned some time ago have been conducted during forty consecutive days at the Lyons railway station. A force of about 40 horse-power is sufficient to keep going twenty-eight electric lamps, each of which gives a light equal to eighty gas lamps, and works with regularity for ten and a-half hours. The effect is splendid, the whole of the station, except the waiting-room, being lighted *à giorno*. The question of economy, however, is not yet settled. It is not known whether the company will agree to pay a somewhat higher price in order to multiply the power of its illumination. These experiments have been tried on Lontain's system, a modification of Wilde's and Siemens' principle. M. Lontain has contrived to send the current generated by an ordinary Wilde's machine into an electro-magnetic engine called a distributor. The central part being strongly magnetised by the current from a Wilde's machine, a number of electro-magnets are influenced by its rapid rotation, and in each of these an induction-current is generated. These induction-currents are powerful enough to feed three electric lamps, and as there are two series of twelve magnets a single machine could, theoretically, feed seventy-two lamps. Actually, however, it feeds only twenty-eight. Lontain uses a new regulator, which works very well by the dilatation of a small silver wire. By its dilatation this part of the apparatus works a lever system, and brings the carbon electrodes into contact. The French Northern Railway has purchased a number of Gramme magneto-electric machines. They intend to use them at their goods terminus and stores.

AMONG works of scientific interest announced for publication during the coming season we note the following:—Messrs. Macmillan and Co. are about to publish a new work by Prof. Clifford, F.R.S., "Elements of Dynamics; an Introduction to the Study of Motion and Rest in Solid and Fluid Bodies." This book is intended for engineers and students of physical science who are unable or unwilling to devote much time to mathematics. Its method consists in making use of the simpler ideas of motion to teach so much of mathematical processes as is required for understanding the more advanced parts. Also, by the same publishers, "An Elementary Treatise on Spherical Harmonies, and Subjects connected with them," by the Rev. N. M. Ferrers, M.A., F.R.S. Messrs. Longmans have just published of the London Science Class-Books Series, "Astronomy," by Dr. Ball, and "Thermodynamics," by Dr. Wormell. Other volumes to follow are "Algebra," by Prof. Henrici; "Botany," by Prof. McNab; "Biology," by Prof. McKendrick; and "Zoology," by Prof. McAlister. Messrs. Chapman and Hall promise two new volumes of "The Library of Contemporary Science"—"Biology," by Dr. Charles Letourneau and "Anthropology," by Dr. Topinard. Messrs. Trübner and Co. announce: "The Epoch of the Mammoth and the Apparition of Man upon the Earth," by James Southall; "The Parthian Coinage," by Percy Gardner, M.A., and "The Ancient Coins and Measures of Ceylon," by T. W. Rhys Davids, being Parts 5 and 6 of "The International Numismata Orientalia;" "The Birds of Cornwall," by Edward Hearle Rodd; "The Barents Relics," by C. L. W. Gardner; "Chemistry in the Brewing Room," by C. H. Piesse; "Origin and Migrations of the Polynesian Race," by Abraham Fornander, Circuit Judge of the Island of Maui; "Dr. Beke's Discoveries of Sinai," by Mrs. Beke; "A Statistical Account of Bengal," by Dr. Hunter. Mr. Stanford promises: "Africa," edited by Keith Johnston, being the first volume of "Stanford's Compendium of Geography and Travel," a work founded on Hellwald's "Die Erde und ihre Völker;" other volumes to follow "Africa" will be "Europe," by Prof. A. C. Ramsay, "North America," by Dr. F. V. Hayden, and "South America," by Mr. H. W. Bates; "Fifteen Thousand Miles on the Amazon and its Tributaries," by C. Barrington Brown and William Lidstone; "The Physical Geography and Geology of Ireland," by Edward Hull; an English edition of M. De Fonvielle's "Aventures Aériennes." Messrs. Kegan Paul and Co., successors to Messrs. H. S. King and Co., promise "Hygiene and the Laws of Health," by Prof. Corfield; and of the International Series, "Studies in Spectrum Analysis," by J. Norman Lockyer, F.R.S.; "The Physical Geography of the Sea," by Dr. W. B. Carpenter, F.R.S., "The First Principles of the Exact Sciences," by Prof. Clifford, F.R.S., and "The Brain as an Organ of Mind," by Dr. Charlton Bastian, F.R.S. Messrs. Blackie will publish a new edition of Thompson's "Gardener's Assistant, Practical and Scientific," revised and extended by Thomas Moore, F.L.S., Curator of the Chelsea, Botanic Gardens, &c., assisted by several eminent practical gardeners; also "Upper Egypt, its People and its Products," a descriptive account of the manners, customs, superstitions, and occupations of the people of the Nile Valley, the Desert, and the Red Sea Coast, with sketches of the natural history and geology, by C. B. Klunzinger, M.D., formerly Egyptian Sanitary-Physician at Koseir on the Red Sea. Mr. Maclehose, of Glasgow, announces: "Outlines of Physiology," by Prof. McKendrick; Messrs. Collins: "Building Construction," by R. Scott Brown; "Machine Construction," by E. Tomkins; and "Mineralogy," by J. H. Collins, in their Advanced School Series.

IN a paper in the *Journal de Physique*, on the spectrum of the electric spark, by M. Cazin, the author concludes that the electric spark in a gas contains incandescent gas particles, which give a bright line spectrum, and solid and liquid particles which produce the continuous spectrum, the former coming from the gaseous medium and the electrodes, the others from the electrodes and the sides near the spark. If the pressure increases, the solid or liquid particles become more abundant, and their continuous spectrum predominates; at last this makes it impossible to distinguish the bright gas lines, or, in other words, the latter, while the pressure increases, seems to dilate, and eventually flow together into one continuous spectrum. By making photographs of the spectra M. Cazin found his views confirmed. Of the nitrogen spectrum at ordinary pressure he photographed sixty-two lines, using nine cells in the battery giving the spark.

HERR J. STEFAN has lately communicated the results of some interesting researches to the Vienna Academy of Sciences, relating to the heat-conducting power of several substances. The conducting power of copper being taken as unity, he found that of iron to be 0·17, ice 0·0057, glass 0·0016, water 0·0015, hydrogen 0·00039, hard india-rubber 0·00026, and air 0·000055.

IN a recent communication to the Vienna Academy M. Ciamician discusses the spectra of chemical elements and their compounds. He finds, in agreement with Lockyer, that the compound spectra, as well as those of the first order of the elements, consist exclusively of bands; and further, that band-spectra belong to molecules and molecular groups, line-spectra to free atoms. From a comparison of the spectra of thirty-one elements he draws these conclusions: 1. The spectral lines of chemically-allied elements correspond to each other either individually or group-wise, so that each natural group of elements has its own spectrum, which, in the individual members of the group, is different only in that the homologous lines are displaced towards the one or the other end of the spectrum, *i.e.*, increase or decrease in wave-length, and that certain lines or line-groups disappear. 2. The increase or decrease of wave-lengths of homologous lines in chemically-allied elements depends on the intensity of their chemical *vis viva*, a greater wave-length corresponding to a greater chemical *vis viva* of the particular element.

ALTHOUGH for years there has been no scarcity in France through drought, still the want of irrigation is much felt almost every summer in the departments of the Mediterranean region. The French Government is about to take measures which might serve as a hint to the Indian Government. A project is being considered for taking advantage of the waters of the Rhone to irrigate systematically that large and already fruitful country. It is impossible to foresee what wonderful changes may result from such a scheme, which it is contemplated to bring into speedy execution.

THE Annual Report of the Queensland Philosophical Society, 1877, just received, is a satisfactory one. It contains the address of the president, Sir James Cockle, on some of the aspects of the evolution theory.

THE second volume, for 1877, of Dr. Emilio Huelin's "Cronicon cientifico popular," has just been published at Madrid. In a recent number we gave a short notice of the first volume. The second volume is in every respect equal to the first.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus radiatus*) from India, presented by Mr. T. Golding and Miss Ward; a Layard's Flying Squirrel (*Sciuropterus layardi*) from Ceylon, presented by Sir Charles Peter Layard; a Brown Coati (*Nasua nasica*) from South America, presented by Dr. G. P. Best; a River Jack Viper (*Vipera rhinoceros*) from West Africa, presented by Mr. I. J. Kendall; two Red Kangaroos (*Macropus rufus*) from Australia, four Chinese Turtle Doves (*Turtur chinensis*) from Java, deposited; a large-billed Crow (*Corvus culminatus*) from India, purchased; a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

THE LIMITS OF NATURAL KNOWLEDGE[1]

THE subject of my address was excellently treated at the Leipzig meeting in 1872, by Prof. Du Bois Reymond. If I take up the same matter again, I do so because I would consider it from a somewhat different and more universal point of view.

I shall also depart from the form and language in which the subject has hitherto been frequently treated. In its generality the theme easily induces the speaker to make excursions into the philosophical domain and to adopt the corresponding manner of expressing himself. I shall use words only of the simplest and clearest description, and I shall not suppose my hearers possessed of anything but a knowledge of the most elementary phenomena in the various domains of nature. In general matters expression is all the simpler and the more intelligible the closer our ideas approach clearness, and, at the same time, truth. I think it advisable, before entering upon the subject itself, to mention shortly the different ways in which the question of the limits of natural knowledge is generally conceived and answered by naturalists.

Amongst the so-called practical scientific men (*Praktiker*) the view is widely spread that a certain and lasting knowledge and understanding of natural phenomena is, on the whole, impossible. They know that hitherto their systems and opinions have not been permanent, and think that scientific theories generally are only attempts to approach the inaccessible reality, attempts which change their tenor and expression with the views of the time. This is evidently not a view based on principles, but only despair caused by failure, the necessary consequence of wrong method and of scientific incapacity.

The practical scientific man relies upon his experience, as he says. This, however, is gained in the following manner:—Each natural phenomenon is accompanied by different and often numerous causes and other circumstances. It is the task of the investigator to find out what are the effects of each one of these causes and circumstances; and this task cannot, in most cases, be accomplished by mere observation. The practical man then selects some cause or circumstance which happens to appear conspicuous to him, and in this he finds the fundamental cause of the phenomenon. This he calls his experience. We therefore understand how these practical men may hold different opinions upon the same phenomenon, why their views bear the stamp of the scientific epoch, and why in course of time they change. We also understand why the theories based on so-called experience are most fertile in those domains where phenomena are most complicated, as in organic morphology, in physiology, and pathology.

[1] Address delivered at the Munich meeting of the German Association, by Prof. C. von Nägeli, of Munich. (The author, in a note to the German original, remarks that this lecture had to replace another in the programme, which had been promised by Prof. Tschermak, of Vienna. At the eleventh hour Prof. Tschermak announced his inability to attend the Munich meeting, and the author was requested by the secretaries to fill the gap thus occasioned. The address therefore, the author states, bears the stamp of its hasty origin, as it was written during a journey in the Alps, when there was neither sufficient leisure nor opportunity for careful and elaborate work.)

The problem of a natural phenomenon is an algebraic equation with many unknown factors. The practical man looks at the equation and tries to solve it, substituting for one or the other unknown factor a generally large and decisive value; the proof of correctness he does not attempt. It is easy to see that in this manner the solution—and with it the true understanding—will certainly not be attained in all eternity.

The solution of an equation with many unknown factors is only possible if just as many equations can be obtained as there are unknown factors, and if the same unknown factors are contained in all. As this is generally impossible with natural phenomena we try to get equations in which there is only *one* unknown factor. This is done by *scientific* experiment (not by the so-called experiment of these practical men) in which all unknown factors are removed save one, and by which the value and effect of this one can be securely determined.

For a long time physics has adopted this way of scientific experiment. Physiology has only recently recognised it in a more general manner as the only correct one. It is true that by this tedious and time-devouring but yet exclusively safe and progressive method we do not erect large edifices of systems which are only fated to fall to pieces again shortly, but we gain simple facts, perhaps insignificant by themselves, but which retain their value for ever and enable us to find new facts. Thus the stock of recognised facts increases slowly but securely. A snail which takes the straight road for its goal progresses, while a grasshopper, with its bounds in all directions, remains always on the same spot. Thus scientific investigation proves to the empirics by facts, that by the exact method certain and permanent knowledge of natural phenomena may be gained.

Many methodical investigators who by the exact method augment the stock of permanent facts, when asked for the limits of natural knowledge, and thinking a solution based upon principles inadmissible, simply reply, "Belief always begins where knowledge ceases." In saying so the course of their thoughts runs thus: Humanity faces the totality of nature. Its insight constantly masters new domains by dint of meditation and investigation. Thus, for example, in the present time we have progresssed much further in the knowledge of nature than was the case during the middle ages and antiquity, and European civilisation is far ahead of that of the rest of humanity. With progressing mental work the empire of knowledge always increases in extent, and the domain where we must be satisfied with belief decreases as constantly.

This conception has an undeniable value in a certain regard. It gives us a measure of the height which scientific natural knowledge had generally attained in every century, and at the same time a special measure for the different human races and nations, for the different classes in a nation, and finally for every single individual. Considerations of this nature have as much scientific interest to the historian and anthropologist, as practical interest to the theologist, the politician, and a number of others.

The phrase that belief begins where knowledge ceases is an actual solution of the question for certain ends. But with this our interest is not satisfied. We turn to the theoretical part of the problem with special sympathy. We wish to know whether the limit where human knowledge must stop can be determined at all or not—if yes, how far our understanding may penetrate into nature, how much humanity may scientifically understand of nature, if during an immeasurable period, let us say at once during eternity, it is occupied with natural investigations, assisted by all imaginable means—what are the boundaries, therefore, which the scientific understanding of nature can never and under no conditions overstep? what is the fundamental limit between the empire of knowledge and that of belief?

This question deserves all the more to be seriously investigated since it is well known that from two opposed sides the absolute power of the human mind over nature is claimed with complete certainty—with decreasing energy by the natural philosophers, with increasing energy by materialists. The former think they can construct formal nature out of herself, and natural knowledge for them only consists in finding the concrete natural phenomena for the constructed abstract ideas, where, of course, they can in no point be freed from the self-deception that they construct the ideas according to conceptions by the senses instead of out of themselves. The latter admit only force and matter in time and space; and that man, who is built up of matter and force, shall master nature, which is built up of the same factors, seems to them a reasonable idea. Both, natural philosophers as well as materialists, raise man to a flattering height, with regard to his own consciousness and pride; they declare him lord of the world, not the real lord who makes the world, it is true, but yet the imaginary lord, who understands the work of the real lord. Can we lay claim with good reason this eminent position?

Many have often tried to answer this question from different points of view; perhaps one of the best replies was given by my predecessor in this assembly, Prof. Du Bois Reymond, in his much-talked-of and often misunderstood address, "On the Limits of Natural Knowledge." I shall only consider this latter reply, which, in an intellectual manner and in rich, poetical language, adorns and covers the gems of thought with the most beautiful flowers of speech. It would have been useful, and would have shown the right way to many a one who cannot so easily get at the kernel through the shell, if result and proof had been comprised in a few short phrases.

The speaker, like the conqueror of a world in the olden times on a day of rest, wishes to point out clearly the true limits of the immeasurable empire which world-conquering natural science has subjected to its understanding, and arrives at the following three conclusions:—1. Natural knowledge, or understanding, is the reduction of a natural phenomenon to the mechanics of simple and indivisible atoms. 2. There are no atoms of this description, and therefore there is no real understanding. 3. Even if we could understand the world through the mechanics of atoms, we could nevertheless not understand sensation and consciousness through it.

General understanding would no doubt have been facilitated considerably if these results had not been introduced as the limits of natural knowledge, but as the impossibility and futility of natural knowledge. Because, since the speaker does not go beyond this negation, investigating natural science cannot define the limits of a domain which she does not even possess—and if she is even deprived for ever of all insight into material phenomena, it can hardly matter to her, as a deposed potentate, whether or not she might claim the spiritual domain, in case of a supposed accession to power.

We may perfectly agree with Du Bois Reymond's thoughts, and yet be convinced that they are not complete and all-comprising enough to define natural knowledge in all directions, that in their incompleteness they lead to false deductions which contradict our natural scientific conscience, and that it is desirable to treat this question not only on the negative side, but to examine whether the human mind is not capable of natural knowledge, of what nature this knowledge is, and what is its extent.

The solution of the question: In what way and how far may I know and understand nature? is evidently determined by three different things, viz., by the answers to three questions:—(1) The condition and capacity of the Ego; (2) the condition and accessibility of nature; and (3) the demands which we make of knowledge. Subject, object, and copula therefore participate in the solution. A separation of this kind may perhaps be thought superfluous, perhaps even inadmissible, because it may be said that the understanding of the object by the subject is an indivisible process. And yet it is correct, because consideration gives prominence now to the one and now to the other factor, and it is also useful, because it requires exhaustive treatment. The difficulties which are in the way of knowledge with regard to the subject or object, are even most conspicuous if we entirely remove one of the two factors by supposing that it offers no difficulties at all. With regard to the capacity of the Ego to understand the phenomena of nature, the undoubted fact is decisive, that our power of thinking, in whatever condition it may be, only gives us nature as we perceive her by our five senses. If we could not see nor hear anything, nor smell, taste, nor touch anything, we would not know at all that there is anything besides ourselves, nor indeed that we are in bodily existence ourselves.

The condition with regard to the correctness of our conceptions therefore always exists—that our external and internal senses report correctly. Our knowledge is only correct in so far as observation by the senses and internal perception (*die innere Vermittelung*) are correct. But an infinitely great probability exists that both, after all, lead us to *objective* truth, because the errors committed by the single individual or by all, are finally always recognised and proved as such, and because natural science, the further it progresses, knows how to remove more and more all apparent contradictions, and how to make all observations agree amongst themselves.

If we remain satisfied in this direction, the question arises, to what extent and in what fulness the senses acquaint us with natural phenomena. With regard to the extent we need only point out the boundaries in order to make them perfectly clear to everybody. In time only the present and in space only that which belongs to our own circumstances is accessible to us. We cannot directly perceive anything of what happened in the past, and of what will be in the future, and nothing that is too distant in space, or that is of too large or too small dimensions.

With regard to the completeness of sensual perceptions there is another boundary which is generally not thought of, and upon which I must enter a little more in detail. Scientific analysis shows the following:—In the totality of force-endowed matter, which we call world, each particle of matter by all its inherent forces is in relation with all others; it is influenced by all, and in its turn acts upon all, of course according to distances. A conglomeration of particles of course behaves like a single particle; the effect which it causes and receives is the total of the effects of all single particles. The crystal, the plant, the animal, man are acted upon by the presence of all material particles, of each single one by itself and of each conglomeration of particles, and this with reference to all forces which are inherent in them, and consequently with reference to all movements which they perform. But these effects in the infinite majority of cases are so insignificant that they may be neglected as quite imperceptible.

The *theoretical* possibility therefore exists that the human organism may obtain bodily perceptions of all phenomena in nature. But how is this matter in *reality?* What impressions are so powerful that they become perceptible to us, and which of them are lost, being too insignificant? Amongst the beings known to us, man and the higher animals have the advantage, that certain parts have developed themselves into organs of sensation, which are extremely sensitive for certain natural phenomena. These organs of sensation, in the course of numerous and successive species and of innumerable generations within each single species, have been developed from the smallest beginnings to high degrees of perfection.

The ingenious idea of Darwin that in organic nature only such arrangements attained full development which were useful to the individual bearer, is so simple, so reasonable, and agrees so well with all experience, that physiologists, who alone are competent to decide here, agree with him perfectly, and are greatly astonished, that a Columbus should not long ago have placed this physiological egg upon its point.

The degree of perfection which each organ of sensation has attained in development therefore corresponds exactly to the requirements, and there is not one in which the human organism is not far surpassed by some animal species, if to the latter the extraordinary fineness of some particular sensual perception became a condition of its existence. But according to this both the human and animal organisms have only developed organs of sensation for such external influences as bear upon their existence in a favourable or unfavourable sense.

We are endowed, for instance, with great sensitiveness for temperature; it is necessary for our existence, otherwise we might perish through cold or heat without knowing it. We are very sensitive towards light; it acquaints us in the best and quickest manner with all objects which surround us and which may be useful or dangerous to us. On the other hand we are not organised to perceive the electricity which surrounds us. While we perceive the increase or decrease of heat and light, we do not know whether the air in which we breathe contains free electricity or not, whether this electricity is positive or negative. If we touch a telegraph wire we cannot feel whether its particles are electrically at rest or in motion.

It was of no use that the sense for electricity should be developed particularly in man and the higher animals, because it is immaterial for the species whether every year some individuals were killed by lightning or not. If this danger were daily to threaten all individuals, the sense for electricity which the lowest animals possess in its first beginnings in the same degree as they possess those for light and heat, would necessarily have developed itself further. We would then perceive by a special organ of sensation the vicinity of a substance in electric tension and be able to escape the stroke of lightning. We would perceive small changes in the electric state and weak electric currents in our vicinity, and also be able to peer into the secrets of the telegraph wire. The want of such an organ might easily have been the cause of our total ignorance of electricity. We can very well imagine the atmosphere of the earth without lightning and thunder. These great electric discharges have helped us to the knowledge of electricity. If accidentally they had not happened, if, moreover, some quite accidental experiences which revealed an attractive and repulsive force generated by friction had not been made, we very probably would have had no idea of electricity, no idea of that force which doubtless plays the greatest part in organic and inorganic nature, which materially affects chemical affinity, which in all molecular motions in organised beings acts perhaps more decisively than any other force, and of which with regard to still mysterious physiological and chemical phenomena we expect the most important explanations.

Our senses are indeed only organised for the requirements of our bodily existence but not to satisfy our intellectual cravings,—to acquaint us with all phenomena of nature and explain them as well. If at the same time they perform this function it is only incidentally. We therefore cannot rely upon our sensual perceptions acquainting us with *all* phenomena of nature. Just as in the case of electric phenomena, which occur in every material particle, we have, as it were, learnt something only accidentally, it is easily possible, indeed very probable, that there are still other natural forces, other forms of molecular motion, of which we obtain no sensual impressions, because they never unite to any remarkable outcome, and therefore remain hidden to us.

Our power of perceiving nature directly by our senses is therefore very confined in two aspects. On the one hand we are probably deficient of the power of sensation for whole domains of natural life, and on the other, as far as we really have this power it is confined in time and space to an insignificantly small part of the whole.

It is true that our natural knowledge is not confined to what we perceive with our senses. By conclusions we may also obtain knowledge of what our senses do not reach. The farthest planet of our solar system, Neptune, was known by calculation with regard to its position, its size and weight, before astronomers had discovered it with the telescope. We know, although we cannot see it with the best microscopes, that water consists of infinitesimal particles or molecules which are in motion, and if it is sugar-water or salt-water, we know perfectly the proportionate weight and the proportionate number of the water, sugar, and salt particles of which it is composed.

By conclusions from facts which were recognised by the senses, we arrive at facts equally certain which can no longer be perceived by the senses. We might therefore, perhaps, indulge in the sanguine hope that starting from the small domain which is opened to us by our senses, little by little the entire domain of nature will be conquered by reason. But this hope can never be fulfilled. As the effect of a natural force decreases with its distance, the possibility of knowledge also decreases as the distance in space and time increases. Of the condition, the composition and the history of a fixed star of the least magnitude, of the organic life upon its dark satellites, of the material and spiritual movements in these organisms we shall never know anything. In the same way the possibility decreases of discovering a still unknown natural force, a still unknown form of motion of the smallest material particles, the less this force or motion possesses the peculiarity of accumulating and causing some collective effect. We may consider ourselves fortunate if ever we obtain only a notion of such a force.

The confined capacity of the Ego therefore allows us only an extremely fragmentary knowledge of the universe.

We now pass from the consideration of the subject to that of the object, *i.e.*, the condition and accessibility of nature. The boundaries, which nature herself opposes to our knowledge, are most evident if we adopt the hypothesis that man, on his side, has the most perfect capacity for natural knowledge. This would be the case if the obstacles of time and space did not exist for him, if he could judge of every phenomenon in the past as well as he can of everyone in the present; if the most distant object did not present more difficulty to him than one in his immediate vicinity, and if he could as easily survey the largest systems of fixed stars and the smallest atoms, as he can a body of his own size; if finally he were provided with senses so perfect that all phenomena of nature, all forces and all forms of motion could be perceived directly by him.

A human race, provided with these perfections, might perhaps be enabled to try the solution of Laplace's problem. Laplace says: "A mind, which for a given moment knew all forces which are active in nature, and the respective positions of the beings of which she consists, if it were comprehensive enough to analyse these data—would unite in the same formula the motion of the largest heavenly body and of the lightest atom. Nothing would be uncertain for it, and the future as well as the past would be present to its gaze. The human mind, in the perfection which it has been enabled to give to astronomy, offers a weak reflection of a mind of this description."

But even a mind as universal as that supposed by Laplace would not be able to solve the problem given. Because the other supposition, of which Laplace does not speak, but from which he starts unconsciously, is the finiteness of the world in all directions, and this is not given. The difficulty which nature opposes to human knowledge is her *endlessness*, endlessness of space and of time, and of everything which depends on this as a necessary consequence.

In space nature is not only infinitely large; she is endless. The ray of light travels through some 190,000 miles in one second; to travel through the whole known universe of fixed stars it would require some twenty million years according to a probable estimate. Let us place ourselves in thought at the end of this immeasurable space, upon the farthest fixed star known to us, then we would not look out into empty space there, but we would see a new starry firmament. We would again believe that we were in the middle of the universe, in the same way as now the earth appears to us as the centre of the universe. And thus we may in thought continue endlessly the flight from the farthest fixed star to the farthest fixed star, and the actual starry heavens we now see, compared to the universe, are after all still infinitely smaller than the smallest atom compared to the starry heavens.

What applies to space applies equally to grouping in space, to the composition, organisation, and individualisation of matter, which is the object of descriptive and morphological natural science. Everything we know consists of parts, and is in itself part of a bigger whole. The organism is composed of organs, these of cells, and the cells of smaller elementary particles. If we analyse further we soon get to chemical molecules and the atoms of chemical elements. The latter certainly still resist further sub-division at present, but we must nevertheless look upon them as compound bodies on account of their properties. Thus in thought we may continue sub-division further and endlessly. In reality no physical atoms in the strict sense of the word can exist, no little particles which would really be indivisible. All size, indeed, is only relative; the smallest body in existence which we know, the particle of the light-and-heat ether may be of any size we choose for our conception, even infinitely large, if only we imagine ourselves to be sufficiently small by the side of it. Just in the same way as *indivisibility* never ceases, we must suppose, by analogy of what we find confirmed in the whole domain of our experience, that the *composition* also of individual particles separated from one another, continues endlessly downwards. In like manner we are forced to suppose an endless composition upwards in always larger, individual groups. The heavenly bodies are the molecules which unite in groups of lower and higher orders, and the whole of our system of fixed stars is only a molecular group in an infinitely larger whole, which we must again suppose to be a unit (*einheitlicher*) organism, and only a particle of a still larger whole.

As space is endless in all directions, so time is endless on two sides; it has never begun and will never cease. The Bible says: "In the beginning God created heaven and earth," and geologists say: "In the beginning the world was a gaseous mass, from which heavenly bodies formed by condensation." But this beginning is only a relative one, the beginning of a finiteness, and the time which has passed since this beginning is only as a moment compared to the eternity before.

From the union of time and space an empire of phenomena results, which forms the contents of descriptive natural sciences as well as of the other part of the investigation of nature, viz., the physical and physiological sciences. Matter, which fills space, is not at rest but in motion, and as the material particles act upon one another with different (attractive and repulsive) forces, each body which moves causes the others to move as well, or rather it changes their motions. It gives off a part of its motion and of its potential energy to others, and these again to others, and so on. This is the chain of cause and effect, also an endless one, as in our conception it neither could begin with a first cause nor can finish with a last effect.

Nature is everywhere uninvestigable where she becomes endless or eternal. We cannot, therefore, conceive her *as a whole*, because a process of conceiving which has neither beginning nor end, does not lead to conception. And this is the reason why Laplace's problem is futile from the beginning. Of course we are permitted to make any supposition we like, even one which for some reason or other is impossible, but not one which is unthinkable. But a formula is unthinkable for which we have not even got the component factors, and which if these factors were given, would never come to an end. The knowledge of *all* forces, which is required for Laplace's formula, supposes that the bodies are subdivided down to their last force-endowed particles, and this is impossible on account of divisibility being endless. The elements therefore are wanting, from which we might compose the formula, viz., the simple natural forces; we cannot even begin with the setting of the formula—and even if we could begin with it we could never come to an end with it on account of the endlessness of the universe in space. Du Bois Reymond has already mentioned the former endlessness as an insuperable limit; even if we could overstep it, the other would still prove equally insuperable.

If indeed the formula of Laplace comprised only the universe known to our senses, or even one infinitely larger (but not one really endless), and if we could introduce into this formula the forces of the known chemical elements and of the supposed ether particles, or even of much smaller material particles, then it might perhaps suffice for long periods of time backwards and forwards from the present, particularly for the middle of the system and for the greater phenomena. But on the one hand disturbances from the circumference would at once necessarily take place, and these would at last render the formula useless for the middle also; on the other hand, disturbances would begin on each single point as well, and as they would increase constantly, they would at last lead to perceptible inaccuracies, because the supposed "atoms," are not real unities, and because the resulting force, with which each single "atom," as a body composed of separate particles, influences the totality, does not remain a constant one, but with its varying surroundings assumes at every moment an equally varying value. Anyhow, a formula of this kind would give us, as astronomical calculation really does, a solution, correct within certain limits, a practical solution, but not a fundamental one.

The investigator of nature must remember distinctly, that all his investigations are confined by limits in all directions, that on all sides uninvestigable eternity bids him categorically to stop. The fact that this has not always been clearly recognised, that particularly the Infinitely Large and the Infinitely Small have been mixed up with Endlessness and Nothing, has led to several erroneous conceptions. Amongst them are the theories of physical atoms in the one direction, and those of beginning and end of the universe in the other. I will only speak of the latter.

It is supposed that the matter constituting the heavenly bodies was in the beginning distributed in a gaseous state; and in this Du Bois Reymond only finds one difficulty: if this matter, as the theory demands, had been at rest and distributed equally, he cannot find out whence motion and unequal distribution have come.

Condensation of matter has now gone on for an infinite time, *i.e.*, since that supposed beginning, and the results are first nebulæ, then burning-liquid drops, which cool down to dark bodies. In the present we are upon one of these condensed, and no longer incandescent world-drops. According to the natural laws known to us, the still incandescent and the already dark heavenly bodies must continue to give off their store of heat to universal space. By and by they must fall upon one another, and even if then a local rise in temperature again takes place, this after all only serves to accelerate the process of cooling on the whole. At last all heavenly bodies will unite in a dark, solid, icy mass, upon which there is no longer any motion nor life.

This is the result of a correct physical consideration. It shows us the desolate end of a present full of change and motion and glowing with life and colour. But in reality this result is only the consequence of our confined human insight; it would only be a logical necessity if we knew *everything*, and therefore were allowed to use our knowledge for deductions with regard to the beginning and end. But as we see only an infinitesimal part of the universe, and possess only a fragmentary knowledge of the forces and forms of motion in this infinitesimal part, our deductions backwards and forwards may perhaps for certain general conditions be without perceptible error for billions of years, but yet, with the lapse of greater periods of time, they must become more uncertain, and eventually totally erroneous. We may illustrate this particularly well with regard to the past.

What we are most certain of, with regard to the past, is the incandescent state in which our earth was at one period, and from this we draw the conclusion by analogy that the other planets of our system were incandescent bodies as well, just as the sun is still to-day. If we go backwards from these suns we get, by further conclusions, to accumulated masses of clouds, the embryos of the later suns, then to cloud-belts, and eventually to the gaseous mass distributed tolerably uniformly, and this is the original state beyond which, with our present insight, we cannot get.

All this proves distinctly that just as upon the earth an eternal change takes place, the heavens likewise are constantly changing. Each change consists of a sum of motions, and supposes a former change or sum of motions, from which it resulted with mechanical necessity, and further on a chain of changes from all eternity. Thus the gaseous state of our solar system must have been preceded by a continuous endless series of changes, and if our scientific insight does not lead us to this, does not even justify us in this supposition, it thus proves only its own inadequacy.

We must, on the contrary, conclude from the eternity of changes in the universe that the whole process of development of our solar system or of the whole starry heaven, from the original gaseous mass, through the ball-shaped nebulæ, fiery and dark globes, to the cold, solid, and dense mass, is only one of the numberless successive periods, and that analogous periods and occurrences have preceded and will follow endlessly. It is true that we perfectly understand, according to our present physical knowledge, how a mass of gas in a state of progressing condensation produces heat, and how the hot condensed mass again gives off this heat until its temperature and that of its surroundings, in our case that of universal space, have become equal. But we do not understand how the solid mass can again become gaseous, and how the necessary heat, distributed in universal space, can again be collected.

There is a gap in our knowledge at this point; and we may fill it by various suppositions. In the present state of almost complete ignorance among physicists and chemists of the properties of chemical elements and of ether, it is possible that, with sufficient condensation of matter and approach of its particles, forces become active of which we have no idea at present, and which may perhaps bring about an explosive dispersion of the solid mass into a gaseous state. It is also possible that the quantity of heat in the endless universe (not in our starry heaven) is distributed unequally, and that there are domains in it which are of a much higher, and others which are of a much lower, temperature than our starry heaven; that in the endless space of the universe heat currents exist, similar to the air currents in our atmosphere, and that we have perhaps for some billions of years been in one of these currents of lower temperature, in which the process of solidification continues on a large scale, just as on a small scale it occurs on the earth's surface during north winds, and that some hot current which sooner or later may pass through our starry heaven may again bring about a gaseous distribution of matter.

This example shows that we may use our experiences of the finite only for deductions within the finite. As soon as man wishes to overstep this domain, which is opened to him by his senses and which is accessible to his knowledge, and wants to form some conception of the whole, he falls into absurdities. Either he leaves unconsidered what he has gained by experience and meditation, and then he loses himself in arbitrary and empty fancies; or he proceeds logically from the laws fo the finite and then he finally arrives at perfectly ridiculous consequences.

The example mentioned before may again serve to illustrate this. The world known to us changes. If we follow these changes according to the laws of causality, backward into the past and forward into the future, and place ourselves upon the before-mentioned physical stand-point of the nebular theory, and adopt what is known to us there as a measure, then we find stages both in the past and the future which more and more approach perfect rest, without ever reaching it altogether. But if we assume a further point of view, and suppose that heavenly bodies and systems of heavenly bodies arise and perish without end in the universe, then we find two possibilities: either, according to the materialistic conception, the successive stages are of the same value; or, according to the philosophical conception, they continually change their relative value, becoming more perfect every time, in which case the universe would in the eternal past more and more approach absolute imperfection (therefore rest), and in the eternal future absolute perfection (therefore again rest). All three conceptions are equally irrational. The first (physical) and the third (philosophical) let the world awaken from dead rest and return to it. The second (materialistic) condemns it to eternal rest, because a change which always repeats itself, means for an eternity nothing else but rest.

With space we do not fare better than with time. We naturally wish to imagine the universe as of finite extent in space and thus make it accessible to our conception. But as the space filled with matter can but everywhere be limited by more space filled with matter, we arrive at the absurd deduction that the world in its circumference is bordered by itself. But if we allow infinity to universal space, and according to our ideas of space it must be infinite, then heavenly bodies follow upon heavenly bodies without end, in different sizes, different compositions, and different stages of development. Now as size, composition, and stages of development move within finite limits, the combinations which are possible constitute of course, to our ideas, an infinitely great, but yet not an endless number. If this number is exhausted the same combinations must repeat themselves. We cannot deny this, even with the conviction that centillions upon centillions of heavenly bodies or systems of heavenly bodies would not suffice to complete the number of possible combinations. Because centillions compared to endlessness are less than a drop of water compared to the ocean. We therefore arrive at the mathematically correct, but to our reason most absurd, deduction that our earth, just as it is now, must occur several times, indeed an infinite number of times in the universe, and that also the jubilee festival, which we celebrate to-day, is celebrated just in the same way upon many other earths.

The logical consequences of this kind may be multiplied. The examples suffice to show, that our finite reason is only accessible to finite conceptions, and that, when it wishes to raise itself to conceptions of the eternal in however logical a manner, its wings become paralysed, and, like a second Icarus, before the sunny heights are reached, it falls back into the depths of finite and obscure ideas.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Physical Science Postmastership at Merton College, has been awarded to Mr. E. T. Milner, of Manchester Grammar School.

CAMBRIDGE.—Mr. J. N. Langley, B.A., of St. John's College, has been elected a Fellow of Trinity College. Mr. Langley was bracketed second in the first-class of Natural Science Tripos 1874.

EDINBURGH.—Mr. Thomas Annandale, who was assistant to the late Prof. Syme, has been appointed to the chair of Clinical Surgery in Edinburgh University, vacant by the removal of Mr. Lister to King's College, London.

UPSALA.—The *Scotsman* of Thursday last, contains a very full and interesting account of the recent Upsala celebration, evidently by one of the Edinburgh delegates. The writer, speaking of a visit he paid to one of the largest schools of Upsala, built for about 500 pupils, says:—"Here, as elsewhere in Sweden, the expense of education is wholly borne by the State. The pupils pay no fees. The building is spacious and airy, and the class-rooms and playgrounds furnished, almost to luxuriousness, with the requisites for the development of healthy minds in healthy bodies. The arrangements for the securing of the required heating and ventilation of the rooms during the long severe winters of Sweden are particularly good. Nearly every class-room is seated for about thirty pupils. Each pupil has his own little desk before him, and a chair with a back fitting comfortably to his body, and adjustable as to height so as to suit each pupil. This seat he retains during the session, so that there is no taking of places in the classes. There are several carefully-selected libraries for the pupils in the school—a marked feature of which is the number of books in English, French, and German; and there is the best proof everywhere that these volumes, which are mostly classic authors in these languages, do not lie idly on the shelves, in the number of Swedes one meets with who can converse tolerably well in one or all of these languages. But what struck us as deserving of the very warmest commendation, are the well-appointed and well-kept museums of apparatus illustrative of the simplest and most fundamental facts of natural philosophy and chemistry; well-dried mounted specimens of the common plants of the district; stuffed and otherwise prepared specimens of the Swedish fauna; large models of typical plants and animals, showing the details of their structure; and skeletons and plaster casts by which the fundamental facts of anatomy and human physiology can be successfully taught. Thus, with the aid of these admirable elementary museums and appliances, which Mr. Forster might well envy, the broad principles of physical, chemical, and biological science are taught to all whose education goes no further than the public schools; and as regards the others, such instruction in the elements of science forms an admirable introduction to the University course."

SOCIETIES AND ACADEMIES

LONDON

Royal Microscopical Society, October 3.—Mr. H. C. Sorby, president, in the chair.—The president read a paper on an improved method for distinguishing the axes of double refracting substances which consisted of a wedge-shaped piece of quartz cut parallel to the positive axis of the crystal, and made to slide into the eye-piece of the microscope. When this passed across the field of view in polarised light every gradation of tint was successively produced by the varying thickness of the quartz, and by viewing crystals through this it was very easy at once to determine the position of their axes by noting the effect upon the series of coloured bands produced by the quartz scale.—A paper by Mr. F. H. Wenham on the aperture of object glasses was read by the secretary. The purport of Mr. Wenham's paper was further explained, and illustrations of the method proposed were drawn on the black-board by Mr. J. E. Ingpen.—Mr. Slack described some curious observations made as to the habit and power of offensive attack by the genus diglena upon anguillula and other species.

PHILADELPHIA

Academy of Natural Sciences, May 1.—On the Cambari (crayfishes) of Northern Indiana, by W. F. Bundy (*Proc.*, 1877, p. 171).—Synopsis of the fishes of Lake Nicaragua, by Drs. Gill and Bransford (pp. 175-191).—On lavendulite from Chili, by E. Goldsmith.

May 15.—Prof. Leidy, on gregarines.

May 22.—Prof. Leidy, on flukes infesting molluscs.—H. C. Yarrow, notes on the natural history of Fort Macon, N.C. (pp. 203-218).—On the brain of *Chimæra monstrosa*, by Dr. Wilder (pp. 219-250).

June 12.—Prof. Leidy, remarks on parasitic infusoria.

June 26.—Prof. Leidy, the birth of a rhizopod (*Euglypha*).

PARIS

Academy of Sciences, October 8.—M. Peligot in the chair.—On an incident mentioned at the congress of Stuttgart, by M. Faye. This relates to recent geodetic operations in the north-east of Spain, directed by Gen. Ibanez.—Apparatus for measuring the heat of vaporisation of liquids, by M. Berthelot. He aims at greater simplicity, while transmitting the vapour dry from generator to calorimeter. A phial with hermetically sealed neck is traversed by a wide vertical tube open at its inclosed top, passing down through the phial to a serpentine in a calorimeter, and (in its way) through a metallic disc, a circular lamp, another metallic plate, a sheet of paste-board, and a wooden plate (the last three forming the cover of the calorimeter). He finds on an average 636·2 as the total heat furnished by water between 100° and zero (Regnault 636·6).—On the determination of the heat of fusion, by M. Berthelot. The two phenomena of fusion and solidification in a body like hydrate of chloral are not reciprocal when one directly follows the other, and the heat absorbed in one case is not equal to that liberated in the other. To measure the calorific weak infusion the body should be brought to a certain final state, proved identical by thermal measurements, *e.g.*, dissolving hydrate of chloral at a given temperature and in a constant quantity of water, and comparing specimens recently fused, and others kept several months or years. Then a known weight of the substance is raised to different temperatures, sometimes above sometimes below the point of fusion, then immersed and dissolved suddenly in the water of the calorimeter. He finds the heat of fusion to be 33·2 cal. for 1 gramme.—On the variations of the heat liberated by union of water and sulphuric acid at different temperatures, by M. Berthelot.—On the relation which should exist between the diameter of magnetic cores of electro-magnets and their length, by M. Du Moncel. For equal resistances of circuit the diameters should be proportional to the electromotive force for equal electromotive forces, in inverse ratio of the resistance of the circuit, including the battery resistance; for equal diameters proportional to the square roots of the resistances of the circuits; from given electromotive force and with electro-magnets in their conditions of maximum the electro-motive forces of the batteries should be proportional to the square roots of the resistances of the circuits.—Programme of the expedition of next year (July, 1878) to the glacial sea of Siberia, by M. Nordenskjöld.—Observations of the planet 175 Palisa, and of the new comet of Tempel, with the garden equatorial, by MM. Paul and Prosper Henry.—On a general method of transformation of integrals depending on square roots; application to a fundamental problem of geodesy, by M. Callandreau.—On the spectrum of the new metal davyum, by M. Kern. He indicates the principal lines.—Pyrogenous decomposition of chlorhydrate, bromhydrate, and iodhydrate of trimethylamine, new characteristic of methylamines, by M. Vincent. The new characteristic is the production of chloride, bromide, and iodide of methyl from such decomposition.—On iodide of starch, by M. Bondonneau.—Synthesis of benzoic acid and of benzophenone, by MM. Friedel, Crafts, and Ador.—Experiments on the tape-like development of human cysticercus, by M. Redon. Man may, like swine, become completely infested by cysticerci. M. Redon caused some cysts from a human body to be ingested with tepid milk into young pigs and dogs; he also swallowed some himself. It appeared that only man presents a favourable medium; the pigs and dogs gave no trace of the tape-worms; but the author after about three months discovered worms in his stools. This throws light on the nature of the development of human cysticercus, and presents a striking exception to the law of parasitism with alternating generations.—Description of the meteoric stones of Rochester, Warrenton, and Cynthiana which fell respectively on December 21, 1876, and January 3 and 23, 1877, with some remarks on previous falls of meteorites in the same region, by Mr. Smith.—M. Bouvel called attention to an arrangement for compressing oxygen and hydrogen to considerable pressures. The wires from a battery are conducted into a thick metallic block containing a strong glass voltameter with one chamber double the other; under the chamber are the terminal electrode immersed in acidulated water, the bottom of the reservoirs being closed by a strong screw. The reservoir communicates also with another cylinder in which a screw can be made to press on the liquid. Two narrow passages rise from the gas chamber and are closed by screws.

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ERRATUM.—In NATURE, vol xvi p. 330, the reference to the *Astron. Nach.* should be to Nos. 1,663 and 1,733.

THURSDAY, OCTOBER 25, 1877

SCIENTIFIC WORTHIES

XI.—SIR JOSEPH DALTON HOOKER

SIR JOSEPH DALTON HOOKER,[1] Director of the Royal Gardens, Kew, and President of the Royal Society—the second and only surviving son of the late Sir William Jackson Hooker, who first made the name illustrious in botany—was born at Halesworth, in Suffolk, on June 30, 1817. He was educated at the University of Glasgow, where the father was Regius Professor of Botany from 1820 to 1840, and where the son took the degree of M.D. in 1839. He has since been affiliated to the great English Universities, receiving in the same year the honorary degrees of D.C.L. at Oxford, and of LL.D. at Cambridge, and subsequently that of LL.D. at his own University of Glasgow. Immediately on completing his medical studies, namely in 1839, he was commissioned Assistant-Surgeon in the Royal Navy, and appointed botanist of the antarctic voyage of exploration by the *Erebus* and *Terror*, under the command of Captain, afterwards Sir James Clark Ross. This celebrated expedition, leaving England in the autumn of 1839, and touching at Madeira, Teneriffe, Cape Verde Islands, St. Helena, and the Cape of Good Hope, entered, in the spring of 1840, upon its special work of antarctic exploration, which, including visits to Kerguelen Island, New Zealand, Australia, Fuegia, and the Falkland Islands, occupied the ensuing three years, years of severe labour and much hardship, but of opportunities such as probably never before fell to the lot of a young naturalist, or were ever turned to better account.

The botanical fruits of this expedition are mainly garnered in the "Flora Antarctica," the "Flora Novæ Zelandiæ," and the "Flora Tasmanica," six quarto volumes, the last of which appeared in the year 1860. These do not contain mere reports of explorations, with descriptions of whatever was novel or of peculiar interest, but are systematically elaborated and complete floras, in which all that had been gathered from every source is incorporated. The excellent analyses of the plants, and in particular those of the cryptogams, are for the most part from the author's own drawings, many of them made during the progress of the voyage.

During the preparation of the first of this series of floras, Sir J. D. (then Dr.) Hooker, being then attached to the Geological Survey of Great Britain, brought out several important papers upon points in fossil botany. They need not be here particularly enumerated, but they attracted much attention, and evinced remarkable aptitude for dealing with a difficult class of questions.

In the interval between the publication of the second volume of the "Flora Antarctica," in 1847, and the first volume of the "Flora Novæ Zealandiæ," in 1853, Sir J. D. Hooker, aided by Government, but mainly on his private resources, accomplished his botanical mission to India. A general account of this undertaking is given in his "Himalayan Journals; or, Notes of a Naturalist in Bengal, the Sikkim and Nepal Himalayas, the Khasia Mountains," &c., in two volumes, 8vo., 1854.

His journey occupied three and a half years, and was not without hardship and adventure. After spending two seasons in exploring geographically and botanically the loftiest Himalayan mountains and valleys, unaccompanied by any European, he was, when on the Tibetan frontier, joined by his friend, Dr. Campbell, the political agent resident in a neighbouring British province; this led to the capture of both by the Rajah of Sikkim, and they were imprisoned for some weeks, during which time they were treated with great indignity and their lives were threatened. During his journey in Sikkim he made a survey of the whole country and the bordering districts of Nepal, from the plains of India to Tibet; this was published by the Trigonometrical Survey Office of Calcutta, and is still the standard map of the country. It is a curious fact that though nearly thirty years have elapsed since Sir J. D. Hooker was in the country, many of the trans-Himalayan passes which he then discovered and measured have not since been visited by any other traveller.

Of the scientific results, the first fruits were given to the world in various papers communicated to the Asiatic Society of Bengal; these were followed by the "Rhododendrons of the Sikkim Himalayas," a folio, with splendid illustrations from the author's own pencil; and in a volume of the "Flora Indica," by himself and his friend and schoolmate, Dr. Thomas Thomson. A series of papers upon certain groups, or discussing special points, followed later; and at length the systematic elaboration of Indian botany has been hopefully renewed, and the "Flora of British India," upon the model of the "British Colonial Floras," has reached the second volume. Although "assisted by other botanists," some of the ablest of these died before their contributions were completed, and a large part of the labour and responsibility has devolved upon Sir J. D. Hooker.

In 1869 Sir J. D. Hooker held the presidency of the British Association at Norwich. In his address he advocated the recognition of the Darwinian hypothesis as the best means of advancing the study of the natural sciences, and dwelt at length on the subject of provincial museums, showing by what means they can be rendered both instructive to the general public and be adapted to the needs of scientific men.

On an earlier occasion, at the meeting of the Association at Nottingham in 1866, he was selected to give one of the two public lectures that form part of the programme at these annual meetings. Choosing "Insular Floras" for his subject he gave the results of his own wide personal experience in a discourse which attracted much attention on account of the soundness of the views he advocated and the originality of the illustrations by which he supported them.

Of special memoirs, such as test a botanist's capabilities, four of Sir J. D. Hooker's are particularly noteworthy. These are, in the order of publication; first, the essay "On the Structure and Affinities of Balanophoreæ," a peculiar and puzzling group of phænogamous root-parasites, here for the first time well investigated upon sufficient material, admirably illustrated, and their affinities acutely discerned. The second is a shorter paper "On the Origin and Development of the Pitchers of Nepenthes," prefacing an account, with striking illustrations, of some new Bornean species, and bringing out the conclusion that the pitcher is a modification of a gland at the apex of the midrib of a leaf. The functions and mode of action of these pitchers became the subject of a later investigation and the theme of his address to the Section of Zoology and Botany, over which he presided, at the meeting of the British Association at Belfast, in 1874. This forms one of the earlier contributions to our new knowledge of carnivorous plants. Thirdly, the "Outlines of the Distribution of Arctic Plants," cognate with which is the elaborate "Introductory Essay to the Flora Tasmanica," published earlier in the same year, 1860. These two papers embody the results of long and wide study of the geographical distribution, systematic association, and various degrees of relationship of existing species, in regard to their probable history and origin. Having been prepared before Mr. Darwin's "Origin of Species" appeared, they are among the earliest and most notable contributions to this part of our science. They are endeavours to test the practical value in systematic botany of now familiar theoretical considerations or hypotheses, the influence of which was felt and the importance dimly divined, in advance of their full development by Mr. Darwin. Fourthly, a rare opportunity was well improved when that most extraordinary of plants, *Welwitschia mirabilis*, of Western Tropical Africa, was placed in Sir J. D. Hooker's hands for study. Later investigations of completer materials may have since cleared up points which were left doubtful, and may have definitely answered questions which were placed on the way to settlement by being suggestively raised. Still the splendid memoir on "Welwitschia, a New Genus of Gnetaceæ," stands unrivalled among botanical monographs of the kind for perfection of illustration, elucidation of structure, and insight into affinities.

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[1] Since these lines were written her Majesty has signified her appreciation of Dr. Hooker's services, especially those in relation to the Indian Empire, by conferring upon him the honour of Knight Commander of the Star of India.—A. G.

Turning next to labours which came in great part by inheritance, we need only refer to the thirteen volumes of the *Botanical Magazine* which follow the thirty-seven edited by Sir William Hooker, and to the two volumes in continuation of the less popular, but botanically important, "Icones Plantarum." Then, after the exhaustion of Sir William Hooker's "British Flora" in its eighth edition, Sir J. D. Hooker replaced it by his own compendious "Student's Flora of the British Islands, 1870," which is now passing to a revised edition. Those who have made the attempt well know how the conscientious preparation of such a work tasks the best powers of a botanist. Upon the presentation, in an English dress, of Le Maout and Decaisne's "Traité Général de Botanique," in a translation by the late Mrs. Hooker, the orders were re-arranged and annotated by Sir J. D. Hooker, and a chapter on the principles of classification and a synopsis of natural groups added. Then, least in size, but not in usefulness nor in difficulty of execution, comes the "Primer of Botany," for the use of young beginners. As far as possible, elementary treatises should be written by masters in science, and Sir J. D. Hooker has contributed his fair share.

We come, at length, to the "Genera Plantarum ad examplaria imprimis in Herbariis Kewensibus servata definita." In this, one of the most arduous and, as we judge, most important botanical works of our time, Sir J. D. Hooker is associated with the veteran Bentham, who has the enviable advantage of being able to devote all his time to botanical investigation, undistracted by professorial or administrative cares, and who brings to the work the largest experience, the surest judgment, and the most indomitable industry—gifts and accomplishments rarely thus associated. As two of the three compact volumes of the "Genera Plantarum" are already published, and the third is in progress, let us hope that we may all ere long see and rejoice together over the completion of a work which marks an epoch in systematic botany. Compilations and digests we may have, as we have had; and supplements and new editions of the present work may naturally and easily be provided; but, as its only real predecessors are the "Genera Plantarum" of Linnæus (1737–64) and of Jussieu (1789)—to which we may add that of Endlicher (1836–40) the latter a wonderful monument of literary labour and bibliographical ability, directed by a fair amount of botanical knowledge—so we may expect that a long generation will pass before an undertaking like the present will be again attempted and carried through.

If this cursory reference to the publications of one who is still, as we fondly hope, only in mid-career, were extended into details and specifications, it would still be far from giving a full idea of the extent and value of Sir J. D. Hooker's scientific services. While his colleague, to whom reference has been made, supplies a notable instance of what may be accomplished by one who (without declining a reasonable share of public duty) has been mainly free from engrossing administrative cares, the life of Sir J. D. Hooker, like that of his father, has fallen in the common lot of scientific men. Or rather, in both the Hookers, unusual gifts and energies have entailed more than ordinary cares and responsibilities. To develop and to sustain and extend such noble and invaluable establishments as those at Kew Gardens was a duty not to be declined, however engrossing. It has been performed in such wise as to win, along with national applause, the gratitude of the scientific world. Throughout his travels and voyages his energies were directed by his father to the advancement of Kew as a centre of scientific botany, and as a means of transmitting to all parts of the world plants useful to mankind. In 1855 he was appointed Assistant-Director, and since the death of his venerated father, in 1865, the burden of maintaining the Gardens at the high condition they had attained through the father's exertions has fallen on the son.

Botanists all over the world count this devotion to Kew Gardens high among Sir J. D. Hooker's scientific services. They admired and cheered the courageous and indomitable spirit with which he resisted and thwarted the attempt of a whilom official guardian to lower the character and diminish the scientific value of this most useful establishment. They rejoice, likewise, to see the presidential chair at the Royal Society occupied for the second time by a botanist and explorer. They concede the paramount claims of public duty, yet not without a shade of jealousy and regret; for administration is time-consuming and endless, while Hookers and their like are few, and botanical work on every side is pressing.

A critical exposition and estimate of the work which Sir

J. D. Hooker has already accomplished, and by which his high scientific position has been earned, must needs be either too technical or too long for a sketch like this. Moreover, the Atlantic is no longer what it once was, when a judgment wafted across it either way was invested with somewhat of the character of the verdict of posterity. And the close relations for forty years of the present writer with the Hookers, father and son, disqualify him for the office of judge. Let that duty devolve upon our successors.

The knowledge and experience of most of our eminent botanists have been gained, and their work mainly done, in the herbarium and botanic garden. No living botanist that we know of has shared Sir J. D. Hooker's opportunities of studying in place the living vegetation of so many parts of the world; these include, besides those already mentioned as places visited during his Antarctic voyage, the southern shores of Europe, North Africa, Palestine, and India to the Chinese borders. When we have welcomed him to New England, as we hope to do before these lines are in print, and when he has traversed our continent from the Atlantic to the Pacific, it may confidently be affirmed that he has seen far more *βοτάνη* than ever fell to the lot of any other of his craft.

Sir J. D. Hooker was elected President of the Royal Society in 1873, an office which he still holds.

May, 1877 ASA GRAY

NOTES ON THE BOTANY OF THE ROCKY MOUNTAINS

[The contemplated visit to the United States of America alluded to in the preceding article has now been accomplished, and Sir Joseph Hooker has favoured us with the notes of his journey following.]

IN company with Dr. Asa Gray, Professor of Botany of Harvard University, Cambridge, U.S., I availed myself of an oft-repeated invitation to us both from Dr. Hayden, the distinguished chief of the Topographical and Geological Survey of the United States Territories, to join the Survey in Colorado and Utah; this we did with the view of instituting a comparison between the floras of these central and elevated territories and those of other parts of the continent, and thus obtaining some insight into the origin and distribution of the North American flora. In order to comprehend the importance of Colorado and Utah as the basis for such investigations, I should state that they occupy a very central position in the continent, and include a section of the Rocky Mountains about 300 miles long and about as broad, namely, from N. lat. 37° to 41°, and from W. long. 105° to 112°.

The mountain region thus limited consists of extensive and often level floored valleys, sometimes many miles broad, and elevated 4,000 to 5,000 feet above the sea, called "parks" in local topography, which are interposed between innumerable rocky mountain ridges of very various geological age and formation, which often reach 12,000 feet, and sometimes 14,000 feet elevation, the maximum being under 14,500.

Those of the so-called parks which are watered by rivers that flow to the east are continuous with the prairies that lie along the eastern flanks of the Rocky Mountains; those watered by rivers that flow to the west are continuous with the so-called desert or salt regions that lie along the western flanks of the range; but the divides between the head waters of the streams that flow either way are often low, and the botanical features of the east and west may hence meet and mix in one park.

Such a section of the Rocky Mountains must hence contain representatives of three very distinct American floras, each characteristic of immense areas of the continent. There are two temperate and two cold or mountain floras, viz.: (1) a prairie flora derived from the eastward; (2) a so-called desert and saline flora derived from the west; (3) a sub-alpine; and (4) an alpine flora; the two latter of widely different origin, and in one sense proper to the Rocky Mountain ranges.

The principal American regions with which the comparison will have first to be instituted are four. Two of these are in a broad sense humid; one, that of the Atlantic coast, and which extends thence west to the Mississippi river, including the forested shores of that river's western affluents; the other that of the Pacific side, from the Sierra Nevada to the western ocean: and two inland, that of the northern part of the continent extending to the Polar regions, and that of the southern part extending through New Mexico to the Cordillera of Mexico proper.

The first and second (Atlantic plus Mississippi and the Pacific) regions are traversed by meridional chains of mountains approximately parallel to the Rocky Mountains; namely, on the Atlantic side by the various systems often included under the general term Appalachian, which extend from Maine to Georgia, and on the Pacific side by the Sierra Nevada, which bounds California on the east. The third and fourth of the regions present a continuation of the Rocky Mountains of Colorado and Utah, flanked for a certain distance by an eastern prairie flora extending from the British possessions to Texas, and a western desert or saline flora, extending from the Snake River to Arizona and Mexico. Thus the Colorado and Utah floras might be expected to contain representatives of all the various vegetations of North America except the small tropical region of Florida, which is confined to the extreme south-east of the Continent.

The most singular botanical feature of North America is unquestionably the marked contrast between its two humid floras, namely, those of the Atlantic plus Mississippi, and the Pacific one; this has been ably illustrated and discussed by Dr. Gray in various communications to the American Academy of Sciences, and elsewhere, and he has further largely traced the peculiarities of each to their source, thus laying the foundations for all future researches into the botanical geography of North America; but the relations of the dry intermediate region either to these or to the floras of other countries had not been similarly treated, and this we hope that we have now materials for discussing.

Our course and direction in America was directly westward to Colorado, where we followed the eastern flanks of the Rocky Mountains for about 300 miles, that is from Denver in the north, to near the borders of New Mexico, ascending the highest northern and southern peaks, and visiting several intermediate parks and valleys, watered by tributaries of the Arkansas, Platte, Colorado, and Rio Grande. From Denver we proceeded north to Cheyenne in Wyoming, and thence westward by the Central Pacific Railway, across the range to Ogden, and the Great Salt Lake in Utah, which lies on the base of the Wahsatch Mountains, themselves the western escarpment of the Rocky Mountains proper in that latitude. After ascending these we proceeded westward by rail through Utah, to Nevada, thus crossing the great dry region that intervenes between the Rocky Mountains and the Sierra Nevada, which is variously known as the Desert, Salt, or Sink region of North America, in accordance with the prevailing features of its several parts. It is elevated 3,000 to 4,000 feet, and traversed by numerous short meridional mountain-ridges, often reaching 8,000 feet, and rarely 10,000 feet elevation; unlike the Rocky Mountains or over the Sierra Nevada, these present no forest-clad slopes, or even a sub-Alpine flora.

From Reno, at the western base of the Sierra Nevada, we proceeded south by Carson City, flanking the Sierra for some sixty miles to Silver Mountain, when we struck westwards, ascending the Sierra, which was crossed obliquely into the Pacific slope. There we visited three groves of the 'Big Trees' (*Sequoia gigantca*) at the headwaters of Stanislaus and Tuolome Rivers, and the singular Yosemite Valley, whence we descended into the great valley of California, and made for San Francisco.

From the latter place we made excursions first to the old Spanish settlement of Monterey, which is classical ground for the botanist, as being the scene of Menzies' labours during the voyage of our countryman, Capt. Vancouver, in 1798 (whose surveys are held in the highest estimation by Prof. Davidson and the officers of the Coast Survey of the United States), whom he accompanied as botanist. Then we went northwards along the coast range to Russian River to visit the forests of Red-wood (*Sequoia sempervirens*), the only living congener of the Big Trees, and almost their rival in bulk and stature. Then to Sacramento, and up the valley of that name for 150 miles to Mount Shasta, a noble forest-clad volcanic cone about 14,400 feet in elevation. Returning thence to Sacramento we took the Union Pacific Railway eastwards, and from the highest station visited Mount Stanford, on the crest of the Sierra Nevada, and Lake Taho, which occupies a basin in the mountains at about 7,000 feet elevation, and with which we finished our western journeyings.

In California the Coniferæ were a principal study, with a view of unravelling their tangled synonymy and tracing the variations and distribution of these ill-understood trees, which attain their maximum development in number of species and in stature on the Pacific slope of the American continent.

The net result of our joint investigation and of Dr. Gray's previous intimate knowledge of the elements of the American flora is, that the vegetation of the middle latitudes of the continent resolves itself into three principal meridional floras, incomparably more diverse than those presented by any similar meridians in the old world, being, in fact, as far as the trees, shrubs, and many genera of herbaceous plants are concerned, absolutely distinct. These are the two humid and the dry ntermediate regions above indicated.

Each of these, again, is subdivisible into three, as follows:—

1. The Atlantic slope plus Mississippi region, subdivisible into (*α*) an Atlantic, (*β*) a Mississippi valley, and (*γ*) an interposed mountain region with a temperate and sub-alpine flora.

2. The Pacific slope, subdivisible into (*α*) a very humid cool forest-clad coast range; (*β*) the great hot, drier Californian valley formed by the San Juan river flowing to the north, and the Sacramento river flowing to the south, both into the Bay of San Francisco; and (*γ*) the Sierra Nevada flora, temperate, sub-alpine, and alpine.

3. The Rocky Mountain region (in its widest sense extending from the Mississippi beyond its forest region to the Sierra Nevada), subdivisible into (*α*) a prairie flora; (*β*) a desert or saline flora; (*γ*) a Rocky Mountain proper flora, temperate, sub-alpine, and alpine.

As above stated, the difference between the floras of the first and second of these regions, is specifically, and to a great extent generically absolute; not a pine or oak, maple, elm, plane, or birch of Eastern America extends to Western, and genera of thirty to fifty species are confined to each. The Rocky Mountain region again, though abundantly distinct from both, has a few elements of the eastern region and still more of the western.

Many interesting facts connected with the origin and distribution of American plants and the introduction of various types into the three regions, presented themselves to our observation or our minds during our wanderings; many of these are suggestive of comparative study with the admirable results of Heer's and Lesquereux's investigations into the pliocene and miocene plants of the north temperate and frigid zones, and which had already engaged Dr. Gray's attention, as may be found in his various publications. No less interesting are the traces of the influence of a glacial and a warmer period in directing the course of migration of Arctic forms southward, and Mexican forms northward in the continent, and of the effects of the great body of water that occupied the whole saline region during (as it would appear) a glacial period.

Lastly, curious information was obtained respecting the ages of not only the big trees of California, but of equally aged pines and junipers, which are proofs of that duration of existing conditions of climate for which evidence has hitherto been sought rather amongst fossil than amongst living organisms.

I need hardly add that the part I played in the above sketched journey was wholly subordinate to Dr. Gray's, who had previously visited both the Rocky Mountains and California, though not with the same object. But for his unflinching determination that nothing should escape my notice which his knowledge and observant powers could supply, and Dr. Hayden's active co-operation, my own labours would have been of little avail.

Moreover, throughout the expedition we experienced great hospitality, and enjoyed unusual facilities, not only from the staff of the Geological Survey, but from the railway authorities, who franked us across the continent, and on all the branch lines which we traversed.

J. D. Hooker

SHARPE'S CATALOGUE OF BIRDS

Catalogue of the Birds in the British Museum. Vol. I., Catalogue of the Accipitres, or Diurnal Birds of Prey. By R. Bowdler Sharpe. 1874.—Vol. II., Catalogue of the Striges, or Nocturnal Birds of Prey. By the same Author. 1875.—Vol. III., Catalogue of the Passeriformes, or Perching Birds. By the same Author. 1877. (London: Printed by order of the Trustees.)

IF the visitor to the British Museum will pause at the foot of the staircase leading up to the Paleontological Gallery and look carefully into the obscurity in the right-hand corner he will perceive a door with a brass plate on one side of it. On entering this door and descending (with care) a flight of darkened steps, he will find himself in the cellar, which has for many years constituted the workshop of our national zoologists. Two small studies partitioned off to the left are assigned to the keeper of the department and his first assistant. The remaining naturalists are herded together in one apartment commonly called the "Insect-room," along with artists, messengers, and servants. Into this room is shown everybody who has business in the Zoological Department of the British Museum, whether he comes as a student to examine the collections, or as a tradesman to settle an account. Amid the perpetual interruptions thus caused, our national zoologist has to pursue his work. Some of the specimens are here, some in the galleries overhead, and some are stored away in cellars at a still lower depth than that in which he sits at work. The library attached to the department contains merely some of the most obvious books of reference, all others have to be obtained on loan from the great national depository of books in the centre of the building. No lights are allowed, and when the fogs of winter set in, the obscurity is such that it is difficult to see any object requiring minute examination.

Under these circumstances, which we trust to see materially altered when the zoological collections are removed to their new home in South Kensington, it is more than creditable to our zoologists that they should have turned out the large amount of scientific work that has issued from their department of the British Museum during the past thirty years. The zoological catalogues of the British Museum are well-known to every worker in natural history; they are not mere catalogues, but in many cases able and exhaustive monographs of the groups of which they treat. Projected and commenced by the late Dr. Gray they have been energetically carried on under the rule of the present head of the zoological department. Dr. Günther is, moreover, himself the author of one of the most important of the series "the Catalogue of Fishes," completed in 1870, in eight volumes, which is now the standard work of reference in icthyology.

Hardly less important as regards the sister science of Ornithology, if brought to so successful a conclusion, will be the "Catalogue of Birds," of which three volumes are now before us. With youth and energy on his side Mr. Sharpe may look forward to do much, but it must be confessed that if he intends to handle the whole subject himself he has an arduous task before him. The number of recognised species of the class Aves cannot now be reckoned at less than from ten to twelve thousand. There are many more workers in ornithology than in icthyology, the literature is still more widely scattered in different magazines and periodicals, and the collections to be consulted, both public and private, are much more numerous. There is also no generally recognised system of classification to follow—such as that of the illustrious Johannes Müller—our great master in the classification of fishes. All this makes the work of a general descriptive catalogue of birds one of almost herculean labour, which only length of time and great devotion to the subject can hope to accomplish.

Let us now see what progress Mr. Sharpe has already made towards the completion of the task. In 1874 he issued the first volume of the catalogue containing the Accipitres, or diurnal birds of prey; in 1875 the second, in which the Striges, or nocturnal birds of prey, were treated of. We have now the third volume before us, in which the great order usually called "Passeres," but here denominated "Passeriformes," is commenced.

Taking this as an average volume, we find about 350 species comprised in it. Unless, therefore, the present rate of progress is materially accelerated, it does not require much calculation to show that forty or fifty years must elapse before a single worker can complete the task. Even if the volumes were henceforth published annually, and 500 species on the average comprised in each volume, upwards of twenty years would be necessary to bring the work to a conclusion, and looking to the present rapid advances being made in our knowledge of birds, the older volumes would be out of date long before the last were ready for publication. But if the present style of work is adhered to, it would seem that our last supposition is one that is hardly likely to be accomplished.

The question remains, whether any alterations can be suggested that will reduce the task to one of more reasonable proportions. It must be recollected that Mr. Sharpe's "Catalogue" is not merely a catalogue of specimens in the National Museum, but approaches in several respects more nearly to a monographic essay on each group treated of. Taking, for example, the present volume, which contains the Coliomorphæ, or Crow-like Passeres, we find, on page 4, a "synopsis" of the five families into which the Coliomorphæ are divided, next a definition of the two sub-families of the Corvidæ, the first of these families. Then follows a "key" to the thirty-nine genera of Corvidæ; these genera are taken in order, and a "key" to the species is placed at the head of each genus Under the head of each species the synonymy is very fully given; then a lengthened description of each species in its various plumages, a general account of its "habitat," and finally a list of specimens of it in the British Museum, with a notice of the locality and mode of origin of each specimen. To prepare all this it is manifest that an elaborate study of each group must be undertaken, and such a study cannot be executed without much time and attention. The result will be that we shall have, when the task is brought to a conclusion, a general work, which must become the standard book of reference for all naturalists who are engaged on the class of birds.

While the plan of work pursued by Mr. Sharpe has our full approbation as a whole, there are several points of detail in which we think improvements might be made.

The descriptions of the species are, especially in the first volume, by far too long. Short diagnoses would be of much greater assistance to naturalists using the work for the determination of species. It may be said, no doubt, that such diagnoses *are* given in the tabular keys of the species under the head of each genus. This is true to a certain extent; at the same time these keys only refer to *one* point of difference between allied species, and if a student misses the particular key selected by Mr. Sharpe he will find it very difficult to open the lock. If instead of the lengthened description, a short diagnosis were given and remarks on the points of difference between the species in question and its nearest allies were appended, the result would be at least of equal, if not of greater value, to the working ornithologist, and at the same time the work would be materially shortened. Nothing serves so clearly to explain the character of an unknown object as reference to an object well known and a specification of the points of difference. Such particulars, now universally added by naturalists to their characters of species, are rarely to be found in Mr. Sharpe's catalogue.

One other point we will mention—that we trust Mr. Sharpe will consider seriously in the preparation of future volumes. The golden rule of priority and the canons of the Stricklandian Code of Nomenclature are now generally accepted by all zoologists. But like many other things that are excellent, "priority" may be pushed to a ridiculous extent, and some writers seem determined to disgust people with it if they can. The chief object of the rule of priority is the attainment of a uniform nomenclature. Unless, therefore, there is a stringent necessity in obedience to its rule to alter a generally-recognised name, it is only defeating the object in view to propose such a change. But we regret to observe that Mr. Sharpe not unfrequently strains the laws of priority in order to alter well-known and universally adopted titles, both of genera and species. For example, Linnæus called the well-known kestrel *Falco tinnunculus*, *i.e.*, the "Bell Falcon." In 1807 the French naturalist, Vieillot, made the group of kestrels into a genus, for which he proposed to use the name *Tinnunculus*—and this practice has been generally followed by all ornithologists who have considered the group of kestrels as worthy of generic rank. But Mr. Sharpe now wishes to reject the generic name *Tinnunculus* in favour of a subsequently-given appellation, upon the ground, we suppose, that the species first given in Vieillot's list of *Tinnunculi* is not a true kestrel. This may be the case, but it cannot be doubted that when Vieillot founded his genus, *Tinnunculus*, the species most in his eye was the *Falco tinnunculus*, and that that species should be taken as the true type of his genus.[1] Again, a well-known South American bird of prey is universally known as *Spizaëtes ornatus*. Mr. Sharpe would have it termed *Spizaetus mauduyti*, solely because, although both the names were published in the same work at the same date, the latter is given four pages before the former. We cannot believe that even the great authority of the British Museum will induce naturalists to recognise such grounds for the displacement of familiar names.

These, however, are but minor defects in a work that is generally well arranged and well executed. Further than this, we have only to remark that Mr. Sharpe might have done better to adopt some one general classification in its entirety rather than to attempt to amalgamate several hardly-to-be-reconciled systems into one of his own. But whatever his classification may result in, there can be no question of the influence the "Catalogue of Birds" will have on the progress of ornithological science, and we heartily wish the author health and strength to terminate his labours at an earlier period than that which we have assigned to them.

THE ALPS

Die Naturkräfte in der Alpen, oder physikalische Geographie des Alpengebirges, von Dr. Friedrich Pfaff, O. Professor in der Universität, Erlangen. (München: Oldenbourg, 1877.)

THIS is a thoroughly unsatisfactory book. The title is attractive, and in spite of all that has been written about the Alps of late years, a treatise such as is here promised is very much wanted. Such a work if taken in hand by a master of physical science capable of grasping together the varied phenomena and exhibiting vividly their mutual bearings and relations, would be of engrossing interest, and could scarcely fail to throw new light upon many obscure questions of science. Failing this, there is room for a work in which the results of recent exploration and scientific observation should be carefully collected, intelligently arranged, and clearly set forth. Such a book might not attract many of those who have no personal experience of the Alps, but would be welcomed by thousands who have keen recollections of enjoyment among the great mountains, and would fain learn something of the nature and laws of the giant forces within whose sphere they have moved. Along with the primary, though no way common, qualifications of accuracy and clearness, the writer of such a work should have such a firm hold of physical principles as should enable him to mark distinctly the limits of the territory conquered by modern science, and distinguish the conclusions which are definitely established from those that are more or less imperfectly proved, or merely to be ranked as conjectural explanations.

In none of these respects does Prof. Pfaff show himself competent for the work he has undertaken. Except in regard to glacier controversies, where his reading is a little more extended, he appears very ill acquainted with what has been written about the Alps in England, Italy, and France during the last quarter of a century; he is strangely inaccurate, especially as to names and numbers, and often sadly deficient in clearness; and finally, there is a complete want of definiteness in his statements as to the more interesting of the disputed questions discussed in his work.

A disproportionate share, more than a third of the volume, is taken up with what may be called the statistics of the Alps—their division into groups, numerical statements as to the heights of peaks, the dimensions of valleys and lakes, the volume of mountain ridges, and various other orographic elements, some of which do not admit of accurate numerical statement, and others are of trivial importance. It is not too much to say that when the author is unable to copy from a clear-headed and accurate writer, such as von Sonklar, this part of his work is frequently a mere muddle.

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[1] Mr. Sharpe gives, as the name to be adopted for the Common Kestrel, *Cerchneis tinnuncula*—apparently under the idea that *Tinnunculus* is an adjective.

In attempting to subdivide the Alps into separate groups it is possible to apply one or other of two guiding principles. You may look either to the configuration of the surface and make the deep valleys and low passes that occur here and there throughout the chain the boundaries between the different groups, or you may attend mainly to geological structure, and form groups in each of which a central mass of crystalline rock is surrounded by a girdle of sedimentary strata, but in so doing must often disregard the actual features of the country. M. Pfaff has alternately adopted the geological grouping of Studer and the orographic arrangement of von Sonklar, with the natural result that the same mountains and valleys are in some cases included in two different groups while others are left utterly unprovided for, there being no one group in which they can be placed. For some groups the author has attempted to assign limits and specify the higher summits, but considering the ill success of these attempts, he has, perhaps wisely, refrained in other cases, and for eleven groups he has given names without attempting to define their limits. Among the dominant peaks we find the two ancient impostors Mont Ollan and Mont Iséran, whose existence has long since been disproved by the active members of our Alpine Club, and, stranger still, we are told to look for the latter in an utterly new direction—in the range west of the Col de Bonhomme "between the Isère and the Rhone"—where the discovery of a mountain over 13,000 feet high would undoubtedly make a lively sensation among the natives. A list of the most important peaks in the Alps is given, in which are enumerated fourteen summits in the mass of Mont Blanc, and twelve of those of Monte Rosa, while the crowning peaks of other important groups such as the Grand Paradis and Piz Bernina, each well above 13,000 feet, are altogether omitted. Heights copied at random from various authorities are hopelessly irreconcilable. In one page the Gross Glockner is 495 English feet higher than the Ortler Spitz; a few pages later the tables are turned, and the Ortler surpasses his rival by 285 feet. What with errors in the spelling of names, confusion of standards—Paris feet being quoted in one line, Vienna feet in the next, and in the third some other foot differing from both—and arithmetical blunders or misprints, this portion of the book is simply bewildering.

Under the head of meteorology the author discusses at much length the laws connecting the decrease of temperature of the air with increasing elevation above the earth's surface. He gives the now-antiquated formula of S. von Waltershausen as in accordance with observation, and then professes to give the results of what, with characteristic accuracy, he describes as the "observations of Glaisher and Coxwell, who, on September 5 1862, in London, rose to the astonishing height of 26,800 feet" (28,565 English feet). The reader who may turn to the report of Messrs. Glaisher and Coxwell's famous ascent from Wolverhampton will fail to find anything in the least resembling the results here given, these being in truth very incorrectly calculated from the average results of all the ascents made in 1862. No reference is made to the bearing of these or other recent investigations on a matter so interesting to Alpine travellers as the measurement of heights by means of the barometer, nor does Prof. Pfaff seem to be acquainted with the various memoirs by Count St. Robert, of Turin, wherein the whole subject is discussed in a masterly manner.

Fully one-third of the volume is devoted to the glaciers wherever it would appear that the writer has made observations on his own account. In this branch of his subject he is moderately well informed, and no doubt has done his best to steer cautiously through the rocks and shoals of personal controversy with which the history of this department of scientific inquiry is unfortunately surrounded, while at the same time he fails to mark accurately the positive contributions of each inquirer to the present sum of our knowledge.

He gravely discusses the dilatation theory of glacier motion, and comes to the conclusion that "dilatation cannot be considered the only cause of the progressive motion of glaciers," and soon after remarks that the gravitation theory has now a majority of adherents; while it would be difficult to name a single competent authority who during the last twenty years has admitted that dilatation has any share whatever in producing the phenomena. He has doubtless read the writings of Agassiz, and Forbes, and Tyndall, but he shows himself unable to grasp the full force of the reasoning of the two latter writers, and in more than one instance has failed to understand them. With regard to the vexed question of the origin of the veined structure of glacier ice, Prof. Pfaff is especially unsatisfactory. He attributes the first notice of it to M. Guyot, though many previous travellers had like him observed it, but failed to discern, as Forbes first did, its significance and importance; and he further on confounds the *dirt bands* of Forbes with the superficial appearance of the veined structure. Especially imperfect and indefinite is the version here given of Tyndall's explanation of the origin of this structure; no reader would be likely to appreciate from these pages either the cogency of the arguments in favour of that explanation, or the difficulties which yet remain to be completely removed.

To those who are used to look for accurate knowledge and scrupulous care in German scientific works, it is disappointing to find that volumes designed for popular instruction in that country should be so deficient in all the requisites for imparting knowledge to unscientific readers, as this and some others which have lately appeared.

OUR BOOK SHELF

Kryptogamien Flora von Schlesien. Erster Band. (Breslau: J. U. Kern's Verlag, 1876.)

THIS flora is dedicated to the president of the "Schlesischen Gesellschaft für vaterländische Cultur," Prof. Goeppert, on the fiftieth anniversary of his receiving his doctor's degree. The whole has been ably edited by Dr. Ferdinand Cohn. It is proposed to continue the flora in two more volumes, one devoted to the Algæ and Lichens, and the third to the Fungi, but two years more must elapse before the completion of the entire work. The first volume includes two parts, the first containing the Vascular Cryptogams and Mosses, the second the Liverworts and Characeæ, with an appendix to the species of Mosses and Hepaticæ, and a copious index. The vascular cryptogams are described by Stenzel, and include twenty-one genera, fifty-three species, and ten sub-species. A history of the discovery of Silesian Pteridophyta is prefixed, and an interesting account of their distribution. Thus the species found on serpentine, limestone, and other rocks, are noted, as well as the hypsometrical distribution. Four regions of elevation are distinguished: 1, from 55 metres to 150 m.; 2, from 150 m. to 500 m.; 3, from 500 m. to 1,100 m.; and 4, from 1,100 m. to 1,500 m. The arrangement of some of the species and sub-species is not quite in accordance with our English ideas. Thus *Woodsia hyperborea*, Koch, is separated into two sub-species: 1, *arvonica*, With.; and 2, *rufidula*, Sw.; equal to *hyperborea* R., Br. and *ilvensis* R., Br. respectively. *Cystopteris montana* of British botanists is *C. sudetica*, Al. Braun and Milde. Then *A. dilatatum*, *spinulosum*, and *cristatum*, are all placed as sub-species of *Aspidium spinulosum*, Sw., and *A. aculeatum lobatum*, and *angulare* are made sub-species of *A. aculeatum*, Döll.

The Mosses and Liverworts are described by Limpricht, and occupy the greater part of the volume, there being 106 genera and 464 species of Mosses, and 39 genera and 132 species of Hepaticæ. A few additional species are added in the Appendix, bringing up the Mosses to 492 species and the Liverworts to 155. The same arrangement is here followed as to history and distribution as in the case of the vascular cryptogams. The descriptions seem excellent, and the information given very full and complete, the characters of the orders and families being given in great detail.

The Characeæ have been described by Prof. Alexander Braun. Probably this was one of the last important works from his prolific pen. All must deplore his recent loss. His vast knowledge, the importance of his contributions to botany, and his genial kindly manner, the readiness he always showed in assisting his students, are well known. To know him was to love him, and we esteem it a high privilege to have been one of his students. The Characeæ are not very numerous, three genera and fourteen species being enumerated; but in the hands of Prof. Braun it becomes a most valuable memoir on the whole group, while the species likely to be found in Silesia are all pointed out. The synonymy must be very confused, as Braun notices that *Chara flexilis*, Waller, includes three or four species of *Nitella*, three of *Tolypella*, and one *Chara*, *C. gracilis* of Sprengel is a still greater monster, as it includes five species of *Nitella*, one *Lychnothamnus*, and three species of *Chara*.

W. R. McNab

The Countries of the World, being a Popular Description of the Various Continents, Islands, Rivers, Seas, and Peoples of the Globe. By Robert Brown, M.A., Ph.D., &c. Vol. I. (London: Cassell, Petter, and Galpin. No date.)

This is certainly an attractive book; the wealth of illustrations renders it so. While we recognise some of the illustrations as having done service elsewhere, many of tehm are new, well-executed, and afford a good idea of the scenery, products, and people of the regions they are meant to illustrate. This volume treats of the Arctic regions and North America, contains a great amount of miscellaneous information, and is written in a rambling easy-going style. It is essentially a popular work, but might have been made valuable even to the geographical student had some of the pictures been dispensed with and a number of regional maps substituted similar to those which are so important a feature in Reclus' "Géographie Universelle," with which masterly and exhaustive work, however, it would be unfair to compare it. We have no doubt Dr. Brown's work will afford pleasure and prove instructive to many readers.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

Having been prevented from attending the recent meeting of the British Association by the necessity of devoting my entire vacation to mental and bodily renovation after the sad family losses I had sustained, I have only become aware within the last few days that my article in the April number of the *Nineteenth Century*, entitled "The Radiometer and its Lessons," had been there spoken of by Prof. G. Carey Foster, in his address as President of Section A, as showing an "unmistakable tendency, *either intentionally or unintentionally*, to depreciate Mr. Crookes's merits, and to make it appear that he had put a wrong interpretation upon his own results," which statement is said by your reporter to have "elicited great applause."

Of Mr. Crookes's own reply in the July number of the same periodical, entitled "More Lessons from the Radiometer," I took no notice; partly because my mind was at the time fully occupied by sad cares and urgent duties, and partly because I thought that his assertions (1) that he had not theorised on the subject at all, (2) that he had not attributed the rotation of the radiometer to the direct impetus of light, and (3) that he had never claimed the discovery of a new force or a new mode of force, were so well known in the scientific world to be inconsistent with fact, that I need not trouble myself to refute them.

Prof. Carey Foster, however, speaking with authority as President of the Physical section of the British Association, has given it as his judicial opinion that what I have written on this subject shows an unmistakable tendency to depreciate Mr. Crookes's merits, and to misrepresent his opinions; and he has further "unmistakably" suggested (as it appears to me) that this *may* have been done with deliberate intention, instead of being done in good faith under the influence of an unintentional bias. As it is impossible for me to allow such an imputation from such a quarter to pass unnoticed, I might fairly challenge Prof. Carey Foster to justify language which I must presume him to have used with all due consideration of its obvious meaning, and of *his* and *my* relative positions. But as he explicitly disavows the more serious part of this imputation, I have now only to ask to be allowed to show, in the columns of the journal which has not only recorded the accusation, but has pointedly directed attention to it,—*first*, that I have not, *even unintentionally*, "depreciated Mr. Crookes's merits" as the inventor of the Radiometer; and *secondly*, that Mr. Crookes really did in the first instance put that "wrong interpretation upon his own results" which I attributed to him. Had Prof. Carey Foster complied with the request I privately made him, that he should specify the passages which (in his opinion) justify his charge, I should have been able to reply to it much more briefly. But by declining thus to particularise, he obliges me to traverse the whole ground covered by his general accusation.

That I was not influenced, when writing on the Radiometer, by any *animus* arising from my personal antagonism to Mr. Crookes on another subject, will appear, I think, from the following extracts from the two lectures which I delivered at the London Institution (by special request) on Mesmerism, Spiritualism, &c., before Christmas, and which were published in *Fraser's Magazine* at the commencement of the present year:—

"The recent history of Mr. Crookes's most admirable invention, the Radiometer, is pregnant with lessons on this point. When this was first exhibited to the admiring gaze of the large body of scientific men assembled at the *soirée* of the Royal Society, there was probably no one who was not ready to believe with its inventor that the driving-round of its vanes was effected by the direct mechanical aid of that mode of Radiant Force which we call Light; and the eminent Physicists in whose judgment the greatest confidence was placed, seemed to have no doubt that this mechanical agency was something outside Optics properly so called, and was, in fact, if not a new Force in nature, a new *modus operandi* of a force previously known under another form. There was here, then, a perfect readiness to admit a novelty which seemed so unmistakably demonstrated, though transcending all previous experience. But after some little time the question was raised, whether the effect was not really due to an intermediate action of that *mode* of Radiant Force which we call *heat*, upon the attenuated vapour of which it was impossible entirely to get rid; and the result of a most careful and elaborate experimental inquiry, in which nature has been put to the question in every conceivable mode, has been to make it (I believe) almost if not quite certain, that the first view was incorrect, and that Heat is the real moving power, acting under peculiar conditions, but in no new mode."—*Lectures on Mesmerism and Spiritualism*, p. 8.

"I hold the warning given by the history of this inquiry, in regard to the duty of the scientific man to exhaust every possible mode of accounting for new and strange phenomena, before attributing it to any previously unknown agency, to be one of the most valuable lessons afforded by Mr. Crookes's discoveries.

"Now I maintain that it requires exactly the same kind of specially trained ability to elicit the truth in regard to the phenomena we are now considering, as has been exerted in the researches made by the instrumentality of the Spectroscope and the Radiometer. And I cannot but believe that if Mr. Crookes had been prepared by a special training in the bodily and mental constitution, abnormal as well as normal, of the Human instruments of the Spiritualistic inquiries, and had devoted to them the ability, skill, perseverance, and freedom from prepossession, which he has shown in his Physical investigations, he would have arrived at conclusions more akin to those of the great body of scientific men whom I believe to share my own convictions on this subject."—*Op. cit.*, p. 70.

No one, I think, can fail to see that in speaking of Mr. Crookes's "most admirable invention," and in giving him the fullest credit for the "ability, skill, perseverance, and freedom from prepossession," with which he had carried on his investigations in regard to it, I eulogised him as warmly as if I had never come into collision with him. It must also be apparent to any reader of these lectures, that I did not impute to him any blame for having originally fallen into an error shared at the time by the "eminent Physicists in whose judgment the greatest confidence was placed;" and that my reason for bringing forwards the subject was to enforce the lesson, that "no new principle of action has any claim to scientific acceptance, save after an exhaustive inquiry as to the extent to which the phenomena can be accounted for, either certainly or probably, by agencies already known."

Circumstances to which I shall presently advert having made me feel it desirable that this "lesson" should be yet more fully and emphatically set forth, I applied myself to a careful reperusal of Mr. Crookes's papers in the *Proceedings* of the Royal Society, with the most earnest desire to present a true history of the whole inquiry; and I availed myself of the opportunity kindly afforded me by the editor of the *Nineteenth Century*, to place before the public what I believed to be a fair statement of the case, with the lessons it conveyed.

Commencing with a description of the phenomena presented by the Radiometer when it was first exhibited by Mr. Crookes at the *soirée* of the Royal Society, I thus continued:—

"It is scarcely surprising, then, that a general impression should at once have prevailed that a capital discovery had been made—that of the *direct mechanical action of light;* which, though not indicating the existence of a new force in nature, showed that the most universally diffused of all forces, next to gravitation, has a *mode of action* which was previously not merely unknown, but altogether unsuspected. And this impression was not confined to those who had only a general acquaintance with Physical Optics; for it was shared by the greatest masters of that department of science, who had followed the course of the experimental researches on which Mr. Crookes had been for some time engaged, and of which this discovery was the culmination."—*Nineteenth Century*, April, 1877, p. 243.

I then went on to give, from Mr. Crookes's papers, a history of the investigations which had led him up to the Radiometer; and showed (p. 249) that at that stage of the inquiry, the argument for the *directness of radiant repulsion*, deducible from what was then supposed to be a fact—the increase of the rapidity of the rotation in proportion to the perfection of the vacuum—"seemed alike valid and cogent."

I next sketched the history of the opposite view originally propounded by Prof. Osborne Reynolds, supported by Dr. Schuster's experiment, and finally established by Mr. Crookes's own later researches, which have culminated in the doctrine of "heat reaction" now generally accepted. In reference to Mr. Crookes's own part in these subsequent inquiries, I say later on (p. 254), that "no sooner was adequate ground shown for calling in question his interpretation of the phenomena, and a *vĕra causa* found in an agency already known, than Mr. Crookes evinced the spirit of the true philosopher in varying his experiments in every conceivable mode, so as to test the validity of his original interpretation." And again in the next page I speak of his "carrying out this beautiful inquiry in a manner and spirit worthy of all admiration."—What higher praise *could* be given to a scientific investigator?

Having brought the history to its conclusion, I thus proceed:—

"Before adverting to the lessons which this remarkable history seems to me to convey, I would point out that this change of interpretation of the facts discovered by Mr. Crookes, does not in the least diminish either the interest of the facts themselves or the merit of his discovery. Nor is the value of his Radiometer in any degree lowered by the demonstration that it does not (as Mr. Crookes at first supposed) afford an absolute mechanical measure of radiant energy under any of its aspects. What (according to present views) it really does measure, is the amount of 'heat reaction' producible in gaseous atmospheres of different kinds and of different degrees of attenuation. And such a precise method of measurement appears more likely than any other mode of investigation, to furnish a test of that kinetic theory of gases, the recent development of which by Prof. Clerk-Maxwell is regarded by competent judges as constituting (if it should receive such verification) the most important advance ever made in molecular physics. Most deservedly, therefore, did Mr. Crookes receive from the Royal Society the award of one of its chief distinctions." (*Loc. cit.*, p. 251.)

To this I may add that I personally congratulated Mr. Crookes most cordially on that occasion, and expressed to him the deep interest with which I had followed his researches throughout. And though I had next to show that Mr. Crookes has another side to his mind, which makes Mr. Crookes the "spiritualist" almost a different person from Mr. Crookes the "physicist," I carefully guarded what I had to say on this point in the following words:—"I would not be thought for one moment to disparage Mr. Crookes's merits as the inventor of the Radiometer, by now bringing into contrast with the admirable series of scientific investigations which led up to that invention, what I cannot but regard as his thoroughly unscientific course in relation to another doctrine of which he has put himself prominently forward as the champion."

I cannot but surmise that Prof. Carey Foster must have read my paper rather carelessly, and have applied to Mr. Crookes, the inventor of the Radiometer, the depreciatory remarks I felt called upon to make in regard to Mr. Crookes, the supporter of a system, a large proportion of which even Mr. D. D. Home has recently denounced as "a seething mass of folly and imposture."[1] If Prof. Carey Foster knew as much as I do of the mischief which *this* Mr. Crookes has done, especially in the United States, on the one hand to his own reputation and to that of British science,[2] and on the other to public morality, by the facility with which he has lent himself to the support of frauds as wicked as those by which fortune-tellers delude ignorant and credulous servant-girls, he would not wonder that I should feel called upon to show that the high scientific ability of Mr. Crookes, the Physicist, neither prevents him from believing in his own day-dreams, nor renders him a match for the cunning of the clever female cheats who play upon his Spiritualistic "prepossessions."

I now pass to the second part of my defence; and shall show that for "making it appear that Mr. Crookes had put a wrong interpretation upon his own results," I can adduce adequate justification from his own published statements.

Of the "repulsion accompanying radiation" shown in his early experiments by the swinging-round of the pith bar, Mr. Crookes said, in 1874 (*Phil. Mag.*, vol. xlviii., p. 94), "*My own impression is that it is directly due to the impact of the waves*

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[1] See his "Lights and Shadows of Spiritualism," containing an unsparing exposure of its "delusions," its "absurdities," and its "trickeries."

[2] On the strength of a private letter from Mr. Crookes, which has been published (in *fac simile*) in the American newspapers, a certain Mrs. or Miss Eva Fay announced her "spiritualistic" performances as "endorsed by Prof. Crookes and other Fellows of the Royal Society." The particulars of the complete public exposure of this woman's disgraceful frauds, showing that Mr. Crookes's *scientific tests* are no more worthy of trust than the late Prof. Hare's *experimental demonstration of the immortality of the soul*, will appear in the forthcoming number of *Fraser's Magazine.*

upon the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation."

In a paper subsequently communicated to the Royal Society (*Proceedings*, March 12, 1875), Mr. Crookes characterised the explanation of the "repulsion from radiation" offered by Prof. Osborne Reynolds, as one which "*it is impossible to conceive*," the phenomenon taking place in a *chemical* vacuum. At the same time he stated that he was unprepared to offer any other explanation, and that "he should avoid giving any theory on the subject until a sufficient number of facts have been accumulated."

After bringing out the Radiometer, however, he reverted (as it seemed to me) to his previous "impression;" the whole phraseology of his papers of January 5 and February 5, 1876, appearing at the time, not only to myself, but to every one of the eminent scientific friends with whom I conversed on the subject, to indicate that he *then* considered the rotation as *directly due to the impact of the waves upon the surface of the moving mass.* Nor have I ever imputed it to him as a matter of blame that he took this view of it; on the other hand I have stated over and over again that this seemed the general impression of the distinguished Physicists to whom we "outsiders" looked for guidance in the matter. Anyone who remembers what took place at the Meeting of the Royal Society at which Mr. Crookes's paper was read, will, I feel sure, bear out this statement.

I shall now specify more explicitly the grounds on which I attributed to Mr. Crookes, no longer as an "impression," but as a definite "interpretation" of his facts, that the rotation of the Radiometer is due to the *direct impact of the waves*, and chiefly (I never said exclusively) to those of the *luminous* waves; and further attributed to him a claim to the discovery of a "new force" or "new mode of force."

This *key-note* seems to me to be most distinctly struck in the following passage:—After pointing out that "there is no real difference between Heat and Light, all we can take account of [I presume he means physically, not physiologically] being difference of wave-length," he thus continues: "Take, for instance, a ray of definite refrangibility in the red. Falling on a Thermometer it shows the action of *Heat;* on a Thermopile it produces an *electric current;*[1] to the Eye it appears as *light and colour;* on a Photographic plate it causes *chemical action;* and on the suspended pith *it causes motion.*"

Now (1) this motion being elsewhere spoken of as due to *the impetus given by a ray of light*, (2) a set of experiments being made to determine *the mechanical values of the different colours of the spectrum*, (3) an observation being recorded on *the weight of sunlight* (without the least intimation that he was "speaking figuratively," as Mr. Crookes says that he did to his audience at the Royal Institution), (4) the term *Light-mill*[2] being used by himself as a synonym for "Radiometer," and (5) no hint whatever being given of the dependence of the result (as argued by Prof. Osborne Reynolds) on a "heat-reaction" through the residual vapour, I still hold myself fully justified in attributing to Mr. Crookes the doctrine of *the direct mechanical action of light;* and I call on Prof. Carey Foster to prove—not that Mr. Crookes himself did not hold that doctrine—but (which is a very different thing) that I am not justified by Mr. Crookes's own language in attributing it to him.

That Mr. Crookes considered such action a "new force" or a "new mode of force," plainly appears from my previous citation; in which he ranks Motion as a mode of Radiant action *additional* to Light, Heat, and Actinism, differing as much from either of them as they differ from each other. If it does *not* mean this, what does it mean?

So, if Mr. Crookes has *not* changed his mind as to the interpretation of his facts, I ask (1) why he now repudiates as inappropriate the term *Light-mill* adopted (if not originally given) by himself? and (2) why does he now admit that dependence of the movements upon the presence of residual gas, which he originally affirmed to be *impossible to conceive?*

I have carefully confined myself to the *main issues* of this question. Prof. G. Carey Foster will doubtless be able to pick out *points of detail* in my article, as to which fault may be found by a severe critic. But I venture to think that I have said enough to prove that what I said on the subject was written under the honest conviction that I had adequate ground for my statements; and that I shall at any rate be absolved from the imputation of having ill-naturedly referred to the history of the Radiometer for the purpose of putting Mr. Crookes in the wrong; the "lesson" with which I concluded the article being as follows:—

"The lesson which this curious contrast [the 'duality' of Mr. Crookes's mental constitution, which I speak of as having plenty of parallels in past times, to say nothing of the present] seems to me most strongly to enforce, is that of the importance of training and disciplining the *whole* mind during the period of its development, of cultivating scientific habits of thought (by which I mean nothing more than strict reasoning based on exact observation) in regard to *every* subject, and of not allowing ourselves to become 'possessed' by any ideas or class of ideas, that the common sense of educated mankind pronounces to be irrational. I would not for a moment uphold that test as an infallible one; but it ought to be sufficiently regarded, to make us question the conclusions which depend solely upon our own or others' subjectivity, and to withhold us from affirming the existence of new agencies in Nature, until she has been questioned in every conceivable way, and every other possibility has been exhausted." (*Op. cit.*, p. 256.)

October 10 WILLIAM B. CARPENTER

I HEAR from Dr. Carpenter that he is sending to you, for publication in the next number of NATURE, a statement intended as a refutation of an opinion which I ventured to express, in my address to the Mathematical and Physical Section of the British Association at Plymouth, respecting an article on the "Radiometer," contributed by him to the *Nineteenth Century* for April. As Dr. Carpenter appears to have interpreted that expression of opinion in a sense different from that which it was intended to convey, I shall be much obliged if you will afford me space in your next issue for a few words of explanation.

The words which I used in referring to his article were these: "An eminent and accomplished scientific man had published, within the last few months, an account of the discovery of the radiometer, the unmistakable tendency of which was, either intentionally or unintentionally, to depreciate Mr. Crookes's merits, and to make it appear that he had put a wrong interpretation upon his own results." The word *depreciate*, which occurs here, is, I am aware, susceptible of various shades of meaning, and perhaps it would have been better if I had guarded myself against the possibility of misconception that lurks in it. What I meant was that Dr. Carpenter's account of Mr. Crookes's researches was likely to make his readers form a lower estimate of their scientific value than, in my opinion, they deserved; but whether or not it was *intended* to have this effect I did not undertake to say. I did not mean, and had not the smallest idea of suggesting, that Dr. Carpenter had been guilty of intentional and conscious unfairness towards Mr. Crookes. I should have thought it entirely unnecessary to disavow this latter interpretation of my words, and indeed should have considered it an insult to Dr. Carpenter's reputation to suppose that anyone would understand them in this sense, had he not himself (in a recent correspondence with me) endeavoured to fix this meaning on them in spite of my repeated assurances that it was not intended.

I do not wish to say anything on this occasion in support of the opinion which I have admitted that I did intend to express, but I shall ask you to allow me to do so in a future number of NATURE. G. CAREY FOSTER

University College, London, October 14

Mr. Wallace and Reichenbach's Odyle

As Mr. Wallace has attempted (presumably with Mr. Crookes's editorial concurrence) to rehabilitate, in the July number of the *Quarterly Journal of Science*, the Odyle-doctrine of Baron Reichenbach, I think it well to state that I yesterday availed myself of an opportunity of personally asking my friend

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[1] Having never heard of any physical philosopher from Seebeck to Sir William Thomson, who looked at the *electric current* generated in the Thermopile as anything else than an effect of the *heating* (whether by *conduction* or by *radiation*) of the two metals of which it was composed, I was greatly surprised at finding it ranked by Mr. Crookes as one of the *immediate* modes of Radiant action; and I called attention in my "Radiometer" paper to what I supposed to be his mistake on this point. It may be that in my ignorance of the newest developments of thermo-electric theory (my knowledge of it not being later than 1872, "Everett's translation of Deschanel," p. 652), I have here unintentionally "depreciated Mr. Crookes's merits;" and I shall be quite ready to recant and apologise for my mistake, if Prof. G. C. Foster will show that it is Dr. Carpenter, not Mr. Crookes, who is here in the wrong.

[2] It is impossible not to see, in the use of this term, a suggestion that the vanes are driven round by the direct mechanical impetus of Light upon them, in the same way as the sails of a Wind-mill are driven round by the direct impetus of the Wind.

Prof. Hoffmann, of Berlin, whether that doctrine any longer finds support among scientific men in Germany. *His reply was a most emphatic negative;* the doctrine, he said, being one which no man of science with whom he is acquainted would think worthy of the slightest attention. Yet in Mr. Wallace's judgment (*query* in Mr. Crookes's also?) the unanimous verdict of the scientific world of Germany, to say nothing of England, is a prejudiced one; only Mr. W. and his spiritualistic allies appreciating correctly the real force of the evidence originally advanced by Reichenbach, and confirmed by those trustworthy (?) authorities, Drs. Ashburner and Gregory.

In thus setting his own judgment on a question which lies altogether outside the scientific domain which he has made his own, against the unanimous verdict of the eminent physicists and physiologists who have carefully "tried" the Od-force and "found it wanting," and in rebuking myself and those who think with me for our incredulity, does not Mr. Wallace put himself somewhat in the attitude of his old opponent, John Hampden, who thinks everybody either a fool or a knave who maintains the earth to be round? WILLIAM B. CARPENTER

October 22

Potential Energy

WITH reference to the views of "John O'Toole" on the subject of energy perhaps you will allow me to say how one of the class to which "poor Publius" belongs has conceived the matter of terminology with satisfaction to himself.

1. Energy being unanimously defined by "the doctors" to be "capacity for doing work," and also energy conveying in its derivation the notion of activity, this term is properly applicable only to the bodies of material systems the motions of which are contemplated. Hence all energy is in its nature kinetic—the very term kinetic is logically included in the term energy.

2. When a material system is in motion it actually possesses, *ipso facto*, a capability of doing work, that is to say, it has actual energy.

3. When in any configuration of the system we contemplate as possible the action of causes which will alter its motions and give it a second configuration, the excess of the energy which it would possess in this second configuration over the energy which it possesses in the first is properly called its potential energy in the first configuration.

4. The assertion that in any configuration the sum of the energies, actual and potential, of a material system is constant, is what Kant would call an analytical proposition, or what "X." (quoting Herschel) calls "only a truism after all." But I further remark—

5. That this truism is not the principle of the conservation of energy, but that this principle is a true "synthetical proposition" which some fairly regard as an almost immediate deduction from Newton's third law, and which others regard as proved by often repeated and much varied experiment; and hence that "X.'s" statement of this great principle in the form—"The sum of the actual and potential energies *of the universe* is a constant quantity," (the italics are mine) is not its proper definition.

6. That, leaving the consideration of bodies, and referring to forces, the term to be employed instead of energy is work, and that the term analogous to the "potential energy of bodies" is the "potential work of forces," this latter being the amount of work which they are capable of doing in displacing their points of application from their actual configuration to any fixed chosen one.

7. That by the expenditure of a fixed amount of work on any material system the same amount of actual energy (whose type is $\frac{1}{2} m v^2$) is under all circumstances produced, and that, through whatever forms this actual energy is made to pass, if the whole of it is always utilised, it will finally be reconvertible into the same original amount of work, this being the principle of the conservation of energy.

8. That instead of the statement in 5, we must substitute the synthetical proposition that "the sum of the actual energy of the bodies in the universe and the *potential work of its internal forces* is a constant quantity," and the same is true of every material system which is regarded as complete in itself; or in other words, wherever and however a given quantity of potential work is lost by the forces of the system, this always appears in the shape of a fixed quantity of actual energy, in the form which we call heat, or in some other.

Hence we have energy, actual and potential, of bodies; and work, actual and potential, of forces.

A few remarks in conclusion. "J. M." has very happily illustrated the propriety of the expression potential energy, as, in strict consequence of the definition of energy, a potential capacity of doing work; and if in his illustration the "power of purchasing" is considered with reference to a further object, there may be not merely a "double remotion from" what we may regard as "tangibility," but a remotion of a higher multiple order. "W. G." has well explained that it is only in consequence of the fixedness of the earth that the potential energy of the system of the earth and stone is by the "doctors" located in the stone. Finally, I can hardly conceive how "X.," who has devoted so much attention to the literature of this subject, can have fallen into such a grievous error with regard to the clock.

Royal Indian Engineering College, G. M. MINCHIN
Cooper's Hill

YOUR "Potential Energy" corespondents will find three letters on the "Conservation of Energy" in the *Engineer* for January 12 and 19 and February 2 which may interest them. The writer "Φ Π" assumes that all the phenomena of force are explained by the theory that only matter and motion exist, and that what we call potential "energy" is only "quantity and motion," which motion is indestructible but diffusible. Z.

London, October 20

Origin of Contagious Diseases

I HAVE been much struck by the following passage in Dr. Richardson's address, NATURE (vol. xvi. p. 481):—

"(*c*) That as regards the organic poisons themselves and their physical properties, the great type of them all is represented by the poison of any venomous snake. . . . It is the type of all the poisons which produce disease."

Now has it been really proved, by experiment, that the poison of snakes produces the effects characterising the contagia? viz.,

"(*o*) . . . Each particle of any of these poisons brought into contact either with the blood of the living animal or with certain secretions of the living animal, possesses the property of turning the albuminous part of that same blood or that same secretion into substance like itself. . . ."

In other words, if an animal is suffering from snake poison does its blood or any of its secretions acquire the power of transmitting the disease, *i.e.*, the effects of a snake's bite, to another individual, as is the case with an animal affected with carbuncle, glanders, hydrophobia, &c., &c.?

Unless this question has been decided in the affirmative it would appear rather difficult to uphold the sentence (*c*) as quoted above. D. W.

Freiburg in Brisgau, G. J., October 14

[Dr. Richardson informs us that D. W. does not properly understand his argument. Dr. Richardson does not suppose that the person or animal poisoned from a poisonous snake is, in turn, poisonous, although that may be the fact. He merely uses the illustration that as a poisonous snake secretes a poison so an infectious person is for the time secreting a poison.]

I SEE by your issue of October 4, that Dr. Richardson has honoured me by mentioning my name and placing me as the first, in modern times, to advocate the hypothesis that living germs are the exciting agents of epidemic and infectious diseases. But he says further, "I protest, I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive." It may be so, but I look in vain through the whole story he narrates in his lecture to find a rational substitute for it, and it appears to me desirable at the present juncture that the principles of the germ theory, as I have interpreted them, should stand side by side with Dr. Richardson's "glandular theory." It is now nearly thirty years since I endeavoured to find some common root or cause for those diseases which we find in plants, animals, and man, and which are communicable among the individuals of each order in nature; also, in some instances, from one order to another. During that thirty years every step in scientific research and medical experience as far as my inquiries have carrried me, has tended to confirm the views I put forward in my original "Essay" and in subsequent papers read before the Epidemiological Society. Notably the latest advocates of a germ theory are two of our most eminent men, the one a leader in science, the other a leading physician. I need hardly say I allude to Prof. Tyndall and Sir Thomas Watson; surely these gentlemen cannot be charged with committing themselves to an hypothesis "*the most distant from the phenomena it attempts to explain.*"

Now if it can be shown that the germs of disease are subject to the same laws as other living things and exhibit similar phenomena, and further, that without the inference that they are endowed with vital properties, it is impossible to unravel the most striking character which they present to us for consideration, viz., the fact that they reproduce their kind, then I think there is more reason for following up, in all its intricacies, the germ theory, than to start with an *assumed catalysis*, *molecular motion*, *and a glandular matrix*, as suggested by Dr. Richardson.

Starting, then, from the indisputable fact that the *materias morbi* of every communicable disease reproduces its kind, I have considered this a primary law, and have tabulated other laws which are associated with living beings by which it will, I think, be found that there is a parallelism of a kind to attract and rivet attention, especially, too, when many otherwise inexplicable circumstances bend to this hypothesis.

Primary Law of Reproduction, by which all living things reproduce their kind.

SECONDARY LAWS.

Objective Laws.	*Subjective Laws.*
1. The diffusion or dispersion of germs.	1. Seasons of activity.
2. Their static existence.	2. Climatic influence.
3. Limited duration of active existence.	3. Relation to latitude.
4. Period of development, maturity, and decay.	4. Subjection to physical forces.
5. Intermittent reproduction.	5. Influence of locality.

Without amplifying this subject, which would carry me far beyond the limits of an ordinary communication, I will only add that though the above tabulation is very imperfect, there is quite sufficient for any one who will follow out the ideas conveyed by it to trace the intimate relation that exists between living beings and the germs of disease. I would refer finally to the fact that many diseases in men and animals have yielded up living germs as their cause, chiefly, I may add, skin diseases it is true; but *aphtha*,[1] closely associated with diphtheria, is, I think, acknowledged by all unprejudiced persons to have its origin in an unmistakable and demonstrable germ. JOHN GROVE

The Zoological Relations of Madagascar and Africa

WITHOUT entering into the details of this very difficult question I wish to be allowed to state some of the general reasons which have led me to a different conclusion from Dr. Hartlaub,[1] and also to point out where he has not quoted my opinions with perfect accuracy. Instead of saying that "the fauna of Madagascar is manifestly of African origin," my actual statement is as follows:—"We have the extraordinary fauna of Madagascar to account for, with its evident main derivation from Africa, yet wanting all the larger and higher African forms; its resemblances to Malaya and to South America; and its wonderful assemblage of altogether peculiar types" ("Geog. Dist. of Animals," vol. i. p. 286). My reasons for believing in the "*main derivation*" of the fauna from Africa can only be understood by considering the theory, now generally admitted, of the origin of the fauna of Africa itself. All the higher mammalia are believed to have entered it from the northern continent during the middle or latter part of the tertiary period, and the occurrence of *Psittacus* and of forms supposed to be allied to plantain-eaters and to *Leptosomus* in the miocene of France, render it probable that many of the peculiar groups of African birds had their origin in the old Palæarctic region. Now Madagascar presents many cases of special affinity with South Africa, especially in insects, land-shells, and plants; and if we suppose it to have formed part of a South African land before the irruption of the higher mammals and birds from the north, we shall I think account for many of its peculiarities. Such facts as its possessing *Potamochœrus* and the recently extinct *Hippopotamus*, while it has thirteen or fourteen peculiarly African genera of birds against four or five that are peculiarly Oriental; of its having many African genera of lizards and tortoises; of its butterflies being decidedly African; of its numerous African genera of Carabidæ, Lucanidæ, and Lamiidæ; while the specially Oriental affinities of its mammals, reptiles, and insects are hardly if at all more pronounced than the South American affinities of the same groups,—all seem to me to warrant the general conclusion that the "main derivation" of the Madagascar fauna is from Africa.

Dr. Hartlaub speaks of my "attempted parallel between Madagascar and Africa, and the Antilles and South America" in such a way that his readers must think I had dwelt upon this parallel in some detail as being special and peculiar. The fact is, however, that I have always referred to it in a very general way. At p. 75 vol. i. I say: "The peculiarities it (the Malagasy sub-region) exhibits, beings of exactly the same kind as those presented by the Antilles, by New Zealand, and even by Celebes and Ceylon, but in a much greater degree." And again, at p. 272, vol. i., I speak of it as "bearing a similar relation to Africa as the Antilles to Tropical America, or New Zealand to Australia, but possessing a much richer fauna than either of these, and in some respects a more remarkable one even than New Zealand." This *general* comparison with the *two* other great insular sub-regions is, I think, justifiable, notwithstanding great differences of detail. There is in all a rich and highly peculiar fauna, a great poverty of mammalia, and a total absence of many large families of birds characterising the adjacent continent, together with special points of resemblance to distant continents or to remote geological periods.

It seems to me that such a problem as this cannot well be solved by means of a group which, like birds, do not require an actual land-connection in order to reach a given country; and, if *all* land animals are taken into account, the evidence does not appear to warrant the supposition of a recent land-connection of Madagascar with India or Malaya. At a very remote epoch such a connection *may* have taken place, but if we are to give any weight to the general facts of distribution as opposed to those presented by birds only, the union of Madagascar with South Africa is more recent and has had more influence on the character of the Malagasy fauna. The numerous and very remarkable points of affinity between Madagascar and South America in almost every group except birds, are not alluded to by Dr. Hartlaub, yet they would equally well support the notion of a former union of those two countries independently of Africa. It seems, however, more consonant with our general knowledge of distribution to consider these as cases of survival of ancient and once wide-spread types in suitable areas; and this is a principle that must never be lost sight of in attempting to solve the problems presented by such anomalous countries as Madagascar. ALFRED R. WALLACE

Selective Discrimination in Insects

YOUR correspondent S.B., in his letter in NATURE of yesterday's date, must be referring to some short abstract only of my lecture on flowers and insects. I quite agree with him that odour is very important in attracting insects, and dwelt upon it in my lecture, as well as in my little book on "Flowers and Insects." A striking illustration is afforded by night flowers, which often become peculiarly odoriferous towards evening, as has been already pointed out by various observers.

S.B. attributes, I think, too little importance to the colouring of flowers, but his letter shows him to be a careful observer, and I hope he will continue to devote his attention to the subject.

He would find H. Müller's "Blumen und Insekten" a mine of most interesting and accurate observation.

London, October 19 JOHN LUBBOCK

Protective Colouring in Birds

WITH reference to the statement in my "Naturalist in Nicaragua," p. 196, that the macaw "fears no foe," &c., the well-known geologist, Prof. Gabb, sends me the following information:—"I willingly comply with your request to repeat the statement about the *Kukong pung* or macaw hawk of Costa Rica. Not having your book by me now I cannot refer to page nor quote your statement exactly. But as I recall it, you speak of the great red and blue macaw as being so well defended as to need no protective colouring, and that no hawk dares attack it. In this you are mistaken. Not only have I seen on several occasions heaps of the unmistakable feathers of the bird in the woods, left in the manner that all woodsmen recognise as hawk's work, but I have the statements of various Indians, not in collusion, confirming each other, and finally I have had the bird pointed out to me (I am not sure but that it may occur in the collection I sent to the Smithsonian). It is a fair-sized hawk of dark been established had quite justified by success the experiment of their formation. The number of societies in union had increased from twenty-one to twenty-five, and of members from 1,050 to 1,300. The change of name having been mentioned, the proposed issue of Transactions was spoken of. In response to the council's appeal for funds, a liberal, though of course not as yet sufficient, response had been made. There are at present about 105 subscribers to the funds, which, after defraying the expenses of the meetings, will be devoted to the publication of Natural History Transactions for the county. The financial statement showed a balance available for purposes of publication of over 20*l.* Mr. Henry Clifton Sorby, F.R.S., of Sheffield, was unanimously elected president.

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[1] See *Medical Times*, 1851, vol. ii. p. 95.

[1] NATURE, vol. xvi. p. 498, and the *Ibis* for July, 1847, p. 334.

THE Committee of the Chester Society of Natural Science announces that in connection with the Gilchrist Educational Trust, six Popular Science Lectures will be given in the Town Hall, Chester, on successive Tuesday evenings, commencing Tuesday, November 6, by Prof. W. C. Williamson, F.R.S., Prof. P. Martin Duncan, M.D., F.R.S., and Dr. William B. Carpenter, C.B., F.R.S. The admission to each lecture is only one penny. The Chester Society has founded a Kingsley Memorial, which has taken the form of a number of prizes, details concerning which will be found in our advertising columns.

THE following is the title of the essay to which the Howard Medal of the Statistical Society will be awarded in November, 1878, the essays to be sent in on or before June 30, 1878, "The Effects of Health and Disease on Military and Naval Operations." The council have decided to grant the sum of 20*l.* to the writer who may gain the "Howard Medal" in November, 1878.

M. DE FONVIELLE sends us an account of a balloon ascent he made on the 18th instant in the *Hydrogene.* The departure took place from the Champ de Mars at half-past three P.M. The balloon at first was pushed by a gentle north-easterly wind blowing in the direction of the valley of the Seine to an altitude of 600 metres, when it met with another aërial current coming from north-west. The air was quite moist. The rays of the disappearing sun covered the ground with a strong red colour, and M. de Fonvielle suggests that the planet Mars takes its reddish shade owing to the large quantities of vapour disseminated in its atmosphere. When the *Hydrogene* was passing from the inferior current to the superior it received a sensible shock and vibrated like a pendulum for some time. From 1,200 metres to 1,600 he found a wind blowing from the north-east like the first met after starting. The air was cold and dry. The temperature, when ascending, was 13° Cent. in the shade and at 1,600 meters he found 3° Cent. under 0. When landing at a quarter-past six the thermometer gave 10° Cent. on the ground at Bonelle, about forty kilometers from the Champ de Mars. During about twenty minutes, having descended in the vicinity of land not above 300 metres, the balloon met a south-westerly wind which was prevailing in the valleys of Bievre. That current of air was obviously enough a modification of the principal wind in consequence of the hilly character of the district. Such local winds observed by meteorologists can lead to no practical conclusions at all. If we consult the readings taken at the stations of the International Service on October 18 at six P.M., we find almost no two stations having the same wind in the Parisian district. The only wind worthy of note was that shown by the direction of the clouds, and of which no account was taken. M. de Fonvielle states he never met in his many aërial journeys with circumstances so strongly in favour of the introduction of cloud observations in weather prognostications. The principles advocated by Buys Ballot, and practised by Norwegian observers, received a strong confirmation.

ON the map of Major W. F. Raynolds, embracing that portion of the Missouri River country traversed by himself in 1859 and 1860, there was first recorded the peculiar hydrographical feature known as the "Two-Ocean Water." Its position is there indicated roughly by means of dotted lines, according to the account given by Bridger, the guide of the party. In the report of the expedition Raynolds remarks that "having seen this phenomeon on a small scale in the highlands of Maine, where a rivulet discharges a portion of its waters into the Atlantic and the remainder into the St. Lawrence, I am prepared to concede that Bridger's 'Two-Ocean River' *may* be a verity." Dr. Hayden, after a careful reconnaissance of the region, reported that such a phenomenon was at least doubtful, at the same time suggesting that the "low ridge in the great water-divide of the continent has doubtless given rise to the story of the Two-Ocean River, and such a stream has found its way to most of our printed maps. The expedition of Capt. W. A. Jones, in 1873, ascended the valley of the Upper Yellow-stone for twenty-five or thirty miles, the trail of the party left the marshy bottom-lands to traverse the drier portion about fifty feet above the stream upon the right bank. The river at this point was then (early in September) rather sluggish, the slope being somewhat gradual. Presently they crossed a small, but rapid rivulet, coursing down the mountain side, and falling abruptly into the valley just beneath them. Beyond them the view was unobstructed, but the stream appeared to ascend the slope towards them, until they observed that the rivulet had divided in the plain below, one portion gliding silently into the river behind them, to find its way at last into the Gulf of Mexico, while the other branch had descended in front to join the westward flowing waters of the Columbia, *via* Snake River, finally reaching the Pacific Ocean. The true position of this remarkable feature of physical geography is clearly shown on the map which illustrates the report of Capt. Jones ("Report upon the Reconnaissance of N.W. Wyoming made in the summer of 1873, by Wm. A. Jones, Captain of Engineers. Washington: Government Printing Office, 1874"). The mountain stream now bears the name proposed by Raynolds—the "Two Ocean Creek"—and its two branches are named respectively, Atlantic and Pacific creeks. Thus is verified another of the stories of that faithful guide and hunter—James Bridger—one of the most worthy of Rocky Mountain pioneers.

THE Eleventh Annual Report of the Warden of the Standards has been issued. The office of Warden of the Standards, on the retirement of the late Warden, Mr. Chisholm, was associated with that of the Permanent Secretary of the Board of Trade, so that the Report is signed by "T. H. Farrer," as Warden of the Standards. The practical duties of the department are discharged by Mr. H. J. Chaney. Among the subjects referred to in the Report is the rude and antiquated method of teaching weights and measures in our schools, which has been referred to in previous papers of the department. It would appear, from present arithmetic books that the student may still be taught the particulars of weights or measures which can be of no possible use to him in after-life. Another interesting point referred to is the merits of "short-arm" and "long-arm" balances. To meet the requirements of modern science a balance is needed by which the weights of bodies may be determined most accurately and quickly. Prof. Mendeléef, of St. Petersburg, and Herr Bunge, Mechanicus, Hamburg, have shown that it is possible to weigh quickly and accurately by the use of a balance whose beam has much shorter arms than those now in general use. A practical test of the relative merits of a beam with long arms and of one with short arms has been made in the department. By this test, as well as from the mathematical consideration of the question, it has been ascertained that whilst the probable error of a weighing made with the short-arm balance is slightly greater than the probable error of a weighing made with the long-arm balance, yet a weighing by the long-arm balance occupies twice as much time as one made with the short-arm balance. Consequently great economy of time is obtained by the use of a short-arm balance. Prof. Barff's process for preventing the corrosion of iron appeared to the department to be likely to prevent the oxidation of Standard weights made of iron. Prof. Barff has undertaken to submit some specimens of iron weights to this process, and it is intended to place these weights in the hands of some local inspector of weights and measures so that it may be ascertained whether iron Standard weights thus protected could safely be used in place of the expensive bronze or brass Standard weights at present used. The Report refers to several other points connected with the working of the department, one of importance to the general public being its operations in connection with the testing of gas meters.

Mr. A. Ringwood, of Adelaide, South Australia, publishes a plan by which one observer may measure the height of the clouds. The observer is to note the altitude and azimuth of the cloud, the azimuth of the cloud's shadow, and the spot in the surrounding country where the shadow falls; from this last observation by means of a map he can find the distance of the shadow from him. From these elements, together with the altitude and azimuth of the sun, a variety of expressions for the height of the cloud can be deduced. The method is equivalent to taking observations at each end of a base whose length is the distance of the shadow from the observer, the observer at the shadow end of the base being the sun. The difficulty of recognising the shadows of individual clouds and the comparatively short time in each day during which it would be possible to do so, joined with the fact that the higher clouds, such as cirrus and cirrocumulus, hardly ever cast defined shadows, must prevent the method from becoming generally useful; but still there is this to be said for it, that if in an observation the base, *i.e.*, the distance of the cloud's shadow from the observer, is long, a good measure of the height of the cloud may be got with comparatively rough observations of the other elements.

The singing of mice is a phenomenon which was recently affirmed by Dr. Berdier in a letter to *La Nature*. A distinguished herpetologist, M. Lataste, suggested that he may have made confusion with the singing of a raniform batrachian, the *Bombinator igneus*, but Dr. Berdier said there was no marshy ground near the room in which he had heard it, and he stuck to his assertion. His observation has been confirmed at a recent meeting of the French Société d'Acclimatation, by M. Brierre, who stated that he, with several others, had heard mice sing at Saint-Michel-sur-l'Heron (in Vendée), in 1851–1853. The singing (which was at first attributed to reptiles) came from an old cupboard bought in a market-place, and concealing mice. It was about sunset that the sounds generally commenced. M. Brierre soaped the joints and the wood so that he might open the cupboard suddenly without noise. He did the latter one evening soon after the sounds had commenced, and succeeded in observing, for about a minute, the movements of the throat of a mouse, which emitted a song like that of a wren, the snout being elongated and held up in the air, as a dog does when he howls. He seized the animal with his hand and called others to see it, but it got off. The singing was resumed the same night and those following. M. Brierre is unable to attribute the singing of the mice (as Dr. Berdier does) to imitation of that of canaries, for he had no birds in the house, nor had the previous proprietor of the cupboard any.

In describing some recent falls of meteoric stones in America, Mr. Lawrence Smith has pointed out (*Comptes Rendus*) that in the last eighteen years there have been, in the United States, twelve falls of meteorites which have been collected; and he notes the remarkable fact that eight of the meteorites, representing more than 1,000 kilogrammes of matter, have fallen in the region of the Western Prairies, and on a surface which does not exceed one-eighth of the extent of the United States. This cannot evidently be attributed to there being a dense population and numerous observers (a consideration sometimes urged). Still more striking is the circumstance that in the last sixty years there have been twenty falls of meteorites observed in the United States, of which ten, or the half, have fallen in this same region; and, moreover, these falls have brought about 1,200 kilogrammes of mineral substance, a quantity twenty times greater than that of the ten other falls recorded as having occurred outside of this region.

It has long been known that the photographic image of a luminous object is dilated at the expense of the dark parts or the field itself. This has been merely attributed to a gradual advance of the chemical action (without further attempt at explanation). The phenomenon has recently been studied by M. Angot (*Journal de Physique*). He finds that the dimension of the images increases with the intensity of the light, with the duration of exposure, with the sensibility of the plate, with diminution of the aperture of the objective, and that it is greater when the plate has not previously been impressed by diffuse light than when it has. M. Angot discards the hypothesis of a mysterious advance of chemical actions, and shows how the phenomena are accounted for by the ordinary theories of optics. This variation of the diameter of images is inevitable in practice; to render it very small the operator should satisfy himself that the objective used is aplanetic, *i.e.*, free, as far as possible, from aberrations of sphericity and refrangibility. He has only then to take account of variations due to diffraction, which may be attenuated by using objectives of large aperture. It is by using an objective without sensible aberration and of fifteen inches aperture that Mr. Rutherford has succeeded in obtaining his magnificent photographs of the moon.

Some interesting experiments on the photo-electricity of fluorspar have recently been described by M. Hankel to the Saxon Academy of Sciences. His attention was drawn to the phenomena in studying the thermo-electric properties of crystals. The new effects were found much more intense than those got by heating the crystal, or by friction of its surface with a brush; moreover, they were of contrary sign, and so must be attributed to an action proper to light. The principal results of experiment are these: The centre of a crystalline face presents, after exposure of about an hour to sunlight, a strong negative tension, while the tension towards the sides is much less, and even most frequently positive. An exposure of the crystal to the temperature of 95° for several hours produces, during cooling, the smallest positive tensions at all points of the crystal. Experiments made by filtering light through coloured glasses, a layer of water, a solution of alum or of sulphate of quinine, showed that the chemical rays are much the most active. Lastly, a too strong concentration of light on the crystalline face removes all sensibility to the ulterior action of light. To give an idea of the degree of tension observed in such experiments, M. Hankel states that a brass plate 95 mm. in diameter, connected with a zinc element, copper, and uninsulated water, gave a deflection (in an electrometer composed of a gold leaf hung from an insulated brass rod between two insulated plates of brass) of 1°·2 on approaching the centre point as nearly as possible, and about 0°·6 on approaching the edge. The deflections obtained by exposition to light reached 21°, and even 26° in the centre of surface of a crystal electrified by light.

As supplemental to the article on the blue gum-tree, at p. 443 of Nature, vol. xvi., the following notes from a report on the culture of *Eucalyptus* in Algeria by Consul-General Playfair will, no doubt, be interesting. With regard to rapidity of growth, it seems that the first trees ever planted in Algeria were sown in 1862, and upon being measured in 1874, that is at twelve years of age, gave a circumference at one metre from the ground of 1·52 metres, another of eleven years growth gave 1·42, and another, planted in fresh alluvial soil, nine years of age, gave 1·57 metres—about six inches more, Col. Playfair says, than he could embrace with both arms. To the question of the sanitary effects of *Eucalyptus* a good deal of attention has been directed. An inquiry was instituted by the Society of Physical and Natural Sciences at Algiers under the presidency of Dr. Bertherand, and the result was that from thirty localities reports were received, all of which speak favourably of the *Eucalyptus* as a fever preventive. On the banks of Lake Fetzara, near Bône, 60,000 young trees of *Eucalyptus globulus* were planted in 1869. At the present time they have attained a height of from 7 to 8 metres each, and have, it is said produced a very marked effect on the locality. Such was the feverish condition of this district on the annual fall of the water and the denudation of its banks that the director of the Jardin d'Essai, who went to examine the condition of the plants, was immediately seized with a violent fever which lasted twenty days. This gentleman, however, now reports that the miasmatic influences which affected him so strongly then have disappeared, and the mosquitoes which rendered the place uninhabitable have disappeared with them. At the great iron mines of Mokta et Hadid it was formerly impossible for the workmen to remain there during the summer; those who attempted to do so died, and the Company was obliged to take the labourers to the mines by train every morning, and to carry them back to Bône at night, a distance of 33 kilometres each way. From 1868 to 1870 the Company planted more than 100,000 *Eucalyptus* trees, and now the workmen are able to live all the year through on the scene of their labour. Consul Playfair advances the following reasons as accounting for the causes of the improvement in climate from planting *Eucalyptus* trees:—"In some places," he says," the trees destroyed miasma by utilising the moisture of the soil in which they were planted, and thus draining marshes; the emanations from their leaves also may have produced a salutary effect. They contain a large quantity of essential oil very similar to turpentine, which they emit in great quantities, especially when stirred by the wind, and this acts, it is supposed, as a febrifuge." We are further told that considerable numbers of *Eucalyptus* have been planted all along the railway from Algiers to Oran. Where this line passes through the Metidja the trees have grown most successfully, but in the Chelif they have proved almost an entire failure. This, however, may have been due to their receiving no attention whatever after being planted.

WE have received reprints of two papers by Dr. C. Le Neve Foster, one "On Some Tin-Lodes in the St. Agnes District," and the other "On a Deposit of Tin at Park of Mines."

WE have received an interesting little publication, by Mr. Edwin Lees, F.L.S., reprinted from the *Transactions* of the Malvern Naturalists' Field Club, on "The Forest and Chase of Malvern, its Ancient and Present State," with notices and illustrations of the most remarkable old trees remaining within its confines.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. Richard Turner; an Azara's Fox (*Canis azaræ*), two Brazilian Cariamas (*Cariama cristata*), a crested Screamer (*Chauna chavaria*) from the Argentine Republic, two Crab-eating Raccoons (*Procyon cancrivorus*) from South Brazil, a Yarrell's Curassow (*Crax carunculata*), a Sclater's Curassow (*Crax sclateri*), a Globulose Curassow (*Crax globulosa*), a Garden's Night Heron (*Nycticorax gardeni*) from Brazil, four Silky Cow Birds (*Molothrus bonariensis*), a Banded Cotinga (*Cotinga cincta*), a Yellow-footed Thrush (*Turdus flavipes*) from Bahia, deposited; an Ostrich (*Struthio camelus*) from Africa, received in exchange.

THE LIMITS OF NATURAL KNOWLEDGE[1]

II.

HAVING considered the capacity of the subject and the accessibility of the object, we must now turn our attention to the copula, *i.e.*, the demands which we make of knowledge.

As all conceptions which we form of nature are exclusively the results of sensual perception, our knowledge cannot go further than to compare the phenomena we have observed, and judge them with reference to one another. If any phenomenon of a special nature occurred only once, if, for instance, we were the only existing organisms our insight would be extremely limited, because all our knowledge of the human organism we have essentially obtained from its connection with all other organic beings. The comparison of many phenomena gives us a unit or a standard by which we can measure and determine each single one. We therefore obtain just as many measures as there are properties in nature which we can perceive by our senses or which can be inferred from sensual perceptions by our judgment. As these measures are deduced from finite facts they have only a relative value, and our knowledge remains finite for the same reason.

We therefore understand a phenomenon, we know its value with regard to other phenomena if we can *measure*, *count*, or *weigh* it. We have a clear idea of the size of the lowest fungus, of which we must place some 2,000,000 or 3,000,000 individuals side by side to complete the length of a metre, of the size of an elephant, of the earth, of our solar system, the radius of which is somewhere about 3,000,000,000 miles. We have a clear idea of the time in which a ray of light carries to our eye the writing of a book which we read, and which is about the $\frac{1}{800000000}$th part of a second—of the life-time of the lowest fungus which in a plant-box or in the human body is replaced by a new generation after only twenty minutes—of the life-time of an oak which may be several thousand years, and of the 500,000,000 years which have passed since the generation of organisms upon our earth.

Natural bodies are composed of parts; the value of their internal structure, of their organisation, is exactly determined by the quantity, nature, and arrangement of these parts. They therefore give us the measure by which we judge the compound whole, and with which we measure its organisation as it were. The morphological or descriptive natural sciences by these measurements obtain their scientific data. Chemistry, which at the present time is still an eminently morphological science and which investigates the formation of compounds from elementary atoms, and mineralogy, which presupposes the uniform arrangement of molecules, have arrived at a state of great perfection. The common measure for organisms is the cell, and further on the organ; the common measure for the systematic unities of organic nature (for varieties, species, genera) we find in individuals and generations.

We are enabled not only to compare the different objects and measure them by one another, but in as far as it changes, we may also compare a system, a unit (*einheitliche*) group of things of similar nature, with itself and measure it by itself. The knowledge of the change is complete if the later stage is proved to be the necessary consequence of the earlier one, or the latter to be the necessary predecessor of the later one, if one can be constructed from the other, if therefore both stages can be brought into the relation of cause and effect to one another.

In the elementary domains of the material this causal relation is the mechanical necessity, which for two successive stages demands the equal sum of motion in a certain direction (or living force) and of potential energy. Among the sciences which apply here astronomy ranks first; next in efficiency are several physical sciences, particularly the mechanical theories of heat and optics. Physiology, or the physics of the organic world, tries to penetrate into a far more difficult and more complicated domain, by following the footmarks of her older sister.

In the higher domains of the material we cannot for our causal knowledge uphold the demand for this mechanical necessity. Indeed this is perhaps impossible in the case of all structure.

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[1] Address delivered at the Munich meeting of the German Association, by Prof. C. von Nägeli, of Munich. (The author, in a note to the German original, remarks that this lecture had to replace another in the programme, which had been promised by Prof. Tschermak, of Vienna. At the eleventh hour Prof. Tschermak announced his inability to attend the Munich meeting, and the author was requested by the secretaries to fill the gap thus occasioned. The address therefore, the author states, bears the stamp of its hasty origin, as it was written during a journey in the Alps, when there was neither sufficient leisure nor opportunity for careful and elaborate work.) Continued from p. 535.

We shall probably never succeed in explaining definitely why the origin of a chemical compound and of a crystal must be the necessary result of known forces and motions of elementary atoms and molecules. This will be the case still less with the formation of cells, with the growth of organisms, with the propagation and inheritance of peculiarities. And yet we may, even in these domains, speak of causal knowledge with some show of right, only the elements which constitute this knowledge are not simple forces and motions, but very complicated combinations of these, which are not analysed further. Our causal knowledge will arrive at perfection when we succeed in predicting future events with the same certainty and exactness as astronomers do. Now we already find certain indications of this in the chemistry of compounds and in organic morphology, since it is possible to make deductions from certain stages of development of an organism with regard to earlier or later stages of the same. And a time will arrive when the organic laws of the still youthful history of development of the individual and of the still younger history of development of the species will have been more investigated, and when we need no longer presuppose ontogenetic and phylogenetic necessity as a matter of course, but when we will also be able to understand the cause of this necessity.

The objection will perhaps be raised that causal knowledge certainly consists of our understanding the necessity, as in the case of mechanics, but that this does not apply in domains where we must start from uninvestigated compound objects. The mechanics of the heavens is based upon general gravitation and centrifugal force, and both are simple forces acting in a straight line. But both are hypotheses, which rest upon our experience and of the reason of which we are ignorant. Astronomy reveals to us the necessity of astronomical phenomena only under the supposition of facts we have experienced—not the necessity in itself. If we were to demand that to our knowledge the "why?" should be clear, there would not even be any astronomical nor yet any physical knowledge. In the organic domains causal knowledge is entitled to the same importance as physical knowledge is in the inorganic field. By experience we know a system of forces and motions, for example, the cell. We ascertain certain general facts relating to this system (in the same way as with gravitation and centrifugal force in the heavens), and we use these facts for further deductions. Our insight into the necessity of some process of growth consists in our recognising this process as a necessary consequence of those facts.

Our knowledge of natural things therefore rests upon our being able to measure them, either by themselves or by one another. Another method of observation leads us to the same result. We understand and master something perfectly, if we create it ourselves, because in this case we see its cause. The only thing in the domain of knowledge, which, based upon our sensual perceptions, we can accomplish, is mathematics. The tenor of this formal science is perfectly clear to us, because, indeed, it is the product of our own mind. We can therefore also understand real things with certainty, as far as we find mathematical ideas, number, magnitude, and everything which mathematics deduces from these, realised in them. Natural knowledge therefore consists in our applying mathematical methods to natural phenomena; to understand a natural event means nothing else as it were, than to repeat it in thought, to reproduce it in our mind.

While designating natural knowledge as *mathematical* and at the same time as *relative*, which judges things according to a measure deduced from themselves, I depart considerably from the views of my predecessor, Prof. Du Bois Reymond. He considers it to be a condition of natural knowledge, that we should succeed in reducing the changes in the material world to motions of atoms caused by their central forces which are independent of time, or in other words, in resolving natural phenomena into the mechanics of atoms.

While Du Bois Reymond thus starts from the undeniable fact that a compound can only be known from its parts, yet he stops not at the finite and real parts, but continues the division down to the real *unities*, which are unthinkable, and thus he marks out the conditions for impossible *absolute* knowledge. But as we do not crave divine but only human knowledge, we may not ask more of the latter than that in each finite sphere it should advance as far as mathematical understanding; and the saying of Kant, that in each special natural science we can find only as much *real* science as we can find mathematics in it, is after all still quite correct.

If Du Bois Reymond wishes to continue the analysis of matter down to atoms with simple central forces, he carries a favourite method of modern physics and physiology to extremes, and if he shows that this way of proceeding does not lead to understanding, he destroys the claims of exclusive adherence to the domain of science, which the employers of this method sometimes raise. If physics and physical physiology go back to supposed atoms, material points, elements of volume which we imagine to be infinitely small, then this hypothesis is justified inasmuch as the real chemical molecules are so small that we may, without error of calculation, consider space to be continuously filled with matter. For instance, for a molecule of albumen, consisting of numerous atoms of carbon, hydrogen, oxygen, and nitrogen, we may substitute a mass differential of this compound. At all events it is useful to make this hypothesis, as it must be seen how far a conception of this kind can be treated mathematically, and as from the result we may draw conclusions backwards with regard to the composition of matter.

But we must beware of the opinion which is frequently associated with this method, that it alone is natural science and that knowledge can only be gained by employing it. In this case we would have to confine our desire to understand nature to a single domain, and we would lose others which are capable of safe confirmation. Natural knowledge need not necessarily begin with hypothetical and the smallest unknown things. It begins wherever matter has shaped itself to unities of the same order, which may be compared to and measured by one another, and wherever such unities combine to form compound unities of a higher order, and yield a measure for their comparison with one another. Natural knowledge may begin at every age from the organisation or composition of matter; at the atom of chemical elements, which forms the chemical compounds; at the molecule of the compounds, which composes the crystal; at the crystalline granule, which composes the cell and its parts; at the cell, which builds up the organism; at the organism or individual, which becomes the element of the formation of species. Each natural scientific discipline has its justification essentially in itself.

Our knowledge of nature is therefore always a mathematical one, and consists either in simple measurement, as in the morphological and descriptive natural sciences, or in causal measurement, as in the physical and physiological sciences. By means of mathematics, however, by weight, measure, or number, we can only understand relative or quantitative differences. Actual qualities, absolutely different properties, escape our understanding, since we possess no measure for them. We cannot conceive really qualitative differences, because qualities cannot be compared. This is an important fact for our attempts to understand nature. Its consequences are, that if within nature there are domains which are qualitively or absolutely different, scientific knowledge is only possible separately within each single one of them, and that no connecting bridge leads from one domain into another. But another consequence is that, as far as we can investigate nature continuously, as far as our measuring knowledge advances without gaps, and especially as far as we understand one phenomenon through another, or can prove it to have arisen from the other, that absolute differences, chasms which cannot be filled, do not exist at all in nature.

I have tried to determine the capacity of the Ego, the accessibility of nature, and the essence of human understanding. It is easy now to fix the limits of natural knowledge.

We can know only what our senses acquaint us with, and this is limited in time and space to an infinitesimal domain, and perhaps only to a part of the natural phenomena occurring in this domain, on account of a deficient development of our organs of sense. Of that with which we are acquainted at all, we can only know the finite, the changeable and perishable, only what is relative and differs by degrees, because we can only apply mathematical ideas to natural things, and can judge the latter only by the measures we have gained from themselves. Of all that is endless or eternal, of all that is stable or constant, of all absolute differences we have no conception. We have a perfect idea of an hour, a metre, a kilogramme, but we have no idea of time, space, matter and force, motion and rest, cause and effect.

The extent and limit of our possible natural knowledge we may shortly and exactly state thus:—*We can only know the finite, but we can know all the finite which comes within reach of our sensual perception.*

If we are clearly conscious of this limitation of our knowledge we free natural observation from many difficulties and errors, which consist, on the one hand, in the attempt to investigate not only the really finite, but a mixture of the finite and the eternal, which is uninvestigable; and, on the other hand, in our not following the finite strictly and incessantly, but stopping here and there in the midst of it and changing it for the eternal.

It would lead me far indeed, if I were to consider the consequences singly, which have arisen from the want of a correct method based upon principles. The most remarkable ones, which at the same time claim a general interest, are the opinions, that finite nature is divided into two radically different domains, and particularly that there is an insuperable limit between inorganic and organic, or between material and spiritual nature. I will speak only of the latter opinion.

The antagonists of an intimate connection between material and immaterial nature draw the line of separation in different places. In the opinion of some, living nature generally (or "life-endowed" (*beseelte*) nature, inasmuch as life is also ascribed to plants) represents something absolutely special, while others admit this only for the animal world endowed with sensation, and yet others only for the spiritually conscious human race; new immaterial or eternal principles are said to apply to the higher grades. Du Bois Reymond holds the second of these views; he says that in the first trace of pleasure which was felt by one of the simplest beings in the beginning of animal life upon our earth, an insuperable limit was marked, while upwards from this to the most elevated mental activity, and downwards from the vital force of the organic to the simple physical force he nowhere finds another limit.

It is difficult for the naturalist to oppose the supposition of immaterial principles, which are said to arise suddenly here and there in nature, as it places itself at once upon a stand-point which floats in the air outside of natural science, and cannot, therefore, be attacked directly and contradicted by him. Natural science can only show that this supposition is superfluous, because everything can be explained in a natural way, and also improbable, because otherwise a contradiction is introduced into finite nature which gainsays the whole of our experience, and offends our mental desire to find causal relations everywhere.

Experience shows that from the clearest consciousness of the thinker downwards, through the more imperfect consciousness of the child, to the unconsciousness of the embryo, and to the insensibility of the human ovum, or through the more imperfect consciousness of undeveloped human races and of higher animals to the unconsciousness of lower animals, and of sensitive plants, and to the insensibility of all other plants, there exists a continuous gradation without definable limit, and that the same gradation continues from the life of the animal ovum and the vegetable cell downwards through organised elementary and more or less lifeless forms (parts of the cell) to crystals and chemical molecules.

But the conclusion we draw by analogy is this:—Just as all organisms consist of and have been formed of matter, which occurs in inorganic nature, so the forces, which are inherent in matter, have of course entered into the formations as well. If matter combines with other matter, then their forces unite to some total result, and this represents the new property of the resulting body; this property is of course only relative. Thus vermilion is mercury + oxygen − heat; sugar is carbon + hydrogen + oxygen − heat. And thus life and feeling are new relative properties which albumen molecules obtain under certain circumstances. Accordingly, experience shows that spiritual life is everywhere connected in the most intimate manner with natural life, that the one influences the other and cannot exist without the other. It is necessary, therefore, as everywhere in nature forces and motions are united only with material particles, that the spiritual forces and motions also appertain to matter, in other words, that they are composed of the general forces and motions of nature and are connected with them as cause and effect. No naturalist can avoid the conception of a causal connection of this nature, unless he becomes unfaithful, consciously or unconsciously, to his first principle. The problem is, therefore, to understand how the forces of inorganic matter combine in matter which forms into organisms, so that their result represents life, sensation, and consciousness. The solution of this problem is yet very remote; but it is possible. We may give sufficient indications for each single point.

Permit me to speak more minutely of one of these points; I mean the one in which my predecessor sees a limit to natural knowledge. This is all the more tempting since for the rest Du Bois Reymond places himself upon the basis of the causal principle, if indeed not in words quite so direct, yet quite as determined and unconditional; and since if this *one* gap were filled, no other would exist for his point of view. To him the whole world-history, even the whole system of the universe, is the consequence of the mechanics of atoms. There is no action of the mind, which could not be calculated from the forces and the motions of matter, if it were possible to know these. The material occurrences which are connected with the solution of an arithmetical problem, with the pleasure of musical sensation, with the intellectual pleasure over a scientific discovery, are products of cerebral mechanics. The mind can indeed be looked upon as the secretion of the substance of the brain, in the same way as gall is the secretion of the liver, as Karl Vogt, and previously Cabanis, have said.

Du Bois Reymond declares all this to be intelligible in principle; but, he says, we learn to know only the conditions of mental life, but not how from these conditions mental life results. Sensation and consciousness doubtless accompany the material processes in the brain by necessity, but they stand outside of the causal law and remain eternal enigmas to us.

It is not uninteresting to follow Du Bois Reymond's view, which I have just stated and which he details and illustrates with various examples, into its consequences, and to consider clearly its general result. We then arrive at this:—The finite mind, as it has developed itself through the animal world up to man, is a double one; on the one side the acting, inventing, unconscious, *material* mind, which puts the muscles into motion and determines the world's history; this is nothing else but the mechanics of atoms, and is subject to the causal law; and on the other side the inactive, contemplative, remembering, fancying, conscious, *immaterial* mind, which feels pleasure and pain, love and hate; this one lies outside of the mechanics of matter and cares nothing for cause and effect.

Generally both sides of mental life are collectively called mind. Du Bois Reymond exclusively designates the latter as mind, and if the separation existed in the way described this would certainly be the truly unintelligible secretion of the material mind, or of the atoms of the brain; it would not be anything but the useless ornament of this material mind, its infallibly following, unreal shadow. Because it stands outside of the chain of cause and effect, it is powerless and without influence upon actions; without it the world's history would have run exactly the same course as it did. Also without consciousness mathematical formulas would have been invented, written down, taught, and applied, telegraphs and steam engines would have been constructed; also without consciousness theological and philosophical discussions would have been held, printed, read, and their authors burnt at the stake; also without conscious memory lessons would have been learnt by heart in the schools and examinations held; also without musical sentiment music would have been composed, repeated at rehearsals, performed and listened to with all external signs of pleasure or disapprobation; also without poetical or artistic sentiment poets, painters, and sculptors would have produced their works, and these would have been admired and criticised. Therefore *without* a conscious and perceived mental life, we should have thought, done, and spoken everything, but only mechanically, and not otherwise than a very artistically-invented dead automaton would think, act, and speak.

We cannot deny the sublimity of this conception of the universe; the impression it makes upon the naturalist must be all the greater, because it proceeds consequentially everywhere and does not offend any natural scientific principle; as to the immaterial and the unintelligible it assigns a domain, which lies outside of the connection of natural and real things. For this reason also this conception cannot be discussed from a natural scientific point of view. And yet to the naturalist certain objections present themselves.

Can we imagine that so many occurrences, which most evidently resulted from sensation and consciousness, have some other sensationless and unconscious origin? Can we imagine that sensation and consciousness are so entirely useless, and while everywhere utility (*zweckmässigkeit*) is so eminently prominent in organic nature, that so useless and superfluous a phenomenon should occur just where we expect the greatest utility? Can we imagine that the causal principle, which governs the whole of nature, fails us just at the most important part? Can we imagine that organised matter accidentally and without cause acquires a property (sensation and consciousness), and loses it again accidentally and without effect, because in the ovum and in the embryo the conscious and perceived mental life would not be present, it would arise gradually, it would be lost in sleep every night, obtained again more or less completely in the waking state, and annihilated for ever in death?

The conscience of the naturalist is little satisfied by this new dualism, although he cannot directly contradict it. It is true that this dualism is infinitely different from the ordinary dualism, since it assigns the exclusive power to the forces of nature, and to the mind only an inactive, empty dignity, and thus hinders in no way the strictly causal or materialistic conception of all material occurrences, also of those which bring about mental life. But nevertheless we would wish for a solution which corresponds more to our experiences and to our theoretical conceptions. And I believe that this solution lies very near if we extend our judgment of the phenomena in organic nature to those of inorganic nature as well.

It is quite correct in Du Bois Reymond to say that we can only know the material conditions of mental life, but that how this results from those conditions remains a secret to us for ever. But it would be an error to suppose that we generally understand the origin of natural life from its causes. In all purely material phenomena we find the same barrier as in the mental ones. We know by experience that in the inorganic world the cause is lost in the effect, but we cannot understand the nature of the transfer. We know by experience that a stone thrown up into the air falls to the ground, and we say that this happens because the earth attracts it; but this attraction is for us incomprehensible.

What we do know is, that two bodies which are apart act upon one another in such a way that, if there is no obstacle, they approach one another until they touch. In what, however, this action consists, how it adduces the mutual motion, is for us just as unintelligible, and will remain just as eternal an enigma, as the origin of sensation and consciousness from material causes. With all material, physical, and chemical phenomena, we find the same. A body charged with positive electricity, and another one charged with negative electricity, move towards each other; two bodies similarly electrified repel one another. If we say that in the former case attraction, and in the second repulsion takes place, then these are only short expressions which comprise whole series of similar phenomena, but give no explanations. But we accustom ourselves to such expressions; little by little we use them so frequently and easily, that we believe we really understand the phenomena they designate. And that is why the view is generally held, that nature in her simpler inorganic phenomena offers no difficulties to our conception, whereas in reality the difficulties are everywhere the same in principle.

The objection will perhaps be raised, that the two sides of the question are not quite so equal as I say; that with purely material phenomena the relation between two material particles, which causes their motion, is indeed incomprehensible; that with mental phenomena this incomprehensible relation between the material particles is also given; but that something else, something new is added, namely, the mental action which accompanies the material phenomenon. But this objection, if indeed we raise it, would be unfounded; we should have overlooked that the two sides into which we should divide the mental phenomenon are equally present with the purely material phenomenon, only that they are not separately conceived here but in one, namely, the sensation and the reaction which this sensation causes.

This fact, that the simplest inorganic phenomena are quite as inaccessible in their origin as the most complicated occurrences in the human brain, constructs the bridge which may lead us to a monistic (*einheitliche*) conception of nature. Let us start from what we know—and in this case it is the complicated mental phenomenon—in order to obtain from it a conception of what we still ignorant of.

We know mental life only from our subjective experiences; we know that we draw conclusions, that we remember, that we feel pleasure and pain. That similar but undeveloped phenomena occur with children and higher animals, we conclude from their actions and from their somatic manifestations, which we interpret as the expression of emotion and sensation. Actual proofs that even the lower animals still possess sensation, which is only different in degree from the conscious sensation of man, we have only in their movements consequent upon some irritation, and in the important circumstance that these movements upon irritation in the ascending animal classes pass through all gradations upwards to the most complicated phenomena in the human brain. From these irritation-movements of the lowest animals we imperceptibly get to those of the unicellular plants and of the sensitive plants, and thence to the phenomena of the apparently insensible plants, which cannot be distinguished from the phenomena of inorganic nature. Between the irritation-movements of plants and animals, however, and the apparently insensible inorganic movements, there is no other difference but this, that in the case of irritation a powerful cause acts upon numberless material particles arranged in a similar manner, and thus produces a movement of place or sensation which becomes perceptible to our senses, while when this perceptible movement is wanting, the cause of the molecular movements, which take place in several directions, is not called an irritation.

In the higher animal world sensation is distinctly present in the movements consequent upon irritation. We must therefore credit the lower animals with it as well, and we have no reason to deny it in the case of plants and inorganic bodies. Sensation causes us to feel pleasure or displeasure. Generally speaking the feeling of pleasure arises when our natural inclinations are gratified, and the feeling of pain when this gratification is denied. As all material phenomena are composed of the motions of molecules and elementary atoms, pleasure and pain must have their original seat in these particles; they must be caused by the manner in which these infinitesimal particles are able to respond to the attracting or repelling forces which act upon them. Sensation, therefore, is a property of the albumen molecules; and if we grant it in the case of albumen molecules we must grant it likewise in the case of the molecules of all other substances.

Let us now consider the relation of two molecules of different chemical elements (for instance that of a hydrogen molecule and one of oxygen), which are at a minute distance from each other. Each of them, according to the present notions of chemistry, consists of two not further divisible, but yet decidedly compound atoms. By means of its composition the atom has different properties and forces, and therefore acts differently (attracting or repelling) upon other atoms. The two molecules in question experience or feel their mutual presence in a different manner; they act upon each other with different attractive or repulsive power.

Let us examine what happens in the case of a certain attraction, for instance, in that of a chemical one. Three possibilities exist: either the molecules follow their inclination and approach one another, or they are condemned to rest through other forces which are equal to the attraction, or they move away from one another, the forces counteracting their inclination gaining the upper hand. The same three possibilities are given for a certain repulsion, for instance, through heat; the two molecules follow their natural inclination and move away from each other, or they remain at the same distance, or they are pushed towards one another by other causes, their inclination being overcome.

Now if the molecules possess anything which is ever so distantly related to sensation, and we cannot doubt it, since each one feels the presence, the certain condition, the peculiar forces of the other, and, accordingly, has the inclination to move and, under circumstances, really begins to move, becomes alive as it were, moreover, since such molecules are the elements which cause pleasure and pain; if therefore the molecules feel something which is related to sensation, then this must be pleasure if they can respond to attraction and repulsion, *i.e.*, follow their inclination or disinclination; it must be displeasure if they are forced to execute some opposite movement, and it must be neither pleasure nor displeasure if they remain at rest.

As the molecules act upon each other with several unequal attractive and repulsive forces, some of their inclinations, whenever they are in motion, are always gratified, while others are offended. But these different sensations are necessarily unequal with regard to condition and intensity, according as they are caused by the general attraction of gravitation, by the general repulsion of heat and of elasticity, by electric and magnetic attraction and repulsion, or by chemical affinity. The simplest organisms which we know, if I may use this expression, the molecules of chemical elements, are therefore simultaneously influenced by several qualitatively and quantitatively different sensations, which conglomerate to a total sensation of pleasure or pain.

At the lowest and simplest stage of material organisation which we know, we therefore find on the whole the same phenomenon as we do at the highest stage, where it appears as conscious sensation. The difference is only one of gradation; at the highest stage the influences have only become so much more vivid in consequence of a vast accumulation of different material particles, and much more compound and intricate on account of the complicated organisation.

If we look upon mental life in its most general signification as the immaterial expression of the material phenomena, as the mediation between cause and effect, then we find it everywhere in nature. Mental force is the capacity of material particles to act upon each other. The mental phenomenon is the performance of this action, which consists in motion, therefore in a change of position, of the material particles and the forces inherent in them, and by this leads directly to a new mental occurrence. Thus the same mental chain encircles all material phenomena.

The human mind is nothing else but the highest development upon our earth of the mental phenomena which move and animate nature everywhere. But it is not the product of secretion of the cerebral substance; as such it would be without further influence upon the brain, just as the secreted gall is of no further signification for the liver. On the contrary, sensation and consciousness have their firm seat in the brain, with which they are indissolubly united, and in which, by their intervention, new conceptions are formed and converted into actions. Just as the stone would not fall down if it did not feel the presence of the earth, so the trampled worm would not wriggle if it had no sensation, and the brain would not act reasonably if it had no consciousness.

This conception satisfies our causal demands entirely. For the naturalist it is a logical necessity to admit only differences of degree in finite nature. In the same way as there is a common measure for everything in space as well as for everything in time, so there must be a common measure for all mental phenomena. In the same way as there are gradations in material nature from the most simple to the most complex, so there must be similar gradations in mental nature, which is parallel to the former. It is true that in atoms and molecules we do not yet find pleasure and pain or love and hate pronounced with decision, but yet we find the first germs, as it were the original beginnings, of these feelings, and it would be the task of a comparative psychology to follow consciousness through unconscious sensation down to the insensible action of material particles.

But the domain of the mind offers far greater difficulties to our investigation than the material domain, because we can only use our subjective perceptions as immediate experience, and because we do not possess a special organ of sense which enables us to make objective observations of other bodies. The observation with our senses, which are organised for quite different objects, acquaints us only in a roundabout way and in a very defective manner with the mental occurrences in other beings, and our judgment of them is all the more uncertain the further we depart in nature from the human species itself. It will therefore, perhaps, never be possible to find the measure for the mental phenomena really, to determine it, and to raise comparative psychology to the rank of a natural science.

Natural knowledge remains limited to what is finite, the naturalist must therefore confine himself strictly to the finite only. The demand, which is often addressed to him, that he should have a more philosophical mind, that he should criticise in a philosophical manner, because it is impossible to avoid metaphysical speculation entirely, only shows how difficult it is to separate two absolutely different domains, which have once been mixed up only to produce general confusion. The power of education and habit also was, up to the most recent period, an obstacle in the way of a complete and radical separation of these two domains, and yet it is certain, and we know by experience, that every metaphysical addition turns natural science and natural investigation into a turbid and muddy alloy.

Natural science must be exact; it must rigidly avoid everything which oversteps the limit of the finite and the intelligible, and which is transcendental; it must proceed in a strictly materialistic manner, because its sole object is finite, force-endowed matter; and it must not forget that this true materialism is an empirical and not a philosophical one, and that it is bounded by the same limits as those of the domain upon which it moves.

I do not wish to say by this that the naturalist is not allowed to philosophise, that he is forbidden to move in ideal and transcendental domains. But he ceases to be a naturalist, and the only thing, which from his vocation is perhaps of advantage to him, is that he keeps both domains strictly apart; that he knows how to treat the one as the pure domain of investigation and knowledge, and the other, while he frees it from everything that is finite, as the hidden domain of presentiment.

To the human mind, to our desire of investigation and knowledge, the whole sensually-perceptible world is open. We penetrate into the greatest distances by means of the telescope and calculation, and into the smallest spaces by means of the microscope and combination. We investigate the most complex and complicated organism, which belongs to ourselves, in the most varied directions. We recognise the forces and laws governing nature, and through this we subject the whole inorganic and organic world, as far as we can reach it. If man reviews the triumphs in the domains of science and power which have been obtained up to the present, and thinks of the still greater future conquests, then he may with pride feel himself lord of the world.

But what is this world, over which the human mind reigns? Not even a grain of sand in the eternity of space, not even a second in the eternity of time, and only an outwork of the true essence of the universe. Because even of the infinitesimal world, which is accessible to us, we only know what is changeable and perishable. All that is eternal and stable, the *how* and the *why* of the universe, remains for ever incomprehensible to the human mind, and if it tries to overstep the limit of finiteness it can only puff itself up to a ridiculously-adorned idol, or desecrate the eternal and the divine by human disfiguration. Even the matured mind, which would have arrived at complete natural scientific insight, and would wish to free the divine of everything finite and perishable, could, in its restriction, make of divinity only a constitutional phantom-king, who, according to the words of a statesman recently deceased, would "reign, but not govern." In the finite world the eternal natural forces rule unalterably, and we recognise their effects in the laws of motion and change. Whether and how they are the tenor and expression of a conscious eternal design is past our comprehension.

If my predecessor, Prof. Du Bois Reymond, ended his address with the crushing words, *Ignoramus, et ignorabimus*, then I close mine with the conditional but more consolatory utterance that we do not merely know, but really understand the fruits of our investigations, and that our knowledge bears in itself the germ of an almost infinite growth, without, however, approaching omniscience by the smallest step. If we practise reasonable resignation, if, as finite and perishable human beings, as we are, we are satisfied with human insight, instead of claiming divine knowledge, then we may say with full confidence—

"We know, and we shall know!"

ON THE SOLAR ECLIPSE OF AGATHOCLES

B.C. 310 (15th August).[1]

THE mean motion of the moon round the earth was formerly assumed to be constant, until Halley showed that it has been gradually increasing by a small amount during the last few thousand years. Halley made this discovery by the study of ancient solar eclipses, which were found always to occur to the *eastward* of their calculated places:—this indicates a slower mean motion of the moon in former times, as may be thus shown—a spectator in the northern hemisphere looking at a solar eclipse will face the south, having the west on his right hand, and the east on his left hand; and he will see the moon cross the sun's disc from right to left. When we calculate backwards to an old eclipse (attributing to the moon her present mean motion), we are, in fact, unwinding, from left to right, the path she has described since the eclipse happened, and by this unwinding process we find that we always place the moon to the *right* (*west*) of the place where she was actually when the eclipse occurred. Thus, all the ancient eclipses being observed at places to the *eastward* (*left*) of their calculated places of observation, we learn that the moon's mean motion was formerly slower than it now is. The coefficient of the moon's mean motion, found by Halley, from ancient eclipses, was

$$10{\cdot}2 \times n^2,$$

where n is the number of centuries.

The acceleration of the moon's mean motion was first explained by Laplace, who showed that the mean central disturbing force of the sun, by which the moon's gravity towards the earth is diminished, depends not only on the sun's mean distance, but on the eccentricity of the earth's orbit. This eccentricity has been diminishing for many ages, while the mean distance remains unaltered. In consequence of this, the sun's mean disturbing force is diminishing, and, consequently, the attraction of the moon towards the earth has been increasing, and with it, of course, the mean motion of the moon has been also increasing. The calculations of Laplace, confirmed and extended by Damoiseau and Plana, gave a coefficient for the moon's mean motion agreeing with that found from observation by Halley.

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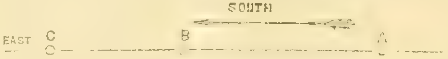
[1] Paper read before the Mathematical Section of the British Association, Plymouth, 1877, by Rev. Dr. Samuel Haughton, F.R.S. (Trin. Coll. Dublin.)

This satisfactory agreement between theory and observation remained unchanged until 1853, when Adams announced[1] that he had found a deficiency in Laplace's calculation, arising from the fact that Laplace had considered the radial disturbing force only, and had neglected the tangential disturbing force.

When the fuller computation is made, it is found that the coefficient of Halley's expression is reduced from 10·2 to 6·11, leaving 4·09 not accounted for.

Adams' calculations were verified by Delaunay, who found them quite correct, and who had the merit of suggesting the explanation of the 4·09, which form a *residual phenomenon.* According to Delaunay, this uncompensated portion of Halley's coefficient is to be explained by the retardation of the earth's angular velocity, and consequent increase in the length of the day, caused by the residual tidal current setting constantly from east to west. This residual current, although excessively small, is a *vera causa* always acting, and must, in due course of time, produce a sensible effect in lengthening the day. It is easy to show that the effect of the lengthening of the day upon ancient solar eclipses acts in the same direction as the acceleration of the moon's mean motion, viz., it throws the place of observation to the eastward (left) of the calculated place; for the earth moves from right to left, in the same direction as the moon, and as its rotation in that direction, from the period of the eclipse, has been *greater* than that assumed in our calculation from the *present* rotation of the earth, it follows that, at the time of the eclipse, all places on the earth's surface must have been absolutely, with reference to a meridian fixed in space, to the westward (right) of their present positions. According to this view, therefore, the displacement eastwards of the places of observation of ancient eclipses, when compared with the calculated places of observation, is the sum of two displacements—one caused by not allowing for the *acceleration of the moon*, and the other caused by not allowing for the *retardation of the earth.*

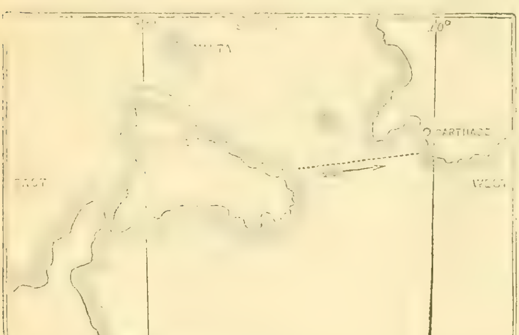


Thus, if B represent the true position of the eclipse in space, its calculated place will be A, to the west of B, the interval A B being due to the neglect of the acceleration of the moon's mean motion (with coefficient = 6·11) in the calculation; and the point exactly below B, on the earth's surface, will have moved on to C, to the east of B, in consequence of the neglect of the retardation of the earth's rotation in the calculation.

Let us illustrate the case by one of the most famous solar eclipses on record, that of Agathocles, on August 15, 310 B.C. The accompanying outline map represents the course taken by the expedition of Agathocles from Syracuse to Carthage.[2]

This eclipse is recorded by Diodorus Siculus, and has been always considered one of the most important in support of Halley's coefficient, 10·2 seconds.

It has recently, however, been called in question by a high authority; for at the meeting of the American Association for the Advancement of Science (1877), "Prof. Simon Newcomb presented a communication on the secular acceleration of the moon, and its increasing deviation from uniformity through many years. He reviewed the existing theory on the subject; the calculation of Laplace according with Halley's estimate of the acceleration as about 10¼ seconds of time, to be multiplied by the square of the centuries for a given period; also the Adams theory, which reduces the explanation of Laplace to 6 seconds, leaving more than 4 seconds to be otherwise accounted for. In ascribing the surplus acceleration to diminished rotation of the earth, we are dealing with a subject where the evidence should be carefully weighed. Much dependence seemed to be placed on the record of ancient eclipses. Prof. Newcomb considered these eclipses separately. *The most promising of the Greek solar eclipses was that of Agathocles, tyrant of Syracuse, occurring at the commencement of his voyage to attack Carthage. But we do not know on which side of Sicily he sailed: according to whether he was on one or the other side of the coast, the difference of time for that eclipse may be calculated as justifying the* 10 *seconds or the* 6 *seconds acceleration of the moon.* The eclipse known as that of Thales has a record still more open to criticism, because it came to its historian by hearsay, and probably through two or three generations after the lapse of a hundred years. It seems curious that if Thales predicted the year (by an estimate of lunar periods) he did not also predict the day. Each of the ancient solar eclipses yielded similar elements of doubt, on careful examination. From the records of lunar eclipses, if all uncertain features be weeded out, the old estimate of acceleration will be reduced one-half. The Arabian records of lunar eclipses were published at Leyden in the early part of this century. The work is very rare. Altitudes of sun and moon are constantly given in it. Calculations from these eclipses give the smaller estimate of acceleration. From all the data he has been able to study, Prof. Newcomb concludes that the whole amount of acceleration is about 8·4 seconds. He hopes to make further estimates from modern records, having had the good fortune to pick up in Paris carefully compiled data of occultations going back to 1680."



Let us compare this statement of Prof. Newcomb with the original account of Diodorus Siculus. Agathocles was blockaded in Syracuse by the Carthaginian fleet, and the town was in danger of starvation; under these circumstances he formed and carried out the daring project of breaking the blockade, and undertaking an expedition by sea against Carthage itself; which he successfully accomplished. Diodorus says: "But Agathocles, thus overtaken and surrounded, hit upon an unexpected chance of escape when night came on; and on the following day there came to pass so great an eclipse of the sun that night appeared universally, the stars being seen in every direction; wherefore the people of Agathocles, believing that the Divinity foreshadowed some evil to happen them, were in still greater anxiety of mind than before. When they had voyaged for six days and as many nights, at the dawn of day the fleet of the Carthaginians appeared unexpectedly, not far off. . . . But when Africa came in sight, an incredible exhortation to the rowers and rivalry took place. The ships of the barbarians indeed went faster, because for a length of time they had been accustomed to the handling of the oars; but the ships of the Greeks preceded them by a small interval; and, having finished their voyage as quickly as possible, they immediately sprang upon the strand like wrestlers; and, indeed, the leading ships of the Carthaginians attacked the aftermost ships of Agathocles, having come within range of missiles." —DIOD. SIC., lib. xx., ch. 5, 6.

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[1] *Proceedings* of the Royal Society, vol. vi. p. 321.

[2] The places passed in order by the expedition of Agathocles along the Sicilian coast are described in the fine lines of Virgil:—

Sicanio prætenta sinu jacet insula contra
Plemmyrium undosum; nomen dixere priores
Ortygiam. Alpheum fama est huc Elidis amnem
Occultas egisse vias subter mare: qui nunc
Ore, Arethusa, tuo Siculis confunditur undis.[1]
Jussi numina magna loci veneramur: et inde
Exsupero præpingue solum stagnantis Helori,
Hinc altas cautes projectaque saxa Pachyni
Radimus, et fatis nunquam concessa moveri
Adparet Camarina procul campique Geloi,
Inmanisque Gela fluvii cognomine dicta,
Arduus inde Acragas ostentat maxima longe
Mœnia magnanimum quondam generator equorum,
Teque datis linquo ventis, palmosa Selinus;
Et vada dura lego saxis Lilybeia cæcis,
Hinc Drepani me portus et inlætabilis ora
Adcipit.—ÆN., Lib. III., 692-708.

'Ο δ' 'Αγαθοκλῆς περικατάληπτος ἤδη γενόμενος, ἐπιλαβούσης τῆς νυκτὸς, ἀνελπίστου σωτηρίας ἔτυχε. Τῇ δ' ὑστεραίᾳ τηλικαύτην ἔκλειψιν ἡλίου συνέβη γενέσθαι, ὥστε ὁλοχερῶς φαινῆναι νύκτα, θεωρωμένων τῶν ἀστέρων παντοχοῦ. . . . 'Εξ δ' ἡμέρας καὶ τὰς ἴσας νύκτας ἀυτῶν πλευσάντων, ἀποφαινούσης τῆς ἕω ἕω, παραδόξως ὁ στόλος τῶν Καρχηδονίων οὐκ ἄποθεν ὢν ἑωράθη.—DIOD. SIC., Lib. xx., ch. 5, 6.

From this narrative it can be clearly shown that Prof. Newcomb is mistaken when he says that "*we do not know on which side of Sicily he sailed.*" It is quite certain that the eclipse occurred before the expedition had weathered the promontory of Pachynus, or had made any sensible westing in their voyage.

The total distance, on a coasting voyage from Syracuse to Carthage, is 350 English miles, and the distance from Syracuse to Cape Pachynus is forty miles. Now, the whole time of the voyage was six days and as many nights, together with a portion of a night at Syracuse, and a portion of a day near Carthage (the stone quarries). Allowing six hours each to these, we have:—

	Hours.
Part of night of outset at Syracuse	6
Six days and as many nights	144
Part of day near Carthage before landing at the "Stone Quarries"	6
	156

This is the minimum time allowable from the narrative, and any longer time allowed will strengthen my argument. The rate of rowing during the voyage was, therefore,

$$\frac{350}{156} = 2{\cdot}25 \text{ miles per hour.}$$

At this rate of rowing it would require 17h. 48m. to reach Cape Pachynus, a distance of forty miles; so that if the expedition sailed at midnight, it would have been off Pachynus, to the eastward, at 5 P.M., which is the time assigned by Petavius for the middle of the eclipse (Syracusan time). It is, therefore, perfectly clear that if the expedition had got so far to the westward as to allow of the coefficient (6·11), the eclipse must be thrown into the wrong day, which is inadmissible.

If Delaunay is to be trusted, the expedition must have gone out of the Mediterranean into the Atlantic before the coefficient 6·11 could be verified. He says:—"Nous avons dit que la durée du jour augmentait d'une seconde dans l'espace de 100,000 ans. Mais cela se produit progressivement, de telle manière que ces augmentations successives des jours s'ajoutent, et au bout d'un grand nombre de jours, font un total appréciable. Si on remonte à une époque, de 2400 ans époque à peu près, à laquelle on rapporte les éclipses historiques dont on a parlé, on voit que l'observation de l'une de ces éclipses a dû être faite $1^{h}\frac{3}{4}$ plus tôt que si le ralentissement du mouvement de rotation de la terre n'avait pas existé.

"La variation relative aux anciennes éclipses va donc jusqu'à $1^{h}\frac{3}{4}$. Ainsi une éclipse a été observée à un certain moment $1^{h}\frac{3}{4}$ plus tôt qu'elle ne l'aurait été sans le ralentissement.

"Prenons les trois éclipses principales rapportées par l'histoire. Celle de Thalès, arrivée 585 ans avant J.—C., a été vue en Asie Mineure; sans le ralentissement du mouvement de rotation de la terre, on l'aurait vue dans l'ile de Sardaigne.

"Celle de Darisse (557 ans avant J.—C.) a été observée en Perse; on l'aurait vue dans la régence de Tripoli, sans le ralentissement.

"Enfin, celle d'Agathocle (310 ans avant J.—C.), signalée près de Syracuse, aurait dû se montrer près de Cadix."[1]

STRIDULATING ORGANS IN SCORPIONS

AT the September meeting of the London Entomological Society, Mr. J. Wood-Mason announced the discovery of tridulating organs in scorpions. While recently working at the anatomy of a species allied to *S. afer*, he had met with structures which, from his familiarity with the analogous ones in other arthropods, crustaceans as well as insects, he had at once without hesitation determined to be sound-producing apparatus—even before he had found that sounds could be produced by them artificially by rubbing the parts together or accidentally in the mere handling of alcoholic specimens. He had, however, been enabled to place the matter beyond all doubt, for while at Bombay waiting for the steamer, he had obtained, by a happy chance, from some Hindustani conjurors, two large living scorpions belonging to another species of the same type; these, when fixed face to face on a light metal table and goaded into fury, at once commenced to beat the air with their palps and simultaneously to emit sounds which were most distinctly audible not only to himself, but also to the bystanders, above the clatter made by the animals in their efforts to get free, and which resembled the noise produced by continuously scraping a piece of silk woven fabric, or, better still, a stiff tooth-brush with one's finger-nails. The species—a gigantic one from the Upper Godaveri district—in which he had first observed stridulating organs, had these organs more highly developed than in the one experimented upon at Bombay, and must stridulate far more loudly, for by artificially rubbing the parts together in a dead alcoholic specimen he could produce a sound almost as loud as, and very closely similar to, that made by briskly and continuously drawing the tip of the index-finger backwards and forwards in a direction transverse to its coarse ridges, over the ends of the teeth of a very fine-toothed comb. The apparatus, which, as in the *Mygale*, is developed on each side of the body, was situated—the *scraper* upon the flat outer face of the basal joint of the palp-fingers; the *rasp* on the equally flat and produced inner face of the corresponding joint of the first pair of legs. On separating these appendages from one another, a slightly raised and well-defined large oval area of lighter coloration than the surrounding chitine was to be seen at the very base of the basal joint of each; these areæ constituted respectively the *scraper* and the *rasp*; the former was tolerably thickly but regularly beset with stout, conical, sharp spinules curved like a tiger's canine, only more towards the points, some of which terminate in a long limp hair; the latter crowdedly studded with minute tubercles shaped like the tops of mushrooms. He had met with no stridulating organs in this position in any scorpions besides *S. afer* and its allies; but in searching for them in other groups he had come to the conclusion that the very peculiar armature of the trenchant edges of the palp-fingers in all the *Androctonoidæ*, and in some at any rate of the *Pandinoidæ* (no *Telegonoidæ* or *Vejovoidæ* had yet been examined), was nothing but a modification for the same purpose, for the movable finger of this pair of appendages when in the closest relation of apposition to its immovable fellow could most easily be made to grate upon it from side to side so as to produce a most distinct crepitating sound; but when separated from it ever so little appeared to be incapable of the slightest lateral movement. It was his intention on his return to India to endeavour to determine this question, as well as many others relative to the species in which the presence of sound-producing apparatus had now been demonstrated by careful observation and experiment upon living animals.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Mr. Thomas Whittaker, from the Royal College of Science, Dublin, has been elected to a Natural Science Scholarship at Exeter College.

At Jesus College the following elections to Welsh scholarships have been made:—In mathematics, Mr. David Davies, from the College, Llandovery; in science, Mr. William Williams, from Dolgelly Grammar School.

The Commissioners commenced their sittings at the Clarendon Hotel on Monday. The proceedings of the Commissioners were of a formal character, but Tuesday, it was understood, they would proceed to take evidence.

CAMBRIDGE.—The master and seniors of Gonville and Caius College will proceed in December to elect a prælector in chemistry, in succession to the late Mr. Richard Apjohn. The duties of the prælector will be to take charge of the college laboratory, to prosecute original research, to instruct in chemistry the members of the college, and such members of the university as the master and seniors may from time to time direct.

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[1] "Cadix," pp. 18, 19.

LONDON.—The London Ladies' Educational Association opened its tenth session at University College for day lectures on Monday. Most of the evening lectures—intended chiefly for governesses and candidates for public examinations open to women—began a fortnight ago. In the past session of 1876–77 there was a decrease in the number of students as compared with the preceding session, in which the number had been greatly in excess of any previous year. There was, however, but a very slight diminution last year in the total amount of fees received, which rose considerably above the former level, the students, as a rule, showing a desire to avail themselves of a regular course of study by attending a larger number of classes. Moreover, the number presenting themselves for examination showed a very marked increase. The range of subjects offered to ladies in the coming session is fully as wide as in any preceding one, and comprises the language and literature of England, France, Germany, and Italy; Greek and mathematics, in elementary and advanced classes; physiology and hygiene; physics; English history, in two classes, intended as a preparation for the Cambridge higher local examinations for women; English Constitutional History; and history of Grecian literature and art; to which will be added, next term, an additional course of Modern History, and a course of lectures on Architecture. Besides these classes, which are for ladies only, ladies are admitted as regular students to the following classes in the college:—Anglo-Saxon, Higher Senior Mathematics, Philosophy of Mind and Logic, Political Economy, Jurisprudence, and Roman Law. Ladies are also permitted to receive practical instruction in the physical laboratory of the college, whilst the Fine Art Department has from its commencement always been open to them.

MANCHESTER.—The Dalton Chemical Scholarship has been awarded to Mr. C. F. Cross. Mr. Cross presented an original investigation upon "Normal primary heptyl alcohol, and its derivatives."

The Dalton Mathematical Scholarship has been awarded to Mr. E. T. Littlewood.

Prof. Boyd Dawkins, M.A., F.R.S., began on the 22nd inst. a course of six museum lectures on "Man's place in the tertiary period." These lectures are open to the public at a small nominal fee.

The session of the evening classes was opened on the 15th inst. by an address on "The Great Masters since Handel and Bach, with especial reference to the form of their compositions." The address was delivered by Mr. Hecht, Lecturer on Music, and was illustrated by selections from the masters' works, performed on the pianoforte.

LEEDS.—On Tuesday the foundation stone of the new Yorkshire College was laid by the Archbishop of York on the site of the Beechgrove estate, at Little Woodhouse, near Leeds, in the presence of a large gathering of friends and subscribers to the undertaking. Dr. Heaton, the chairman to the council of the College, delivered a short statement in the nature of a history of the college from its initiation and its establishment down to the present time. With these details our readers are already familiar. The cost of the site has been 13,000*l.*

BRISTOL.—On Saturday next, October 27, the Dean of Westminster will give a public address to the students of University College, and those interested in its success. The introductory lectures have been duly delivered, and in most cases attracted large audiences, and were well reported in local papers.

ABERDEEN.—*Apropos* of a recent correspondence in the *Times*, the following fact is of some interest:—

At a recent meeting of the Aberdeen University Court a letter was read from Sir Louis Mallet, Secretary to Lord Salisbury, asking what special provision the University would make for superintending the conduct of students selected for the Indian Civil Service during their two years of probation. In reply, the court adopted a motion, in which they resolved respectfully to inform the Secretary of State for India that the University could not undertake to institute any separate or severer system of oversight or discipline for one class of students than for another, and that the present system had been found in practice to be perfectly effectual in securing the steadiness, moral training, and good behaviour of the students.

A memorial from the University Council, asking the court to take steps to institute evening lectureships in science and art, was referred to the Senatus for a report.

DUBLIN.—Prof. Emerson Reynolds will commence a course of lectures on General and Medical Chemistry on every Tuesday, Thursday, and Saturday from November 1 to March 31 following. The first course of Practical Chemistry will comprise laboratory instruction in Qualitative Analysis (including Spectrum Analysis), commencing in Michaelmas Term; Volumetric and Simple Gravimetric Analysis, commencing with Hilary Term; Organic Preparations and Analysis, commencing with Trinity Term. The second, or advanced, course of Practical Chemistry will comprise instruction in the higher branches of Experimental and Analytical Chemistry, and in Methods of Research.

Prof. Macalister, M.D., will commence a course of lectures on Zoology in November, to be continued through each term until the end of June.

Prof. E. P. Wright, M.D., will commence a course of lectures on the Morphology of the Cells and Tissues of Plants, and one on the Natural History of Algæ and Fungi in November.

CORK.—Prof. J. Reay Greene has resigned the Professorship of Zoology and Botany in the Queen's College, Cork, retiring on a pension. There is, however, no vacancy, as Prof. Harkness will lecture the students on these subjects.

GALWAY.—Prof. Cleland, M.D., F.R.S., has resigned the Professorship of Anatomy in the Queen's College, Galway, having been elected to the valuable Professorship of Anatomy in Glasgow College, vacant by the resignation of Prof. Allen Thomson. The vacancy in the Queen's College, Galway, will be filled up by H.E. the Lord-Lieutenant of Ireland on October 27.

LANCASTER.—A very fine set of new buildings for the Lancaster Royal Grammar School was opened on September 24. The buildings include a well-fitted laboratory, erected at the expense of W. Bradshaw, Esq., placed at some distance from the main building. It is a pleasant, well-lighted room, thirty feet by twenty feet. The whole school is taught physics, and every boy will pass through a course of chemistry at about the fourth form stage. We hope the authorities will feel encouraged soon to introduce other branches of science as a regular part of the curriculum.

AMSTERDAM.—A university has been opened at Amsterdam.

SCIENTIFIC SERIALS

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. x. fasc. xv.—New contribution to the efficacy of the elastic ligature in surgery, by M. Scarenzio.—Gleanings in the Zoological Museum of Pavia, by M. Pavesi.—Note by M. Curioni on the contents of his work, "Applied Geology of the Lombardy Province."—On microphytes, which produce certain diseases in plants, by M. Cattaneo.—Physico-chemical researches on the different allotropic states of hydrogen, by M. Tommasi.

Journal de Physique, September, 1877.—From this number we note the following papers: On the application of a new apparatus for the determination of visual astigmatism, by M. Javal.—On the spectrum of the electric spark in a compressed gas, by A. Cazin. The author made two series of experiments, one in which he simply observed the spectrum directly by means of the spectroscope, and the other in which he photographed the spectra and thus obtained more accurate results.—Experimental determination of the principal elements of an optical system, by A. Cornu.—On the currents produced by a liquid passing through a tube, by E. Edlund.—On the spectra of chemical compounds, by P. Moser.—On the modes of crystallisation of water and the causes of the various aspects of ice, by Raoul Pictet.—On the influence of light on the electrical resistance of metals, by R. Boernstein.

Zeitschrift für wissenschaftliche Zoologie, vol. 29, part 2 (July).—H. Reichenbach, on the early development of the fresh-water crayfish, 75 pp. 3 plates.—H. Ludwig, on Rhopalodina (class echinodermata).—O. Bütschli, on the process of division of cartilage-cells; on the development of *Paludina vivipara*, in relation to Bobretzky's and Lankester's recent papers; on the development of *Neritina fluviatilis*, and on the segmentation process and formation of the blastoderm in *Nephelis vulgaris*.

Part 3 (September).—Prof. A. Wrzesniowski (Warsaw), Contributions to the natural history of the infusoria, 57 pages, 3 plates, containing descriptions of many new species, and discussions on *Oxytricha*, *Epistylis flavicans*, *Ophrydium versatile*, &c.—Marie von Chauvin, on the power of adaptation of the larvæ of *Salamandra atra*.—Ernst Zeller, on the reproduction of opalina (parasitic on batrachians), 2 plates.—W. Kurz, studies on the lernæopods, 3 plates.

SOCIETIES AND ACADEMIES

London

Entomological Society, October 3.—Prof. J. O. Westwood, M.A., F.L.S., president, in the chair.—Mr. W. L. Distant exhibited a specimen of the ravages of *Dermestes vulpinus* in a cargo of dried hides from China. On the arrival of the cargo in this country it was found to be swarming with the insect in all stages.—Mr. McLachlan also exhibited a piece of wood which had formed part of a case containing hides from Shanghai and which was riddled with borings of the larvæ of the same insect. The president remarked that his attention had been directed some years ago to the depredations of this larva in a cargo of cork.—Prof. Westwood exhibited a drawing of the pupa of a species of *Anabolia* which swam about in water like a *Notonecta* and was remarkable for using its middle legs as swimming apparatus. Prof. Westwood also made remarks upon the homology of the mouth organs in the pupæ of Trichoptera and suggested that the mandibles of the pupæ (which are aborted in imago) are for the purpose of eating their way out of the cases in which they undergo their transformation.—The president next exhibited a small lepidopterous insect from Lake Nyassa with a pupa case of a species of *Tachina* from which it was supposed to have been bred.—Prof. Westwood next called the attention of the Society to the remarkable lepidopterous larva attached to the homopterous larva which had been handed to him by Mr. Wood-Mason at the last meeting and stated his belief that the relation of the Lepidopteron to the Homopteron was probably one of true parasitism, the former (*Epipyrops*) feeding on the wax secreted by the latter. Mr. Wood-Mason stated that he was inclined to consider the *Epipyrops* larva as a messmate of the Homopteron having attached itself to the latter for the sake of being carried about to its food-plant and having covered itself with the waxy secretion for the purpose of rendering itself inconspicuous to its foes.—Prof. Westwood then exhibited a moth from Brazil which had been bred from a caterpillar found among the hairs of some animal.—The president finally read a note from Albert Müller announcing the formation of an entomological station at Basle.—Mr. Meldola announced that the Longicorn beetle received from Birkenhead and exhibited at the last meeting had been identified by Mr. C. O. Waterhouse as *Monohammus titillatus*, Fab., a species inhabiting the United States. Mr. Meldola also exhibited a collection of Lepidoptera formed by him in 1875 in Ceylon and the Nicobar Islands.—Mr. H. Goss exhibited a series of *Lycæna Arion* taken in the Cotswolds which were remarkable on account of the small size of some of the specimens, about one-third being below the average size.—Mr. McLachlan read a paper on *Notiothauma Reedi*, a remarkable new genus and species of Neuroptera from Chili pertaining to the family *Panorpidæ*.—A paper was communicated by Mr. A. G. Butler on the Lepidoptera of the family *Lithosiidæ* in the collection of the British Museum.

Wellington

Philosophical Society, July 21.—Mr. W. T. L. Travers, F.L.S., president, in the chair.—The hon. Mr. Mantell read a paper by Mr. J. C. Crawford, F.G.S., on gold found in the rocks of the Tararua and Rimutaka ranges in the province of Wellington. Mr. Crawford had forwarded a specimen to Melbourne, and Mr. J. Chapman, the assayer to the bank of Victoria, had reported that the specimen was composed of sulphate of iron, and gold at the rate of 1 oz. per ton. The hon. Mr. Mantell said he would like some explanation regarding the presence of sulphate of iron. Dr. Hector stated that there must have been some mistake, probably iron bi-sulphide was meant. He reminded the society that a great deal had been done in prospecting the country referred to by Mr. Crawford, and that in 1869 he (Dr. Hector) had communicated to the society the results obtained. Eighteen analyses had been made of quartz specimens from reefs in the district; of these only six had proved auriferous, varying from mere traces up to 13 dwts. per ton of gold, the richest being from Wainuiomata, the same locality from which Mr. Crawford's specimen had come. In his former communication he had warned prospectors against the solid quartz reefs which traverse the sandstones and slate, as the gold at Makara and Terawiti appears to occur in jointed sandstones, chiefly as dendritic films.—Capt. Edwin, R.N., communicated a notice by Mr. J. F. Marten, of Russell, Bay of Islands, regarding the occurrence of the tidal wave which took place on May 11 last. Mr. Travers said he had observed this occurrence in Wellington harbour, and that he believed Dr. Hector had taken observations of the rise and fall. Some years ago a similar wave was observed in New Zealand, after which we had news of an earthquake in America, and no doubt the wave on May 11 last was due to a like cause.—Dr. Hector reported that tidal disturbance on May 11 had been observed on every part of the New Zealand coast, and also in Australia in the same manner, but not so intensely as the waves of August, 1868. The origin of the waves on that occasion was clearly traced to a great volcanic disturbance near the west coast of South America, and in this instance a violent convulsion has also been reported from that quarter as having occurred on May 10. We have not the full particulars yet, but if this date is correct the wave felt on our coast must have been due to a still earlier shock, perhaps in some other place, as it was first noticed at 5 A.M. on the 11th, corresponding to 1 P.M. of the 10th on the South American coast. From this date must be subtracted about seventeen hours for the time of transmission of the wave across the Pacific Ocean, which would require that the shock should have taken place about 8 A.M. on the 9th. This tends to confirm the belief that there is a periodicity in earthquakes, and that they occur independently at distant localities at nearly the same time. He observed that a writer in the last received number of NATURE notices this coincidence in reporting a sharp earthquake at Comrie, in Scotland, on May 11. At Napier, where the engineer of the harbour works, Mr. Weber, makes exact observations, the tides were disturbed from the 11th to the 19th. The position of Napier renders it peculiarly sensitive to oceanic oscillations. Thus on May 1 the highest sea ever experienced in Napier washed over the shingle spit and damaged the rails in front of the Court-house. This phenomenon was only local, and attributed to a long continuance of south-east wind. He called attention to a recent paper by Mr. Russell, the Government Astronomer at Sydney (*Journ.* Ast. Soc. N.S.W., 1876, p. 37), which states that the slightest earth shocks felt in New Zealand are nearly always recorded on the tide gauges in Sydney and Newcastle, and are most unaccountably coincident with abnormal readings of one of the thermometers in the Observatory. If we had well-placed tide-gauges on the New Zealand coasts it is probable the most interesting results would be obtained. Every addition to the observed facts bearing on this subject would be valuable. The investigation of earthquakes would be similar to that of the influence of sun-spots recently examined by Prof. Balfour Stewart, in so far that the release of prodigious latent energies might depend on very obscure and trivial exciting causes. Mr. Carruthers said he did not consider it necessary to suppose that seventeen hours must elapse before a tidal wave due to the same cause as the South American earthquake would reach New Zealand. He did not think the earthquake caused wave, but that both were due to the same cause. He thought earthquakes were locally intensified exhibitions of a great deep-seated movement of the floor of the ocean, and that if the floor were not in movement an earthquake, however violent, would be unable to propagate a wave for such distances as from America to New Zealand. The intensified action which so often shows itself in this part of South America he thought was due largely to the great bend made in the line of elevation of the Andes at this point, which had the effect of converting a deep-seated movement of the earth's crust into a violent crushing of the surface. Dr. Hector explained that the period of seventeen hours for the transmission of a wave across the Pacific Ocean was derived from observation in 1868, when the commotion of the sea extended not only to New Zealand and Australia, but to Japan, Sandwich Islands, and the Cape of Good Hope. He agreed that earthquakes were widespread phenomena locally intensified, but it is the strong local convulsion that originates the oceanic waves. Such waves could not keep pace with a tremor propagated through the solid floor of the ocean, which travels at six times greater speed and generates what is termed the *forced wave*. The ocean wave once generated would take its own time. Dr. Newman did not think we had yet sufficient data to decide on the subject. The depth of the ocean should be considered. He could not agree with Mr. Carruthers that earthquakes extended over so large an extent of the ocean bed. The president said that the works of Darwin, Humboldt, and Mallett on this subject, would be found interesting. He agreed with Dr. Hector that we must look outside our globe for the prime causes of such disturbances, such as sun-spots, influence of the moon, &c.—Before the close of the meeting, Dr. Hector drew attention to several exhibits on the table, more especially to an albino of the New Zealand crow (*Glaucopis wilsoni*), and to a Tui (*Prosthemadera tui*, Zel.) with brown plumage. A fine series of crustacea from the Californian coast, and a selection of the more interesting fossils obtained during the past year by the Geological Department, were also exhibited and explained.

PARIS

Academy of Sciences, October 15.—M. Peligot in the chair.—The following papers were read:—On some applications of elliptic functions, by M. Hermite.—On the movements of the apsides of the satellites of Saturn and on the determination of the mass of the ring, by M. Tisserand.—On the non-transparence of incandescent iron and platinum, by M. Govi. He denies the assertion that iron raised to a red or white heat becomes transparent.—Employment of lime-water to fix fatty acids of feed-water of boilers in engines provided with surface-condensers, by M. Hetel. The fatty matters become fixed in insoluble combinations, so that the water reaching the boiler is neutral or even slightly alkaline, containing only a calcareous soap and free glycerine, which is inoffensive and non-adherent to the boiler.—On the ravages produced in the vines of Narbonnais by the disease of anthracnose, by M. Porte.—On the employment of colza and rape, sown in vineyards to preserve the vine from frost, by MM. Serrès and Rérat. These seeds are sown in October [or] November, and by May, when the frosts are most to be feared, the plants have grown to more than a metre in height, giving good protection. When the frost is fairly gone the rape or colza is cut and the vines then grow with more vigour.—Reply to a former note by M. Stephan on the discovery of the planet 174, by M. Watson.—On cases of reduction of Abelian functions to elliptic functions, by M. Hermite.—Formation of allylene at the expense of bromocitrapyrotartaric anhydride, by M. Bourgoin.—On dibromomethylcarbylamine, by M. Tcherniak.—Researches on the physical constitution of the blood corpuscle, by M. Bechamp. He has succeeded in demonstrating the separate existence of an enveloping membrane by nourishing the corpuscles in a solution of fecula. The membrane is thus made both more resistant to the action of water, and more visible, while retaining its osmotic properties. The effects obtained in thus treating the blood of duck, &c., are minutely described.—On the organic *débris* contained in the quartz and silex of Roannais, by M. Renault.—Researches on vegetable glycogenesis, by M. Jodin. *Inter alia*, the constant presence of certain sugars in all champignons, proves the independence of the glycogenic function, and the chlorophyllian function. Researches are desirable on the influences which cause the saccharimetric quantity in leaves to vary, and the nature of the relation between these variations and the exercise of the chlorophyllian function, &c.—Researches on fatty bodies introduced fraudulently into butter, by M. Husson.—Relation between barometric variations and the sun's declination, by M. Poëy. Low pressures follow exactly the course of the sun, while high pressures follow an opposite course. The observations were made at the Observatory of Havana.

GENEVA

Society of Physics and Natural History, July 5.—M. Loret continued the account of his researches on the polarisation of quartz, carried on in conjunction with M. Ed. Sarasin. A first series of measurements of the solar light with a spectroscope having a fluorescent eye-piece, had been extended as far as the Fraunhofer line R. In the new series, and in order to carry further their observations on the ultra-violet part of the spectrum, they operated upon the light proceeding from a strong induction discharge issuing between two points of cadmium. They determined the rotatory power of the quartz for eight of the principal lines of the ultra-violet spectrum of that metal, for which M. Mascart has given the wave-lengths, and for two lines situated still farther beyond. Their experiments show that in these limits, much more extended, the law of Boltzmann is confirmed in a very satisfactory manner. It appears, however, necessary to add a third term to his formula.—Prof. Schift communicated the results of his experiments on the contractions presented by the diaphragm after its nerves have been cut.—M. H. Fol described observations made by him on the origin of the follicle which surrounds the egg of ascidians. It has been wrongly supposed that the cellules of the follicles form part of the stroma of the ovary. The cellules originate in the interior of the young eggs, at the surface of the germinative vesicle, and traverse the whole thickness of the vitellus to reach the surface of the ovule and detach themselves from it. This example of so singular a mode of formation of the follicular cellules is unique, so far as known, in the animal kingdom.

August 2.—Major Ed. Pictet presented the hydrographic chart of the Lake of Geneva, in its south-west part, from its line Coppet-Hermamé to the exit of the Rhône. The form of the bed of the lake is defined by equidistant curves of five metres, vertical distance. The work will be published as the first development of an old investigation by Sir H. de la Beche.—Prof. Colladon has investigated new cases of lightning-stroke, which has confirmed his former conclusions on the effects of lightning upon trees. One of the determining causes of a stroke of lightning on a tree is the neighbourhood of a spring or of a subterranean sheet of water. That cause acts more powerfully than the relative height of different tides. He shows that most of the metallic wires used as lightning conductors are too slender; he calculates that they ought to have a section of at least 500 square millimetres.

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